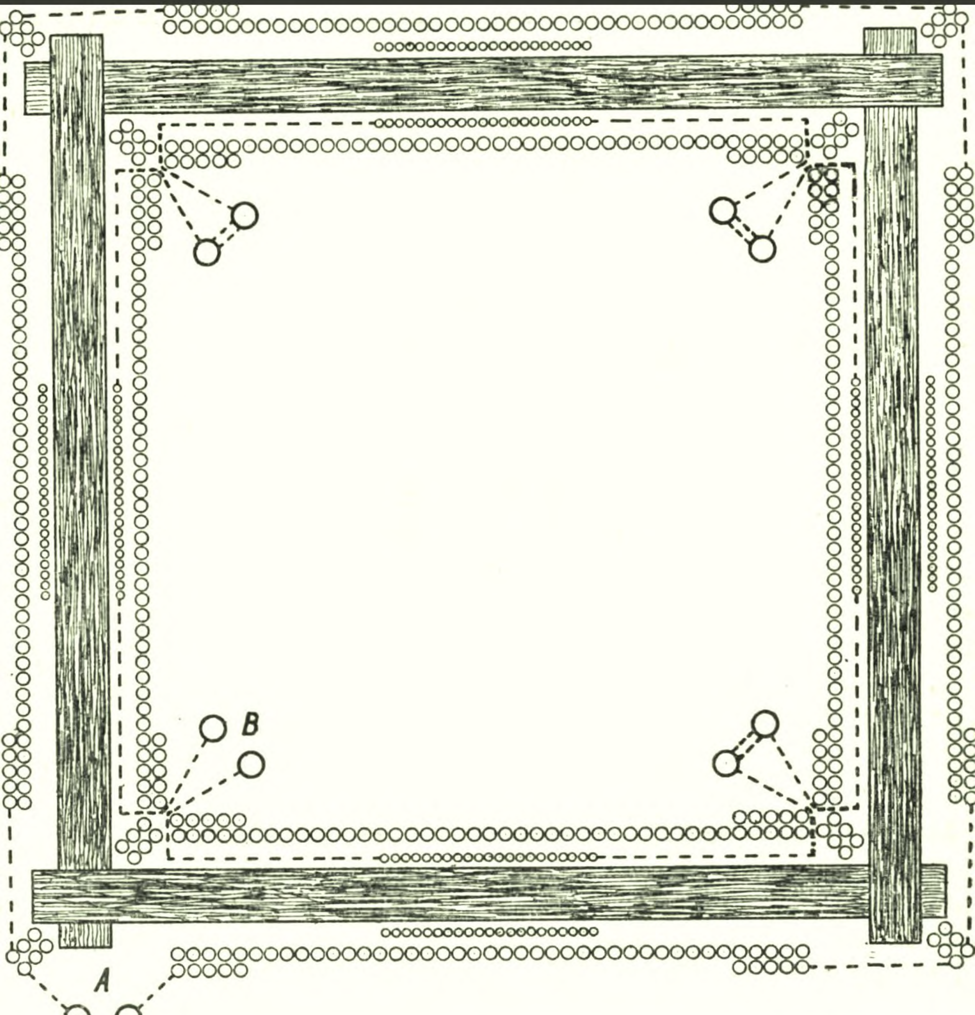

Proceedings of the Institution of Electrical Engineers

Institution of Electrical Engineers, Society of Telegraph Engineers, Society of Telegraph ...

JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

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VOL. 34.

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OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 34. 1904. NO. 170.

Proceedings of the Four Hundred and Twelfth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 10, 1904—Mr. R. KAYE GRAY, President, in the chair.

The Minutes of the Special General Meeting held on June 9, 1904, were read and confirmed, and those of the Annual General Meeting held on the same date were taken as read, and confirmed.

The list of candidates for election into the Institution was read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

Arthur Albert Day.
Frank Wallis.

From the class of Associates to that of Members—

James Cecil Shields.

From the class of Associates to that of Associate Members—

Charles Edward Stewart Bill.
James Richard Craddock.
Bernard Booth Granger.
John Percival Vissing Madsen.
Samuel C. Rhodes.
John Senior.
Charles Henry Shanan.
William Greenell Wallace.
Alfred Tufnell Yates.

From the class of Students to that of Associate Members :—

Herbert Ashlin Skelton.

From the class of Students to that of Associates :—

Selborne Evans.
William A. Fitzgerald.
Henry Montagu Lyons.
Harding Beebee Simons.
George Edward Smith.

Donations to the *Library* were announced as having been received since the last meeting from The Admiralty, Mdlle. André, Chateau de Roth, Luxemburg (per H. W. Miller), E. Anthony, Dr. H. Borns, C. Bright, Cassell & Co., C. Chevrier, Gauthier-Villars, R. T. Glazebrook, R. Kaye Gray, Griffin & Co., Indian Telegraph Dept., A. T. M. Johnson, Major-General B. Lovett, W. P. Maycock, Methuen & Co., C. Naud, G. Newnes, Ltd., Maschinenfabrik Oerlikon, T. Parker, Patent Office Library, R. E. Peake, Philadelphia Electrical Bureau, Rentell & Co., R. T. Rohde, Scientific Publishing Co., G. Semenza, H. Somach, J. Swinburne, Whittaker & Co.; to the *Building Fund* from W. W. Cook, S. Evershed, F. T. Fawcett, R. Livingstone, A. P. Patey, E. R. Rudge, D. E. Wardlaw, F. H. Webb, L. Wood; and to the *Benevolent Fund* from C. W. Barnes, L. Birks, C. G. Cadman, V. K. Cornish, F. Edmondson, Lieut.-Col. H. S. Hassard, J. P. Lawrence, M. C. Olsson, C. Silver, G. G. Tomkins, and F. H. Webb, to all of whom the thanks of the meeting were duly accorded.

Mr. R. KAYE GRAY presented the Premiums referred to in the Annual Report of Council for the year 1904.

Mr. Gray then vacated the Presidential chair, which was taken by Mr. Alexander Siemens, the new President.

The President.

The PRESIDENT (Mr. Alexander Siemens): Gentlemen, my first duty is to ask Sir William Preece to move a resolution.

Sir William Preece.

Sir WILLIAM PREECE: Gentlemen, whether the duty of retiring from the chair is pleasant or unpleasant, it certainly is pleasant to the senior Past-President of this Institution present to-night to propose a resolution of thanks to the junior Past-President for his services during the past year. I do not know whether my accidental appearance here is due to the magnetic influence of the name of Gray or not; but I can say this, that after an acquaintance of quite thirty years or more I have found Robert Gray the same Robert Gray that I knew when a boy. Throughout the whole of his life he has carried his character on his face. There is nobody who has ever been brought into company with Robert Gray who has not felt the magnetic influence and the geniality of the man. I have known him perform many duties of great responsibility and anxiety, and some which have been very arduous, but I always found him going through those duties with the same smile and the same genial expression on his face. There is one thing which I think everybody who has ever been brought into contact with him will acknowledge, and that is the possession of great tact in dealing with others, and I know nobody who surpasses Robert Gray in that respect. If any disturbance has taken place in

the Institution, or if he has been in the world where matters of business are conducted, his tact has always carried the business through, with credit to himself and joy to everybody else. And now I have to speak as to his duties in this chair. I cannot say much of them, for the very simple reason that I have not been able to be present during his twenty months of service. Unfortunately, last year I was laid low with a very serious illness, and I have spent a twelvemonth practically away and out of business, but I am delighted to think that on my first reappearance at this Institution I have such a pleasant duty thrust upon me. One other point connected with Mr. Gray, apart altogether from his tact in the chair, has been his munificence out of the chair. There is nobody on the face of the earth who values money more than a Scotchman; but I never knew a Scotchman who held money in such small regard as Robert Gray. Gentlemen, there has been an extremely interesting visit paid recently to America, and that visit to America has been conducive to the reputation of the British electrical engineer. That is due to the way in which our little army was governed and controlled and manipulated by our Past-President. Something will be said of this, I have no doubt, during the coming session, but I can only say that I have the unanimous opinion of everybody that I have seen who has returned from St. Louis, that the business and duty and dignity of the office of President of this Institution could not have been discharged better than by Mr. Robert Gray. Gentlemen, I beg to propose: "That the best thanks of the members of the Institution of Electrical Engineers be given to Mr. Robert Kaye Gray for the very able manner in which he has filled the office of President during the twenty months, 1903–1904, and for the indefatigable personal attention that he has given to the duties of the office." Sir William Preece.

Professor SILVANUS P. THOMPSON: Mr. President and gentlemen, it is indeed a pleasure to second the vote of thanks that has been so well proposed by Sir William Preece, for I feel perfectly sure that there is not a single member of this Institution, whether present here to-night or elsewhere, who would not in his heart echo Sir William Preece's words and say that we are indebted to Mr. Gray for the service that he has rendered to us during these twenty months, an unusually long Presidency. We may remember that it began a little before the ordinary time, when Mr. Swinburne vacated the Presidency in order that Mr. Gray might preside during the visit to England of the International Meeting of Telegraph Engineers in June, more than a year ago. We all remember how splendidly our Institution, in the person of Mr. Gray, received the International delegates of that Telegraph Congress, and gave them that splendid concert in the Albert Hall, the expense of which cannot have fallen lightly upon the shoulders of our late President. Then he not only received the international delegates on that occasion, but he served as our figurehead during the trip that some of us had the privilege of making round the electric installations in the north of Italy. Those of us who had the opportunity of accompanying the President on that occasion must indeed have recognised, in the way in which he moved among our Italian friends, how admirably fitted he was to represent, by his Professor Silvanus Thompson.

Professor Silvanus Thompson.

dignity, his courtesy, and his amiability, all that is best in an English gentleman. Then there came his year of office, when he filled the chair at our meetings. It is one of those things which we have to look back upon with very mixed feelings, that in the middle of the Session death suddenly took from us our late lamented Secretary, Mr. McMillan. What we should have done during the interregnum before a successor could be appointed no one knows, if it had not been that our President, busy man though he is, with important commercial cares heavily upon his shoulders, devoted himself incessantly, coming down day after day, afternoon after afternoon, making himself familiar down to the very last point with the actual business of the Institution, and carrying it through just as if he might have been its paid Secretary. His devotion to the services of the Institution in that emergency can never be exaggerated, they are indeed very great. And lately we have had Mr. Gray piloting a party round the United States, in that excursion and that visit and that Congress of which Sir William Preece has spoken. We indeed owe to our Past-President an unusual debt. His presidency has been an unusual one, marked by a number of unique events. I trust that we shall ever cherish in our hearts the recollection of that memorable year of office when Robert Kaye Gray was President of the Institution.

The President.

The PRESIDENT: Gentlemen, I need not put the vote formally again; you have already expressed your feelings. I have very great pleasure, Mr. Gray, in conveying to you the thanks of the Institution for the very great devotion you have given to its interests during your term of office, and as your immediate successor I regret very much that you have given an example which it will be hopeless to attempt to follow.

Mr. Kaye Gray.

Mr. R. KAYE GRAY (Past-President): Mr. President, Sir William Preece, Dr. Thompson, and gentlemen, I am sure any man in the position in which I now am must feel only too gratified at the extremely kind manner in which have been conveyed the thanks of the Institution for what he has been able to do during his period of office. I can assure you that, with the help of a very willing Council, each member of which has been always ready to undertake any task and to do anything that was asked of him provided that it was reasonable, any work I have had to do has been a source of pleasure to me. A retiring president must admit that there are moments of anxiety during his service, but the kindly feeling and forbearance of his colleagues on the Council, and the assistance of the permanent officials of the Institution, renders the duty of a president less onerous than it would otherwise be. This has been my happy experience. Gentlemen, I cannot say more, except to thank most sincerely Sir William Preece and Dr. Thompson for the exceedingly kind manner in which they have referred to what has occurred during my term of office as your president, and to offer to you my gratitude for the equally kind manner in which you have received the resolution.

The PRESIDENT then delivered his inaugural address :—

INAUGURAL ADDRESS.

By ALEXANDER SIEMENS, President.

(Delivered November 10th, 1904.)

In taking the chair for the coming year, I am placed at a disadvantage compared with my predecessors, in so far as I cannot begin my address by thanking you for having elected me, but have to explain how it has come about that I occupy this honoured position.

The lamented death of our late Secretary induced the Council to consider how a continuous policy in the management of the affairs of the Institution could be best insured; and as a result certain alterations in the bye-laws have been suggested by the Council, and have been sanctioned by you in due course. This revision was, however, not formally completed in time to propose a President-Elect at our last annual General Meeting, and the expedient was adopted of proposing Mr. Robert K. Gray for a second term of office, to which he agreed, on condition that he might resign in favour of a President-Elect as soon as the alteration of the constitution had been duly carried out. As our new Secretary had been only a short time in office, the Council decided to elect a Past-President as the successor of Mr. Gray, and did me the honour to select me, when Mr. Gray formally intimated that he did not wish to serve a second time as President.

During the recess Mr. Gray, with a numerous party, went to the United States, as you all know, and during his absence it has been my sad duty to represent you at the funeral of our honorary member and Past-President, Major-General Webber, who has the further claim on the regard of the Institution, that he was the last surviving founder.

Members of Council know better than the other members of the Institution how indefatigable General Webber was in promoting the interests of the Institution in every possible way, not only by attending meetings of Council and of Committees, but by suggesting improvements wherever he thought imperfections existed. He notably took an active part in all the negotiations connected with the acquisition of a suitable site for an Institution Building, about which the Council reported at the last meeting. In him we have lost a true friend of the Institution, and his memory will be held in honour as long as the early days of our Institution are remembered.

It would perhaps appear natural to take this opportunity of comparing the status of the Institution, or rather of the Society of Telegraph Engineers, at its inception, with the position now occupied, but this has been done so recently in Presidential Addresses that my words would be only a superfluous addition to the remarks more ably

expressed by my predecessors. On the other hand, the causes which influence the growth of industries, and thereby the rise of institutions connected with them, will bear further discussion, as they are of the most varied character, and are an apt illustration of the interdependence of all factors of modern civilisation.

Considering the features of this civilisation generally, it will be acknowledged, after a short reflection, that its true foundation is the lowering of the cost of production—in other words, the progress of civilisation is in proportion to the cheapening of articles of consumption. Although this may sound purely materialistic, the process of lowering the cost of production, which implies the employment of all the knowledge we can acquire, and of all the training the best schools can give us, results in greater ease of acquiring the necessaries for keeping alive, and therefore it gives us more time to cultivate other aspects of our existence.

This general principle has manifested itself as much in the electrical industry as elsewhere, and further progress appears to be dependent on further lowering of the cost of its products. When we come to inquire into the means for carrying out this principle we soon recognise that the solution of the problem depends upon a variety of circumstances which all influence the result, although they may be investigated and discussed separately.

For "internal working," if I may say so, every industry depends upon three principal factors :—

1. The capital which provides the works and the raw material.
2. The workman who converts the raw material into the finished product.
3. The management.

It is quite idle to try to establish the relative importance of these three factors, for each one is indispensable, and likewise the prosperity of each one is inseparable from the prosperity of the others; so that no industrial establishment can flourish where these three factors do not co-operate heartily and harmoniously. As a first condition for further progress we should, in fact, recognise the identity of interest between the capitalist, the workmen, and the management, as any measure which is intended to unduly favour any one of these three factors would inevitably increase the cost of the produce beyond what it ought to be and thereby restrict its sale, thus damaging all three.

When we examine, in detail, in what way each of the three factors can contribute to the lowering of the cost of production we find ample confirmation of the intricacy of the problem.

Taking capital first, its share in the value of the finished article is proportionate to :

a. The cost of the depreciation and maintenance of the works and of their outfit.
b. The cost of the raw materials.
c. The profit, representing interest on the total capital invested in the undertaking.

In order to minimise the first item the works ought to be carefully planned and equipped, so as to be capable of turning out the maximum quantity of produce, and their machinery should be selected and laid out in such a way that its wear and tear is only due to its legitimate working, and that the handling of all materials is reduced to a minimum.

The second item, the cost of raw materials, is too intimately connected with fiscal questions for treatment in this address; but incidentally it should be noticed that practically there is no such thing as "raw material," at least the Royal Commission on Trade declared itself unable to define what is "raw material." It is true that each industry works up what it designates as "raw material," but this in turn is the finished product of some other industry; even the products of nature, whether minerals, plants, or animals, form the basis of great industries, and are more correctly called finished articles than raw materials. In any case for the natural progress of industries the cost of the materials, which they work up, should be kept as low as possible, and every increase of it is a hindrance to their proper development.

There is apparently not much difficulty in diminishing the third item, the profit on the capital invested; but it stands to reason that the interest earned by the capital could not fall below a certain amount without the industry in question losing the support of capitalists and collapsing. The rate of interest may therefore be considered a fixed quantity; and in order to diminish the amount payable under this head, it is important to avoid burdening an undertaking with more capital than is required for setting it up as a going concern and for providing a reasonable amount of working capital.

A particularly dangerous way of increasing capital expenditure is buying patents, as the best inventions may be superseded at any time either by better ones or by a change in trade requirements, and in such cases the capital invested in the patent would be quite unproductive, while the interest would still have to be paid. It is much more equitable to remunerate an inventor by a royalty, so that his reward may be in proportion to the success of his invention, and to charge the amount, that may have to be expended in developing the invention, to revenue account.

When considering the influence of the second factor, the workman, on the value of the product, it becomes clear very soon that the amount of work turned out in a given time has a twofold influence on the result, as the time occupied in producing an article not only regulates the amount of the wages but also the proportion of the dead charges to be debited to the article. It is, therefore, quite certain that in order to contribute his share to the lowering of the cost of production of an article the workman has to turn out a greater number of these articles in a given time, and the problem is how to attain this object without endangering the interest of any one of the three principal factors. This sounds almost as if slave labour would prove an economic plan in the carrying on of industries; it can, however, hardly be contended that the interest of the workman

is considered under such conditions, and for this reason alone it should be condemned, even if there were no other objections to it.

There are, however, legitimate ways in which the amount of wages to be charged against an article can be diminished. Most important among them is the substitution of machine work for hand labour, and where this is done unskilled labour should be utilised for running the machines under the supervision of a few skilled artisans.

Here again the interests of the workman appear to suffer, and whenever he is confronted with a new labour-saving machine he airs his old grievance that machines will deprive the working man of his living, just in the same way as he opposed the use of all machinery a hundred years ago. The lesson taught by experience has evidently not made an impression; but nobody can deny that, instead of making skilled labour superfluous, the introduction of machinery has resulted in more skilled workmen being employed, and at better wages than before, and similar developments are likely to follow when the workman is further relieved from hard toil by his best friend the labour-saving machine. Another feature of machine work is the greater accuracy that can be attained, so that, while the cost of production is lowered, the quality of the product is improved, and at the same time the interchangeability of parts is secured, which in itself contributes to the lowering of cost.

It is not easy to convince workmen that "increase of output" is of advantage to them; if we are to accept statements published in the newspapers, they rather believe in the direct opposite, "restriction of output," although their official organs repudiate the idea. This desire to do as little work as possible is, in a way, ingrained in us all, but in the workman it is in addition a reminiscence of the mediæval guilds, those ideal trade unions which have been abolished everywhere as incompatible with modern industries. In their time they have filled a useful purpose, but the conditions which made them appropriate have entirely passed away, and any attempt to revive them or their principles is doomed to failure. According to their constitution nobody was allowed to exercise a trade, or even to sell its products, who had not been brought up to it in accordance with strict regulations, first serving an apprenticeship, then becoming a journeyman, and finally advancing to be a member of the guild if he could produce a satisfactory masterpiece. The number of apprentices taken on was prescribed and the number of masters in each community was limited, so that every craftsman was tolerably certain of constant employment.

No wonder that such a condition of things has a great attraction for the modern workman, especially when he happens to be out of work, and that he thinks he can improve the position of his class by restricting output and by insisting on a minimum rate of wages, after the manner of the old guilds. His reasoning apparently is that modern conditions of living require a number of things to be done every day; consequently the less work each individual does the more individuals have to be employed, and if a minimum rate of wages is enforced the question of the unemployed is satisfactorily solved.

In the Middle Ages, when each community was to a great extent

self-contained, and the intercourse even between neighbouring towns was hampered by bad roads and insecure travelling, it was possible to adopt this solution of the problem, especially as the style of living was very much the same for the great majority of people, while their requirements did not include much beyond their bodily wants. During this time the workmen, when they had become full members of the guild, were practically chained to the soil, and in return for this immobility their number in a community was limited. They had to do all their travelling, if they were that way inclined, while they were journeymen, and most of them used that time for picking up information useful for their trade, even in foreign countries. As the facilities for travel increased, and with it the intercourse between distant countries, the prices of products fell which formerly had been obtainable only by the very rich, and gradually the great majority of people became accustomed to surround their daily life with luxuries, meaning thereby everything that is not absolutely required to keep body and soul together. This alteration in the way of living has been further accentuated and extended by the substitution of machine work for manual labour; in fact this change of the mode of production is still going on to the great advantage of the consumer: in other words, everybody is sharing in the benefit of the cost of production being lowered.

On the position of workmen the further circumstance has exercised an important influence that, by the facilities of intercourse and by the employment of machinery, huge factories are enabled to undersell works on a small scale, so that for successful competition in modern industries capital has become a necessary adjunct. It is, therefore, no longer possible to insist on masters being selected only from the class of workmen, nor is the work which is absolutely necessary for the daily existence of mankind sufficient to keep all workmen in constant employment, so that the old remedy of the guilds can no longer be applied. Restricting the output has in fact nowadays exactly the opposite effect to that for which it is intended. When the price of an article is raised owing to this supposed remedy, a number of consumers begin to do without it, and the demand for it diminishes, so that fewer people can be employed on its production. Instead, therefore, of securing employment for more men, it will endanger the continuance of work for those who restrict output under the mistaken impression that they are benefiting their fellow-workers.

How far this restriction of output is carried sometimes can be illustrated by the action of the workmen in shipyards and boilershops, who either refuse to work pneumatic riveting machines or insist on doing no more work with the machines than could be done by hand, exacting the same price per rivet as for hand labour. It escapes their attention that they handicap their employers in competition with others who are free to utilise such labour-saving devices to their full extent, and that unsuccessful tendering means no work at all.

On the other hand, a good example of the beneficial effect of lowering the cost of production is furnished by the trade in glow lamps. At first, when they were made by a few highly skilled work-

men and sold at twenty-five shillings each, there were very few consumers able to indulge in the luxury of electric light, and the number of hands employed was correspondingly small. That condition of things has been absolutely changed since the price has been lowered to its present level; thousands of skilled workers are employed in the production of lamps, in addition to the tens of thousands of unskilled people who attend the machinery used in the manufacture and perform the manifold functions connected with this trade.

Such a result could not have been attained without the use of labour-saving machinery of the most varied description, and it seems incredible that, in spite of this and similar demonstrations of the beneficial influence of increased output at lower cost, the workman should remain blindly hostile to this principle and cling to his exploded remedy of restriction of output. This attitude is illustrated by a remark which a leading workman made when the importance was impressed on him of diminishing the amount of wages chargeable to each individual article, because in this way the article became cheaper, more customers could be found to purchase it, and consequently more workmen would be required to produce it. His observation was that he fully understood the argument, and that it meant, in his opinion, when carried out to its full extent, that everybody would find employment if no wages were paid at all. It is rather sad to think that such a man, who has been elected by his fellow-workers to a high office in his trade union, should not be able to discriminate between the rate of wages paid to a workman and the proportion of such wages chargeable to each article produced by that workman.

Workmen have also a rooted objection to seeing machines worked by unskilled men, which is apparently based on the mistaken notion that such unskilled men are replacing skilled workers. They, however, overlook the fact that labour-saving machines are only introduced where the same articles produced by hand labour would be too expensive for sale, consequently no skilled workmen could be employed instead of labour-saving machinery. Another fetish of the working man is his desire that all workers should be treated alike and no preference given, absolutely ignoring that the capable and industrious are more deserving than the lazy and careless workers. How disastrously this tendency sometimes operates I can tell from a personal experience, when I started the first continuous-working tank glass furnace in Leeds about twenty-five years ago. Up to that time all glass for bottle-making was melted in pots and the glass-blowers were never certain when their services would be required, as it was almost impossible to melt the batch in a given time. Again, their work was frequently interrupted if the furnace man was unable to control the heat properly so that the melted glass was either too hot or too cold. In any case the working out of the last portion was very troublesome, as the pipes had to be dipped far into the furnace to reach the bottom of the pots. All these drawbacks are overcome in the tank furnace, where the melted glass is always ready for the blower; its temperature is under perfect control and its level is always the most convenient for the workers.

As it happened the secretary of the trade union worked at the furnace and expressed repeatedly his great satisfaction at this complete solution of all their difficulties. His union consented to the prices of bottles being lowered, but in spite of that the men succeeded in earning higher wages than before, owing to the greatly increased facilities of working. Encouraged by the success, the firm desired to establish three shifts of workers in order to utilise to the full the continuous working of their plant, but the trade union would not give their consent, alleging that the firm were making enough profit working two shifts. The firm then resolved to build some more furnaces of the same kind, but, to everybody's astonishment, the trade union threatened to withdraw all their members if another furnace were built. I asked the secretary for an explanation, and reminded him that he had nothing but praise for the improved system of glass melting. In reply he said that he had nothing to take back, but that it was precisely the very great advantage that men enjoyed who worked at these furnaces which induced his Council to prohibit their further building, as it was not likely that all the Yorkshire employers would follow suit, and they did not wish to have some of their men working under better conditions than others. To this decision the trade union adhered for nearly twenty years, and the consequence has been that hardly any glass bottles are now made in England.

I was reminded of this story when I saw in the newspapers, a short time ago, the bitter complaint of the glass workers that foreign bottles were dumped in this country to the detriment of the native workmen. If these men had understood, twenty-five years ago, the principle that to lower the cost of production is to facilitate and increase the sale of an article, and thereby to benefit the whole community, themselves included, they would not have opposed the introduction of the tank furnaces, and they might also have remembered that competition would have forced all employers very soon to adopt this improved method of glass melting, so that all the members would have worked under the same favourable conditions. Such a glaring case of shortsightedness does, perhaps, not occur very often, but this particular one has done irreparable injury to the manufacture of glass bottles in Yorkshire.

Similar mistakes on the part of the trade unions can only be prevented by enlightening the workmen as to the necessity of lowering the cost of production, so that he uses his best endeavours to increase the output by adopting the latest methods and the best labour-saving machinery, and by convincing him that restricting the output means restricting employment.

However important Capital and Labour are to an industry, the third factor, "Management," is none the less essential for its satisfactory progress. Under this term everything is included which appertains to:

(*a*) The selection of site for works, their planning and their equipment.

(*b*) The commercial work connected with the business.

(*c*) The arrangements for carrying on operations.

(*d*) The designing of new apparatus and the investigation of new methods of manufacture.

It is easy to understand that in every one of these directions good management can do much to lower the cost of production, but this is hardly the time to discuss all these points in detail. Only on the last I venture to make a few remarks, as the proper design is a matter in which the members of this Institution are interested, more particularly those who have only recently graduated from a college. A good design should have for its aim the lowering of the cost by employing simple component parts which can either easily be obtained or cheaply manufactured.

The first requirement is fulfilled by utilising standard sizes, and in this respect the work of the Engineering Standards Committee is of the greatest possible importance to the industries of this country. In addition to these "public standards" much can be done to facilitate work by adhering to standard parts in each factory, so that it is not necessary to carry an immense variety of parts in stock.

A number of graduates do not realise that the object of their working practically in the various shops, in addition to studying at a college, is to teach them by their own experience what the difficulties of the various operations really consist of. It is not so important that they learn to overcome these difficulties as easily and as quickly as a skilled workman, but that they acquire the habit of thinking how to avoid these difficulties when they are designing new work, and how to take advantage of the material at their disposal. In other words, a designer should have an intimate acquaintance with the scientific principles that come into play and with the ways in which they are applied, and he should know, if possible from personal experience, the capabilities and the limitations of the workman and of the machinery by which the operations are carried out. It is, however, by no means sufficient for preparing suitable designs to be well acquainted with the means employed to execute them; it is just as important to form an accurate conception of the problem that has to be solved, and to possess a thorough knowledge of previous attempts in the same direction, and of the causes of their success or failure.

Another branch of knowledge required for the good management of factories is a careful training in the way of conducting experimental investigations, coupled with good judgment as to which investigations are worth taking up. No doubt the list of qualities that ought to be found in a good manager could be extended still further, but enough has been said about all the three principal factors to show the great number of different points which have to be considered in connection with the internal working of modern industries.

Besides these internal factors there are all the external circumstances which influence the well-being of the industries, and it is easy to see, even without entering at length into further details, that no true picture of the state of an industry can be arrived at if only a few features of their complicated constitution are to be taken into consideration.

These remarks about industries in general apply naturally to the young electrical industry quite as much as to the oldest established one; there are, however, some features of its development which have greatly contributed to its rapid rise, and could perhaps be imitated

in other industries to their great advantage. At first the long time during which weak battery currents alone were employed, chiefly for telegraphic purposes, permitted the completion of important scientific investigations which in the end led to the discovery of those fundamental laws which form the basis of all practical applications of electricity. Nor has science deserted the industry when it began to deal with powerful currents. I will only remind you of Dr. John Hopkinson's mathematical demonstration in this room, that alternate-current machines can be worked in parallel.

This example is particularly interesting, as in this case science has pointed out to the engineer how he could solve one of his problems; usually the function of science has been to prevent its followers from being deceived by appearances or by plausible fallacies. By such negative indications alone science can give definite answers, while its deductions of a positive character cannot be accepted without further proof, owing to the uncertainty whether all premises necessary for the correct solution of the problem have been taken into consideration. For this reason deductions arrived at theoretically should be tested by practical applications before further conclusions are based upon them.

Here again the electrical industry has had the great advantage of possessing from its very beginning most sensitive instruments, which in addition are easily handled, so that the verification of theories has been a comparatively easy matter, and it has been possible to control by accurate measurements all operations not only in the laboratory but also in the workshop.

One feature of electrical measurements has contributed more than anything else to the rapid rise of the industry, and that is the adoption of the same units of measurement in all countries. The idea of such a system originated with Gauss and Weber, of Göttingen, about seventy years ago, but the British Association for the Advancement of Science has the honour of having taken the first step to realise their proposals by appointing a committee to suggest practical electrical units. This committee, after due deliberation, chose as the basis of their system the centimetre, the gramme, and the second, and I should like to know why they chose the metrical units, unless they had convinced themselves that these were the best for the purpose.

At that time (1861) there was no question of international units, nor had the metrical system been adopted as universally as it is now; all the more weight ought, therefore, to be attached to the decision of this Committee, which consisted of eminent scientific men and prominent engineers, so that both the theoretical and the practical aspects of the question were kept in view during their deliberations. Certainly there is no doubt about the beneficial effect of the adoption of this system by all countries, and I have no recollection of ever hearing that the decimal division adopted in it has given rise to any difficulties. It is true some modifications have become necessary, which were discussed and accepted at a series of International Congresses called for the purpose, but so far as I am aware nobody has ever suggested that it would be to the advantage of any country to start a system of electrical units of its own. In spite of this favourable experience we all know

that there exists still an obstinate resistance against the general introduction of the metrical system into this country, although it has been shown over and over again that the reasons advanced for this obstruction are based on fallacies and are opposed to the experience of other countries. Such an attitude is very largely due to want of practice with a thoroughly consistent system of weights and measures.

In this connection it is significant that all engineers, who have had to deal for any length of time with metrical measurements, unhesitatingly prefer them to the English want-of-system.

I am, of course, aware that some prominent engineers profess to have tried to work with metrical units, and have been unable to work as quickly and easily with them as they could with their accustomed units. About them one is tempted to say that their conclusion in this particular matter is about as reliable as that of a man who tries to skate, and, after his first attempt, gives it as his opinion that you cannot move faster by skating than by walking.

An outcry is also raised against the metrical system on account of the great disturbance of all business transactions that would result from the change of weights and measures, and appalling figures are marshalled as to the expense of giving effect to it. It does not appear that other countries have been ruined by discarding obsolete weights and measures, nor should the expert opinion of the Inspectors of Weights and Measures be ignored, who unanimously consider the introduction of the metrical system to be a pressing need to put an end to the existing confusion, and advocate, in addition, that such an introduction should be made compulsory.

The very same thing has been recommended by two or three Parliamentary Committees, after hearing innumerable witnesses for and against the change.

Why should such well-considered opinions be calmly ignored, and why should the bogie of expense be trotted out before the public, when the real sentiments of the opponents about the disturbance of trade, caused by a change of weights and measures, are disclosed by the fact that they themselves advocate a revised British System, based on the inch, the introduction of which would have the same disturbing influence as that of the metrical system, but would lack the advantage of making the international metrical system the universal one?

For it cannot be doubted that the example of Great Britain would immediately be followed by the British Colonies and by the United States, so that the whole community in all civilised countries would enjoy the same advantages as the electrical engineers already possess.

Against this argument the opponents of the metrical system usually point out that even in France some instances can be found where the old measures are used in preference to the metrical ones, but on examination it turns out that only the old names are preserved in some districts and in some trades, but that in reality the measures in use are based upon metrical units.

The few exceptions only prove the rule; and it would be as wrong to say that the metrical system is not universally used in France or Germany, as it would be to contend that English is not the language

of the United Kingdom, because you can find people in some remote parts, who do not know that language.

As a last resort the expense of changing the screw-threads is urged against the change to the metrical system, and the Continental practice of calling their system "Whitworth Thread" is considered an incontrovertible proof that the metrical screw-thread is impracticable. If all taps and dies and leading screws had to be exchanged at once, it would certainly be a costly affair, but such a measure is not likely to be adopted, as no advantage could result from it. For the real difficulty with screw-threads is that giving dimensions on paper is not sufficient to insure that the screws, manufactured according to such instructions in different works, are really interchangeable.

This subject has been investigated by a Committee of the War Office, and their conclusions throw a very interesting light on the controversy. In their opinion it is only possible to obtain interchangeable screws, if the leading screws by which they are made have all been cut on the same screw-cutting lathe, or are at least cut on benches which are fitted with a leading screw manufactured on the original bench. If another link is interposed differences in the screws turned out become perceptible. As a consequence of the finding of this Committee a screw-cutting lathe has been set up at the National Physical Laboratory, where leading screws for screw-cutting lathes are to be manufactured. The same experience has been made in other countries, where nominally "Whitworth's Threads" are used. It is not possible to make screws interchangeable by prescribing their dimensions, the only way is to obtain taps and dies or leading spindles cut by the same tools. If it is not a case of extreme accuracy, there is no difficulty in cutting English thread by means of a metrical leading screw or *vice versâ*. By using a wheel with 127 teeth on the driving axle an accuracy of 1:6350 can be obtained which is sufficient for ordinary purposes. In any case it is much to be regretted that such a useless encumbrance of our daily life, and such an obstacle to foreign trade, as the English system of weights and measures, has not yet been abolished.

Among other external factors which influence the technical side of industries are the Patent Laws of the various countries—a subject on which the most divergent opinions have been pronounced.

It would lead me too far if I attempted to discuss all the features connected with the patenting of inventions, but I should like to draw your attention to the fallacy of considering a preliminary examination, as practised in the United States, for instance, to be of any advantage to an inventor. That system of examination involves as much trouble and expense as a lawsuit, but it is no bar to a lawsuit being commenced later on, and then the same case has to be argued over again. A favourable result of such an examination is, therefore, in no way a guarantee of the validity of the patent—on the other hand, I willingly admit that anticipations may be detected by this search.

It is, however, the business of an inventor to find out for himself what has been done before, and to make himself thoroughly acquainted with his subject before he begins to suggest improvements. In this work he should be assisted by the staff of the

Patent Office, where a reference library for technical subjects should be at his disposal, containing not only patents, industrial designs, or models and trade marks, as prescribed by Article XII. of the International Convention for the Protection of Industrial Property, but also files of the principal technical and scientific periodicals and transactions. Searching such records will prove to be a liberal education, and will in the end be of greater advantage to the inventor than having this work done for him by the members of the staff, who cannot be as well qualified to make the search as the inventor himself, as their acquaintance with the essential features of a new device is necessarily inferior to his.

Other branches of legislation, beside the Patent Laws, affect industries directly or indirectly, either by prescribing measures for safeguarding health and life or by laying down rules for the building of factories, or by dealing with the liability of employers in case of accidents. With all these I do not propose to deal in further detail, as I hope that by merely outlining the influences under which industrial work is carried on, I have made it clear that they are very numerous and of the most varied character.

It is, therefore, extremely difficult to foresee what consequences a serious alteration of some of the existing conditions will entail, while it would be quite wrong to draw conclusions about them from a comparison of the industrial state of a country at the present time with its position before railways, telegraphs and other modern contrivances revolutionised the intercourse between nations. By their aid manufacturers in any country are now in a position to make themselves quickly acquainted with the latest improvements affecting their business that may have been discovered anywhere, and they can also ascertain without much delay and with very little trouble what the requirements are of the remotest communities. In this respect manufacturers of all countries are now competing on an equal footing, where formerly those of one country may have had great advantages over the others.

Another cause of change has been the desire of many countries to create their own industries instead of depending on foreign supplies, and it is not very wonderful that the British industries have now rivals where sixty years ago they had absolute command of the markets.

If complaints are now raised that this foreign competition is ever increasing, and proposals are made to protect British industries against it, two points require particular attention.

One is that it is hopeless to endeavour to re-establish the former monopoly, and the second that it might be useful to investigate whether the former industrial supremacy has not led to neglect in many respects, for which we now have to suffer.

Whatever remedies may be proposed to enable British industries to hold their own in the general competition, there is in my opinion only one safe test for selecting the right ones, and that is that their adoption should lower the cost of production for the benefit of the whole community.

Colonel R. E. B. CROMPTON, C.B.: Gentlemen, it is my privilege to propose: "That the very best thanks of this Institution be accorded to Mr. Alexander Siemens for his most interesting, and, I may add, somewhat controversial address, and that, with his permission, the address be printed in the Journal of the Institution." I have only one regret, and that is that Mr. Siemens cannot be persuaded to bring some of the matters before us in the form of a paper, for I am sure it would lead to the most interesting discussion that has ever taken place in this Institution. Colonel Crompton.

Mr. J. E. KINGSBURY: It is my privilege to second the resolution which Colonel Crompton has moved. I am aware that this resolution is not one which requires many words, because it will unquestionably receive your hearty approval. But, at the same time, I do not think it would be right for me to allow the opportunity to pass in a simply formal manner. Colonel Crompton has remarked that the President's excellent address is somewhat controversial; he has also suggested that it might be of advantage if Mr. Siemens would do us the honour of presenting some of its features in a paper later on. I think I may say that Mr. Siemens has already done that, or at least in that part which I suspect Colonel Crompton has in mind as being controversial. Whilst the paper may be controversial in some respects, I think I may say that it has in it the essence of that which is to us the real means of our existence as electrical engineers. In the last two lines of his address Mr. Siemens has given us what, in an address from the pulpit, is usually put first; the real text of the address is that the lowering of the cost of production for the benefit of the whole community is what we should strive for. On that we are dependent, and Mr. Siemens has given us some most valuable advice as to the means of attaining it. We know that some, at least, of his grey hairs are due to his efforts on behalf of the manufacturer in trying to overcome some of the prejudices of the British workman. In that we owe him a considerable debt, but we owe him more than that, for he has given us here the basis upon which our manufactures depend: he has taught us what we have to look for in developments. Whilst it may certainly be claimed that, as electrical engineers, a part of our duty has been the provision of luxuries, we may also claim that these have become necessaries, that what we are doing is to provide such necessities for the community, and these necessities will constantly increase. Our President has given us an address which is not only interesting, but extremely valuable, and I have very great pleasure in seconding the resolution that has been proposed by Colonel Crompton. Mr. Kingsbury.

The resolution was then put to the meeting by Colonel Crompton, and carried with acclamation.

The PRESIDENT: I thank Colonel Crompton and Mr. Kingsbury very much for the kind way in which they have proposed and seconded this vote of thanks, and you, gentlemen, for the manner in which you have received it. I was very sorry that I could not quite keep the controversial matter out of the address, but I considered it an extremely important point, and a very great advantage to the electrical industry, that that industry possessed units which wer The Pres

The President.

all countries, and which made it possible for electrical engineers to understand formulæ and deductions and new discoveries which were made in all foreign countries. We know perfectly well that if we have to convert measures from one into the other—I will not say English—there is always a great liability that errors will creep in, and all those have been avoided in the electrical industry, which is certainly a very important and serious advantage. That was the principal point I wished to bring forward. I thank you very much for the way in which you have listened to the address.

I have to announce that the twenty-fifth anniversary of the German Elektro-technische Verein will be held on the 22nd of this month, and they have invited your President to be present at that celebration, and as it happens that I have to be in Berlin about that time I shall attend on your behalf. The Council have requested me to do so, and have written to the German Society to say that I shall represent you, and I hope I shall be acting in accordance with your wishes if I take the opportunity of again thanking the German engineers who are concerned for the very kind way in which they received our travelling party three years ago.

The meeting adjourned at 9.20 p.m.

Proceedings of the Four Hundred and Thirteenth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 24, 1904—Mr. J. E. KINGSBURY, Vice-President, in the chair.

The Minutes of the Ordinary General Meeting held on November 10, 1904, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—
Percy Sewell Sheardown.

From the class of Associates to that of Members—
Henry White Bowden.
Henry Llewellyn Thomas Foster.
Frederick William Schiller.

From the class of Associates to that of Associate Members—
Samuel Romilly Roget.
Norman Arthur Thompson.

From the class of Students to that of Associate Members—
Alfred Carleton Blyth.

Messrs. H. G. Wood and C. K. Falkenstein were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Member.

William H. Scott.

Associate Members.

William N Brand.
Harry Chapman.
Charles O. Clark.
F. W. Courtenay Clarke.
Richard S. Dahl.
Edward E. V. Duckenfield.
Joe Frost.
Roland C. Hall.
James W. Haworth.
Robert Howe.
Robert Hudson.
F. Gómez Humáran.
Albert H. Marshall.
Ernesto Marty.
Mathew F. Newton
James Plucknett, Jun.
Francis B. Rylands.
Charles N. Sims.
Gordon S. Sommerville.
Harry E. Todd.
S. Von Ammon.
Stephen N. Wellington.
Fred Wilkinson.
Sydney Windle.

Associates.

Francis I. L. Ditmas.
Frederick H. Goodwin.
Walter G. Hodgson.
Wilfred L. Hutchinson.
Edwin Henry Lee.
Richard H. Martin.
Arthur M. Wilson.

Students.

Edgar P. Austin.
Maurice G. Bland.
William F. Brown.
Geoffrey A. Freire-Marreco.
Aston C. Gardner.
Charles M. B. Mersh.
Frederick C Prentice.
John Pullar.
Thomas N. Riley.
Harold M. Smith.
Cyril F. Upward.
Percy S. Ward.
Joseph H. Wilkinson.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Blackie & Son, Mr. T. Feilden, and the Institution of Mining Engineers, to whom the thanks of the meeting were duly accorded.

The following paper was then read :—

HYDRODYNAMICAL AND ELECTROMAGNETIC INVESTIGATIONS REGARDING THE MAGNETIC-FLUX DISTRIBUTION IN TOOTHED-CORE ARMATURES.

By Professor H. S. HELE-SHAW, F.R.S., LL.D., ALFRED HAY, D.Sc., Member, and P. H. POWELL, M.Sc., M.Eng.

(*Paper read November 24th*, 1904.)

INTRODUCTION.

Some four years ago, two of the authors of the present paper described a hydrodynamical method of attacking two-dimensional magnetic problems. A full account of this method, including a detailed description of the apparatus used and examples of the results obtained, will be found in a paper read before the Royal Society in 1900.* Here it will be convenient, for the sake of those members who do not happen to have seen an account of the method, to give a brief outline of it.

The laws which govern the distribution of magnetic flux in space of two dimensions are identical with those which apply to the stream-line motion of a perfect incompressible fluid in two-dimensional space. If, therefore, we had such a fluid at our disposal, and by causing it to flow between parallel bounding walls determined the distribution of the stream-lines, this distribution would also represent that of the magnetic lines or tubes in a corresponding two-dimensional magnetic problem. This is the underlying principle of the hydrodynamical method as applied to magnetic problems.

In trying to put the principle of this method into practice, we encounter the following difficulties :—

(1) The difficulty of mapping out the invisible stream-lines of the fluid.

(2) The fact that there is no such thing as a "perfect" fluid at our disposal, every actual fluid being more or less viscous.

(3) The difficulty of finding an analogue for magnetic permeability, in order to enable us to study the flux distribution in cases where the field consists of regions having different permeabilities.

The first difficulty is surmounted by using a colourless liquid into

* *Phil. Trans.*, vol. 195 (Series A), pp. 303–327.

which are injected thin colour-bands, consisting of the same liquid mixed with a suitable dye.

The second difficulty at first sight appears insurmountable. But a closer mathematical investigation by the late Sir G. G. Stokes* has revealed the somewhat astonishing fact that when any viscous liquid is made to flow in a very thin layer between parallel bounding walls, *the stream-lines obtained are identical with those of a perfect liquid.* This theoretical deduction was subsequently fully confirmed by us in the paper already referred to. The liquid used by us was glycerine, and the dye for mapping out the stream-lines was a mixture of aniline dye with permanganate of potash. The colour-bands were injected through a series of very minute apertures at one end of the slide between whose parallel plate-glass walls the liquid was allowed to flow.

The third difficulty consists in devising a method by means of which the effects of varying magnetic permeability may be imitated. Now it may be shown, both theoretically and experimentally, that when a viscous liquid flows in a thin parallel layer, the quantity of liquid passing per second across a plane of unit length drawn normally to the stream-line is, for a given pressure gradient, proportional to the cube of the thickness of the liquid layer. Hence if the liquid is made to flow through several regions of different thickness, the ratio of the cubes of the thicknesses gives the ratio of the permeabilities in the corresponding magnetic problem.

The practical method employed by us consisted in coating one of the thick glass plates forming the slide with a thin layer of paraffin wax, and then forming a cavity or cavities of the required shape by carefully scraping away the wax over certain areas. These areas, over which the liquid layer had a greater thickness, represented the regions of high permeability. The permeability varying in proportion to the *cube* of the thickness of the liquid layer, no difficulty was experienced in constructing slides in which the permeability over certain regions had as high a value as 1,000.

The slide through which the liquid flowed being suitably illuminated, a photograph of it was taken, and thus a permanent record obtained of the particular stream-line distribution. The stream-line diagrams which accompany the present paper are reproductions of the photographs so obtained.

The validity of the hydrodynamical method having been established, we proceeded to apply it to some problems in dynamo design whose solution has always been a matter of considerable difficulty. In order to render the investigation more complete, however, we decided to supplement the results obtained by the hydrodynamical method by experiments carried out on an actual dynamo fitted with several armature cores having different sizes of teeth and slots, and different air-gaps.

In the calculation of the magnetomotive-force required to produce a given magnetic flux in a dynamo having a toothed-core armature,

* *British Association Report*, 1898.

the designer encounters two important difficulties. The first of these is the determination of the reluctance of the air-gap, including the allowance to be made for the distortion due to the armature teeth and for the fringing effect around the edges of the pole-pieces; and the second is the determination of the reluctance of the teeth themselves.

It is in connection with the first problem—that of air-gap reluctance—that the hydrodynamical method is of particular value. Although attempts were made to apply it to the second problem—that of tooth reluctance—by *continuously* varying the thickness of the liquid layer representing the tooth, yet the difficulties of doing this satisfactorily proved so great that the attempt had to be abandoned, and we preferred to carry out this part of the investigation electromagnetically, using the ballistic method.

Hydrodynamical Investigation of Air-gap Reluctance.

For the sake of completeness, and with a view to rendering the present paper as useful as possible to the dynamo designer, we propose, in what follows, not to restrict our attention to our own investigations, but to supplement these by an account of the more important researches carried out in this direction by other workers, and to state the results obtained in a form which will render them available for immediate practical use.

A. *Fringing Problem.* If we except various more or less arbitrary and imperfect empirical rules, the first attempt to deal with this problem in a scientific manner is due to Messrs. C. C. Hawkins and R. Wightman, who, in a paper entitled "The Air Gap Induction in Continuous Current Dynamos,"* obtained a correction for the fringing effect on the basis of certain very simple (though incorrect) assumptions regarding the form of the magnetic lines in the vicinity of the polar edges. Shortly afterwards, Mr. F. W. Carter† succeeded in obtaining an exact mathematical solution of an ideal case, which, however, so closely corresponds to the actual problem, that from a practical point of view the solution may be regarded as furnishing a perfect correction.

The most convenient way of taking the fringing effect into account is to add to the length of the polar arc (in estimating the effective cross-sectional area of the gap) a term consisting of the product of the single air-gap length into a certain coefficient c whose value depends on the ratio $r = \frac{\text{distance between pole-pieces}}{\text{length of single air-gap}}$, the "distance between the pole-pieces" being measured along the armature circumference, and hence

* *Journal of the Institution of Electrical Engineers*, vol. 29, p. 436.
† *Ibid.*, p. 925; also *Electrical World and Engineer*, vol. 38, p. 884.

representing the length of the interpolar arc. The following table contains the values of c corresponding to various values of r:—

$r =$	4	5	6	7	8	9	10	12	14	16	18
$c =$	1·32	1·59	1·79	1·98	2·15	2·30	2·43	2·65	2·84	3·00	3·15
$r =$	20	22	24	26	28	30	35	40	45	50	60
$c =$	3·28	3·40	3·51	3·61	3·70	3·78	3·98	4·14	4·28	4·40	4·66

By means of this table, the necessary correction in any given case may be at once applied.

B. *Problem of the Flux Distortion brought about by Armature Teeth.* It is at once obvious, without the necessity of going into any elaborate investigation, that the presence of teeth in the armature core is equivalent to an increase in the gap length (or a reduction in the gap area); for the teeth disturb the uniformity of the flux distribution, causing a crowding of the lines into the regions of the polar surface which are opposite the teeth, and thereby increasing the fall of magnetic potential, from the polar surface to the crown of any tooth, above the value which it would have if the distribution were uniform (*i.e.*, if the core were smooth) and the total flux unaltered.

Assuming that the exact effect produced by the teeth is known, a convenient method of applying the necessary correction consists in multiplying the gap length by a certain "correction coefficient," and then, using this corrected value, in proceeding with the calculation as if the armature core were smooth—*i.e.*, as if the flux distribution were uniform. The "correction coefficient" clearly represents the ratio of the maximum to the mean induction.

It is not difficult to see that the value of the correction coefficient depends on the two ratios s/t and s/g, where $s =$ width of slot, $t =$ width of tooth (at top), and $g =$ length of gap. In Figs. 1–9 are given a series of stream-line diagrams for various values of the two ratios s/t and s/g, and a careful examination of these enables one to form a very good idea of the effect produced by the teeth. The chief value of the diagrams, however, lies in the fact that they enable us to determine the numerical values of the correction coefficient in the various cases. All that is necessary for this purpose is to determine the number of stream-lines per unit of length in the space well under cover of a tooth, and to divide this by the mean number of stream-lines per unit of length for tooth and slot. The values of the correction coefficient so obtained are given in Table I. (p. 8).

Attention may be drawn to one or two features clearly exhibited by the diagrams. Two well-known effects are shown very plainly: the *refraction* of the lines as they pass from a medium of low into one of high permeability, and the crowding of the lines along the sharp edge of the tooth. It will be further noticed that between the straight

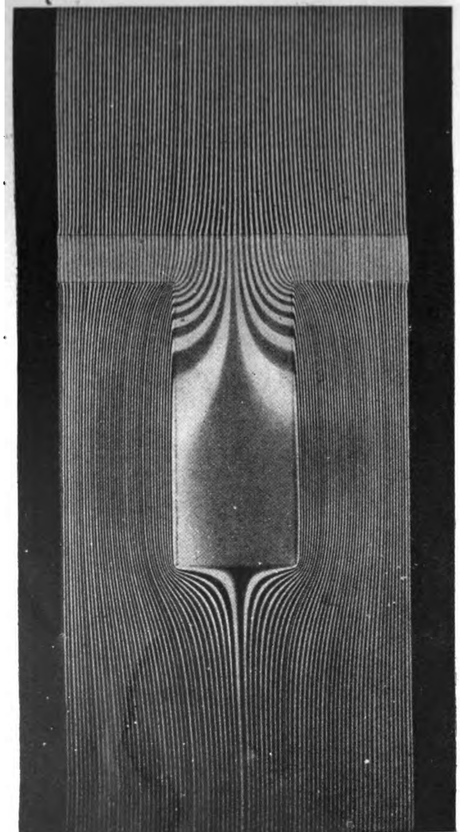


FIG. I.

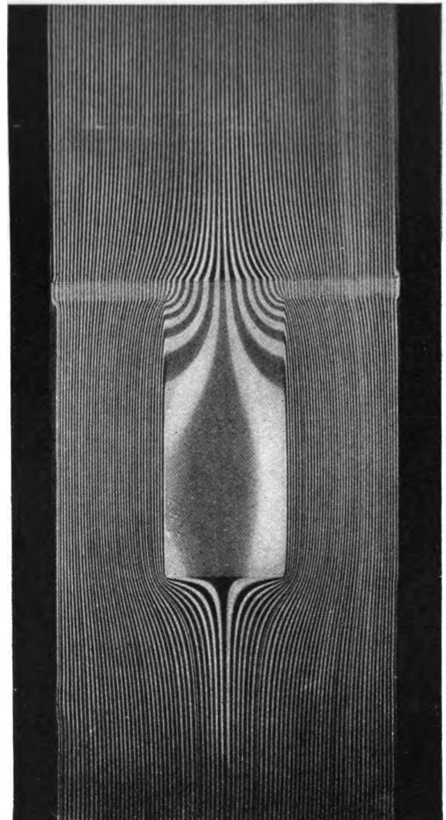


FIG. 2.

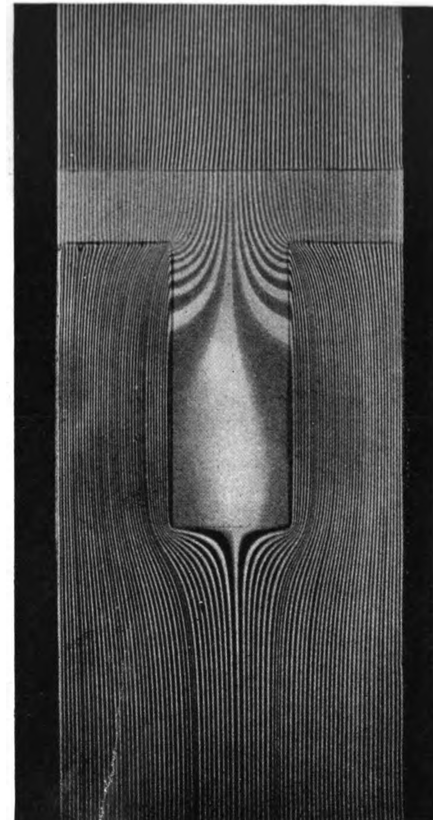


FIG. 3.

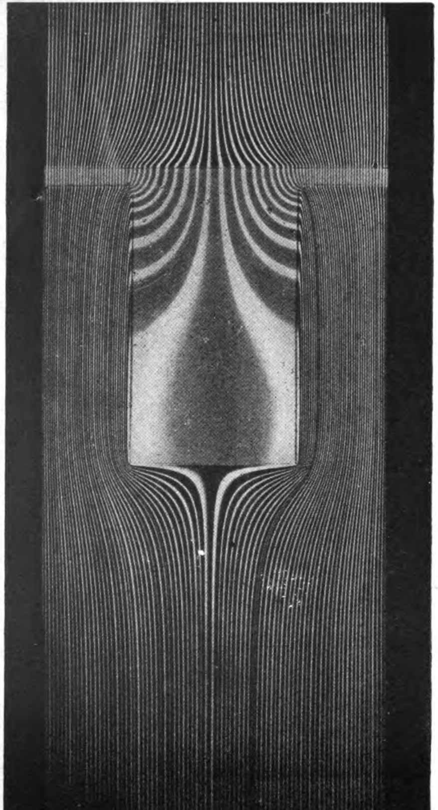


FIG. 4.

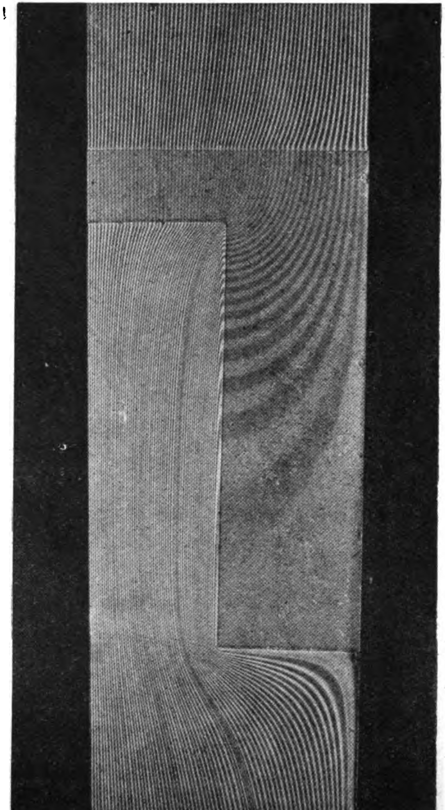


FIG. 5.

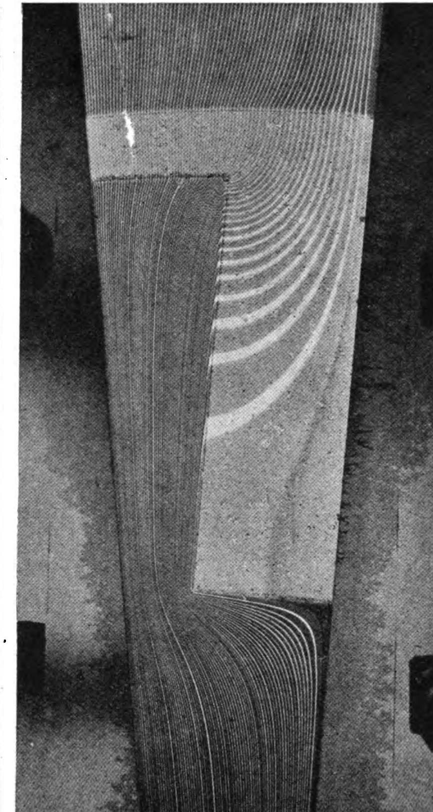


FIG. 6.

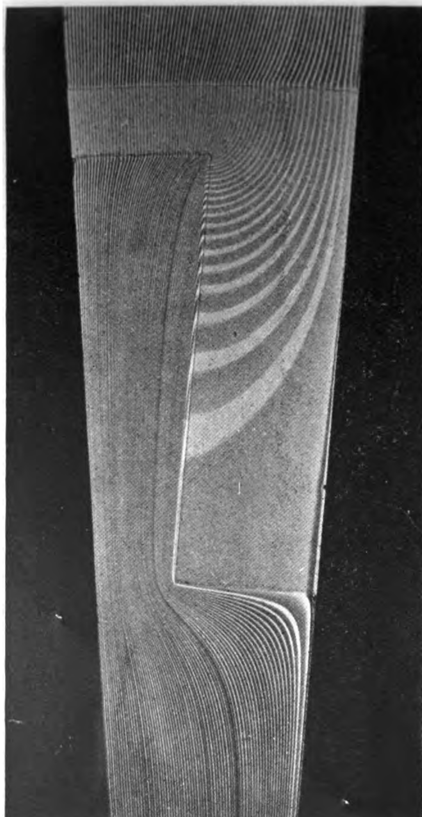


Fig. 7.

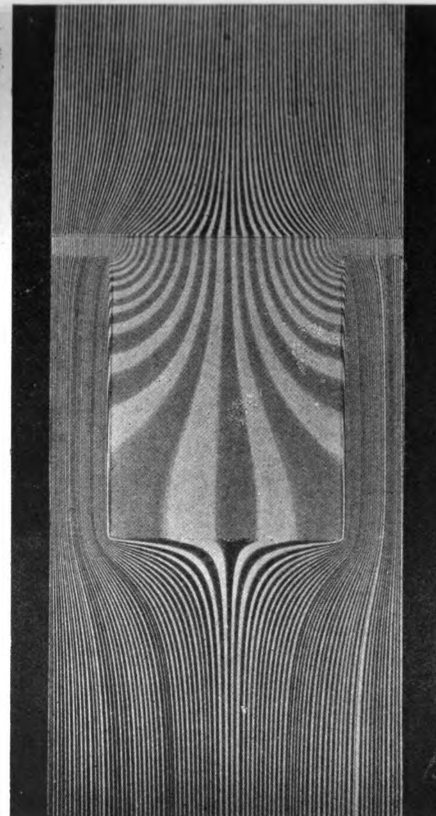


Fig. 8.

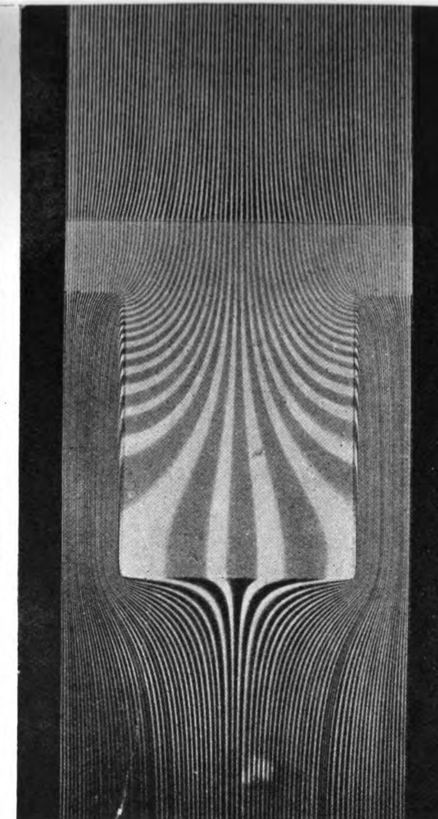


Fig. 9.

lines in the region immediately above the tooth and the curved lines entering the slot and passing into the flank of the tooth, and exhibiting a concavity towards the tooth, there are certain intermediate or transition lines which are characterised by a point of inflection, the upper part of the line being concave and its lower part convex towards the tooth; the convexity gradually decreases as we take lines further and further away from the tooth, and is ultimately replaced by a concavity along the entire length of the line.

While our experiments were in progress, a very important contribution to this subject appeared by Mr. F. W. Carter,* who followed up his previous success in attacking the fringing problem by working out a solution for the air-gap flux distribution in the case of a toothed-core armature, on the assumptions of negligible pole-face curvature and infinite permeability in the teeth. According to Mr. Carter, the correction coefficient is given by

$$\frac{1 + \frac{s}{t}}{1 + (1 - \sigma)\frac{s}{t}},$$

where $\sigma = \frac{2}{\pi}\left\{\tan^{-1}\frac{s}{2g} - \frac{g}{s}\log\left(1 + \frac{1}{4}\frac{s^2}{g^2}\right)\right\}$.

Mr. Carter's solution furnishes a correction coefficient which may be regarded as quite accurate enough for all practical purposes, and a reference to Table I. shows very satisfactory agreement between the values determined by our stream-line method and those calculated by means of Mr. Carter's formula. Such divergences as exist in some cases are to be attributed to want of accuracy in working out the values from the stream-line diagrams, due to their comparatively small scale.

Previous to the appearance of Mr. Carter's paper, various attempts were made to deal with the problem, and it is interesting to compare the results furnished by some of these older methods with the correct values now at our disposal.

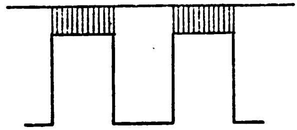


FIG. 10.

Method I.—A first very rough approximation may be obtained by assuming that none of the lines pass down towards the slot, but that they all crowd into the tops of the teeth, forming a series of isolated patches as shown in Fig. 10. The correction coefficient is on this

* *Electrical World and Engineer*, vol. 38, p. 884.

assumption given by $\frac{t+s}{t} = 1 + \frac{s}{t}$, and leads to a very considerable over-correction, which increases with increase of $\frac{s}{t}$.

Method II.—It is assumed that the effective area of the air-gap is the arithmetic mean of the polar area and that corresponding to the tops of the teeth. The correction coefficient is given by

$$\frac{t+s}{t+\frac{1}{2}s} = \frac{1+\frac{s}{t}}{1+\frac{1}{2}\frac{s}{t}}.$$

This method leads to an over-correction when $\frac{s}{g}$ is small, and to an under-correction when $\frac{s}{g}$ is large.

Method III.—The lines are assumed to be straight, and to be distributed in wedge-shaped masses as shown in Fig. 11. The

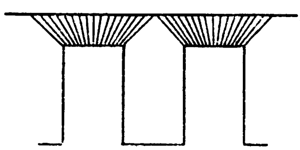


FIG. 11.

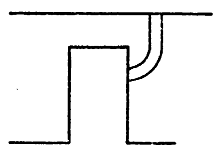


FIG. 12.

reluctance over a width of one tooth and slot, of a thin layer of unit breadth (measured in a direction parallel to the shaft), and at a distance x from the top of the tooth is $\frac{dx}{t+\frac{s}{g}x}$, where dx is the thickness of the layer. The total reluctance is thus

$$\int_0^g \frac{dx}{t+\frac{s}{g}x} = \frac{g}{s}\log\left(1+\frac{s}{t}\right).$$

But the reluctance corresponding to a smooth core is $\frac{g}{t+s}$. Hence the correction coefficient is $\left(1+\frac{t}{s}\right)\log\left(1+\frac{s}{t}\right)$. This method leads to an over-correction in most cases.

The three methods so far considered take no account of the ratio $\frac{s}{g}$.

Method IV.—This method, due to Messrs. Hawkins and Wightman,* assumes that the lines which fall outside the space immediately above

* *Journal of the Institution of Electrical Engineers*, vol. 29, p. 436.

a tooth consist partly of straight lines and partly of quadrants of circles whose common centre is at the edge of the tooth, as shown in Fig. 12. The reluctance of the air-space above the tooth is, per unit length of tooth measured in a direction parallel to the shaft, equal to $\frac{g}{t}$, while the reluctance of an equal length of air-space above a slot is

$$\frac{1}{2\int_0^{s/2}\frac{dx}{g+\frac{1}{2}\pi x}} = \frac{\pi}{4\log\frac{g+\frac{\pi}{4}s}{g}}.$$

Hence the joint reluctance of the two air-spaces is, per unit length along shaft, given by

$$\frac{\pi g}{4\,t\log\frac{g+\frac{\pi}{4}s}{g}} \Bigg/ \left(\frac{g}{t} + \frac{\pi}{4\log\frac{g+\frac{\pi}{4}s}{g}}\right) = \frac{\pi g}{\pi t + 4g\log\frac{g+\frac{\pi}{4}s}{g}}.$$

Now, since the value of this reluctance for a smooth core is $\frac{g}{t+s}$, it follows that the correction coefficient is given by

$$\frac{1+\frac{s}{t}}{1+\frac{4}{\pi}\cdot\frac{g}{t}\log\left(1+\frac{\pi}{4}\frac{s}{g}\right)}.$$

For the sake of comparison, the values calculated in accordance with each of these methods are included in Table I. (p. 28), and an examination of the results shows that none of these older methods is reliable.

Reluctance of Teeth in a Slotted Armature.

The problem of determining the reluctance of the teeth has generally been dealt with on the assumption that the equipotential surfaces in the teeth and slots are cylindrical surfaces coaxial with the core discs. This assumption is far from being correct, as is at once evident by an inspection of the stream-line diagrams and an examination of the experimental results given below. Nevertheless, for practical purposes the assumption yields sufficiently accurate results.

The problem is considerably more complicated in the case of armatures of small diameter, where the taper of the teeth may produce a very considerable difference between the cross-sectional areas of the top and bottom of a tooth. The most satisfactory method of dealing with the problem in such cases is that given by Mr. W. B. Hird in a paper contributed to the Journal of this Institution.*

* *Journal of the Institution of Electrical Engineers*, vol. 29, p. 933.

TABLE I.

Air-gap Correction Coefficients.

Fig.	$\frac{s}{t}$	$\frac{s}{g}$	Stream-line Method.	Carter's Method.	Method I.	Method II.	Method III.	Method IV. (Hawkins and Wightman.)
—	·500	3·42	1·18	1·16	1·50	1·20	1·22	1·21
1	·510	2·80	1·13	1·14	1·51	1·20	1·22	1·19
2	·515	6·65	1·26	1·24	1·515	1·21	1·22	1·28
—	·524	2·35	1·11	1·12	1·524	1·21	1·23	1·18
3	·530	1·68	1·10	1·09	1·53	1·21	1·23	1·14
—	·670	4·57	1·21	1·24	1·67	1·20	1·28	1·30
—	·770	2·27	1·22	1·16	1·77	1·28	1·32	1·23
—	·810	4·00	1·23	1·25	1·81	1·29	1·33	1·32
4	·940	9·60	1·52	1·48	1·94	1·32	1·37	1·53
—	·945	4·30	1·31	1·30	1·945	1·32	1·38	1·38
5	1·00	4·00	1·25	1·29	2·00	1·33	1·39	1·38
6	1·00	4·40	1·37	1·32	2·00	1·33	1·39	1·40
—	1·00	4·50	1·32	1·32	2·00	1·33	1·39	1·40
7	1·02	4·45	1·35	1·32	2·02	1·34	1·40	1·41
—	1·28	3·90	1·33	1·34	2·28	1·39	1·52	1·44
8	1·88	13·2	1·98	1·95	2·88	1·48	1·62	1·95
—	1·94	3·86	1·37	1·40	2·94	1·49	1·63	1·55
9	1·98	3·34	1·39	1·36	2·98	1·50	1·64	1·51

The object of the experiments about to be described was to investigate the exact distribution of the magnetic flux in the teeth and slots of a slotted core under varying conditions of excitation, and with varying length of gap and width of tooth.

Description of Apparatus.

The apparatus used was specially designed for the purpose, and was constructed in the workshops of the University of Liverpool.

The dynamo (Fig. 13) was of the single-bobbin type, with field core

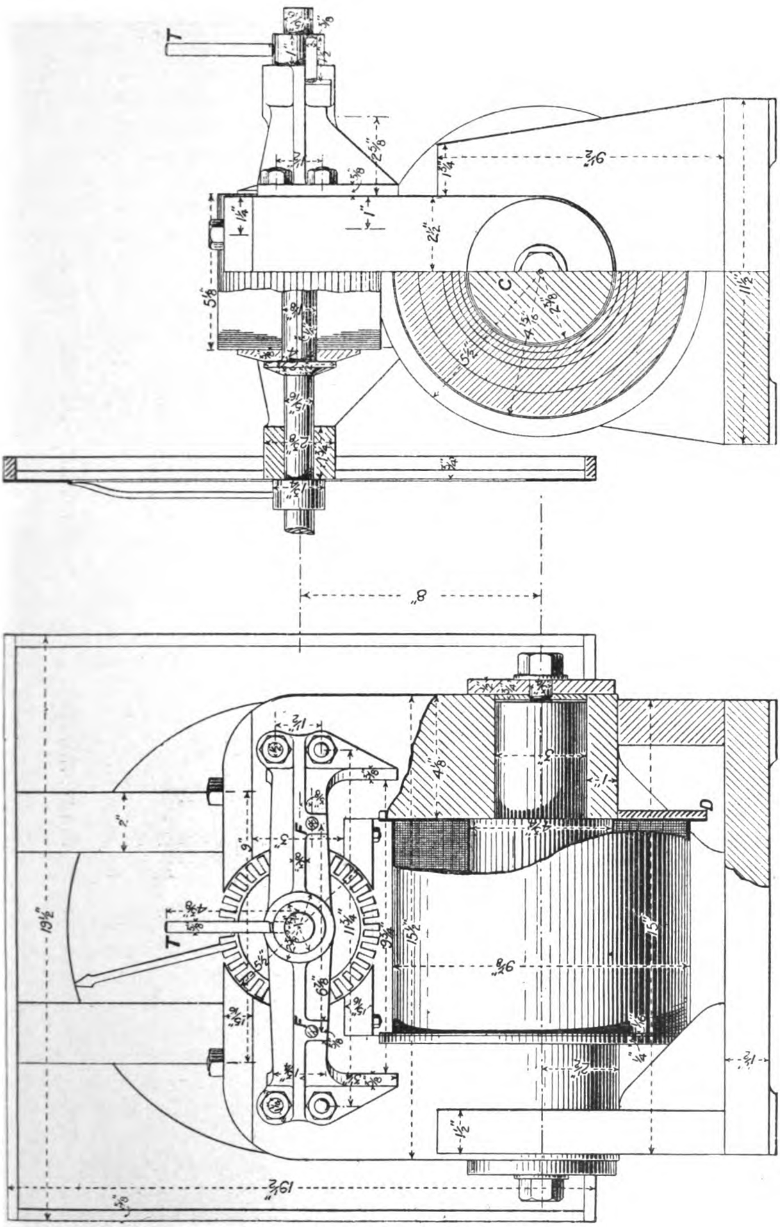
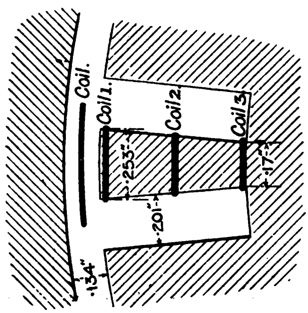
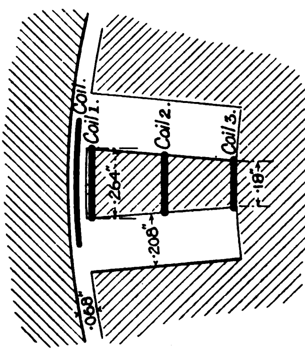


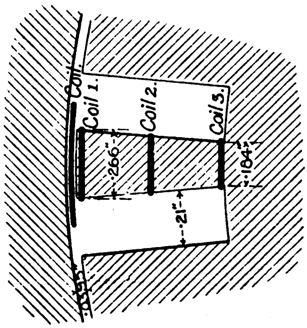
FIG. 13.



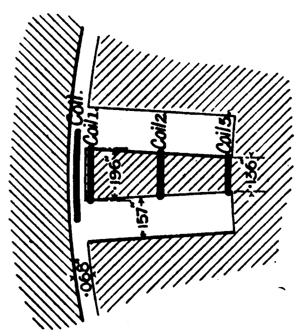
Armature 5¾″ diamr. 36 teeth.



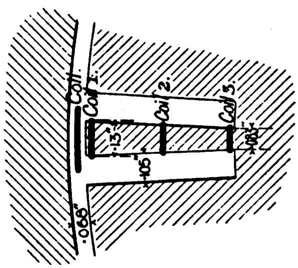
Armature 5⅜″ diamr. 36 teeth.



Armature 5 7/16″ diamr. 36 teeth.



Armature 5⅜″ diamr. 48 teeth.



Armature 5⅜″ diamr. 72 teeth.

Fig. 14.

and pole-pieces of Allen's dynamo steel. The pole-pieces were bored out to a diameter of $5\frac{1}{2}$ in., and into the space between them could be introduced any one out of a set of five experimental armature cores. The cores consisted of sheet-iron stampings—supplied by Messrs. Sankey & Co.—threaded on the shaft, and maintained in position by means of a feather key. The details of the five cores are given in Fig. 14, which also shows the positions of the search-coils by means of which the flux at three different cross-sections of a tooth was determined. The three cross-sections corresponded to the top, the middle, and the bottom of a tooth. In addition to the three coils surrounding the tooth, a fourth coil, of width equal to that of a tooth and slot, was used for the purpose of determining the total flux per tooth and slot, and the mean value of the gap induction. This coil was slung in position by means of silk threads suitably supported from the armature shaft, and provided with a device for adjusting the coil in the

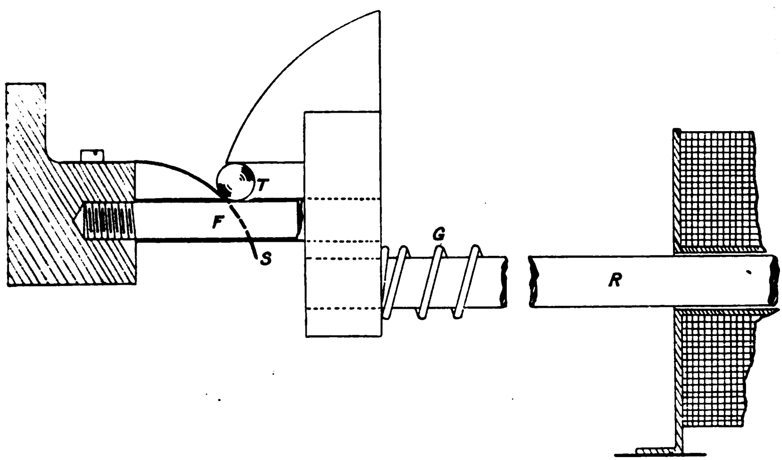


FIG. 15.

exact position required (the middle of the air-gap). Each of the three coils surrounding the tooth consisted of five turns of No. 46 wire. Considerable difficulty was experienced in insulating the coils satisfactorily, but this was successfully overcome.

In order to avoid the various difficulties and uncertainties connected with the measurement of the magnetic flux by the methods of breaking and reversing the exciting current, we decided to produce the reversal of flux by a bodily rotation of the armature with its search-coils through 180°. For this purpose the armature was first placed in such a position as to bring the tooth carrying the search-coils opposite the middle of one of the pole-pieces. The particular search-coil in use being in connection with the ballistic galvanometer, the armature was then suddenly rotated through half a revolution, the tooth being thereby brought opposite the middle of the other pole-piece. This operation

occupied an interval of time not exceeding one second. In order to limit the motion of the armature, the shaft was fitted with a striker T (Fig. 13) capable of moving between two steel stop-pins F F. In order to prevent excessive shock due to the sudden stoppage of the armature, buffer springs S (Fig. 15) were provided, and in order to prevent subsequent recoil suitable catches, one of which is clearly shown in Fig. 15, were fitted. Each catch is forced outwards by a spring G, which is normally in compression, and which surrounds the core R

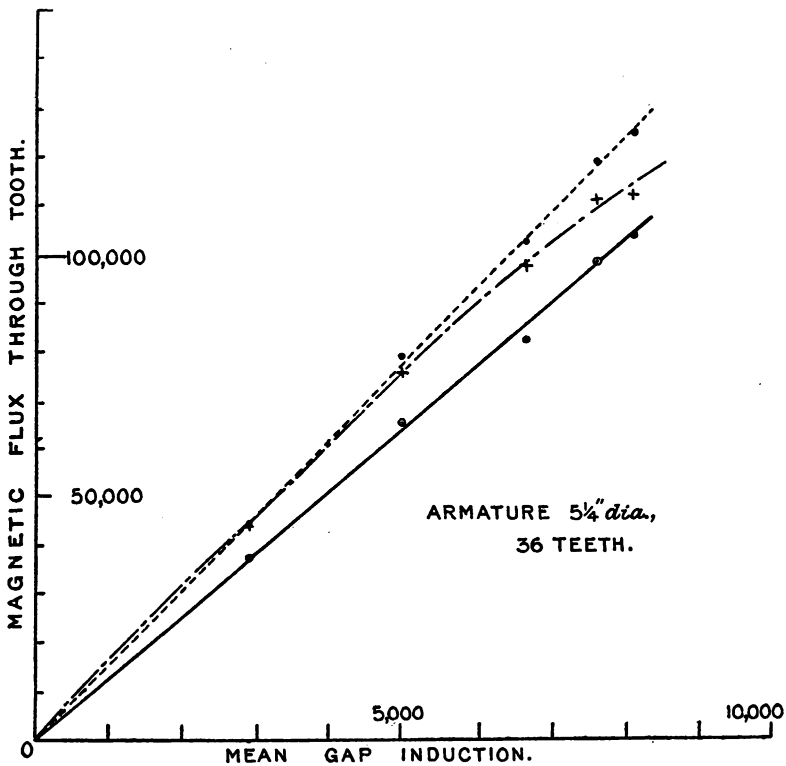


Fig. 16.

of a solenoid, the catch being attached to the end of the core. When the solenoid is excited, it sucks in its core, withdrawing the catch and leaving the armature free to rotate through 180°. A similar coil-and-plunger release was fitted to the catch on the other side.

In order to arrive at a correct estimate of the nett cross-section of iron in the tooth, a large number of measurements was made of the thickness of the discs forming the different cores, and the number of discs in each core was carefully counted. The exact dimensions of the various exploring coils were also determined with great care. The

results of the more important of these measurements are given in Table II.

TABLE II.

DETAILS OF ARMATURE CORES AND SEARCH COILS IN GAP.

Diameter of core, in inches ...	$5\frac{1}{4}$	$5\frac{3}{8}$	$5\frac{3}{8}$	$5\frac{3}{8}$	$5\frac{7}{16}$
Number of teeth	36	36	48	72	36
Length of single air-gap, in cms.	·340	·173	·173	·173	·100
Width of slot, in cms.	·5105	·528	·399	·267	·533
Width of tooth at top, in cms....	·640	·671	·498	·330	·675
" " middle " ...	·510	·528	·399	·267	·559
" " base " ...	·430	·457	·345	·211	·467
Width of coil in gap, in cms....	1·165	1·15	·833	·62	1·20
Length " " " ...	9·70	9·68	9·5	9·39	9·49

The moving-coil ballistic galvanometer used in these experiments was of special construction; we found that no suitable commercial instrument could be obtained. A detailed account of the principles underlying the design of such an instrument has already been published by one of us.* It may, however, be mentioned that the periodic time of the instrument was about 80 seconds, and that the electromagnetic damping device consisted of a copper disc suspended below the coil and acted on by a small electromagnet. Throughout the experiments the galvanometer was very frequently calibrated by means of a standard solenoid.

RESULTS OF BALLISTIC EXPERIMENTS.

The magnetic flux through the top, the middle, and the bottom of the tooth was determined for five different values of the mean gap induction, ranging from about 3,000 to about 9,000 C.G.S. lines per sq. cm., for each of the five armature cores experimented upon. The results obtained are given in Table III. (p. 36), and are graphically exhibited in Figs. 16–20, the curves there shown having the mean gap induction for abscissæ and the magnetic fluxes through the various cross-sections of the tooth for ordinates. The full-line curve in each case refers to the total flux through the top of the tooth, the dotted curve to the flux through the middle of the tooth, and the chain-dotted curve to the flux through the base of the tooth. The three readings marked with an asterisk in Table III. are open to some doubt, and are almost certainly too low; unfortunately,

* *British Association Report*, 1903; or *Electrician*, vol. 51, p. 1013.

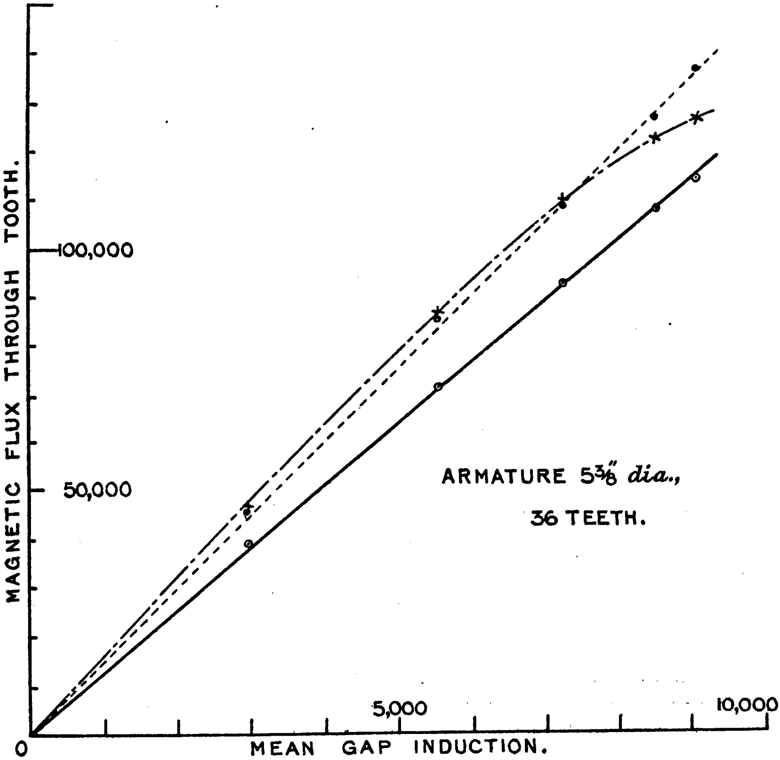

MAGNETIC FLUX THROUGH TOOTH.
100,000
50,000
ARMATURE 5⅜" dia.,
36 TEETH.
0
5,000
10,000
MEAN GAP INDUCTION.

FIG. 17.

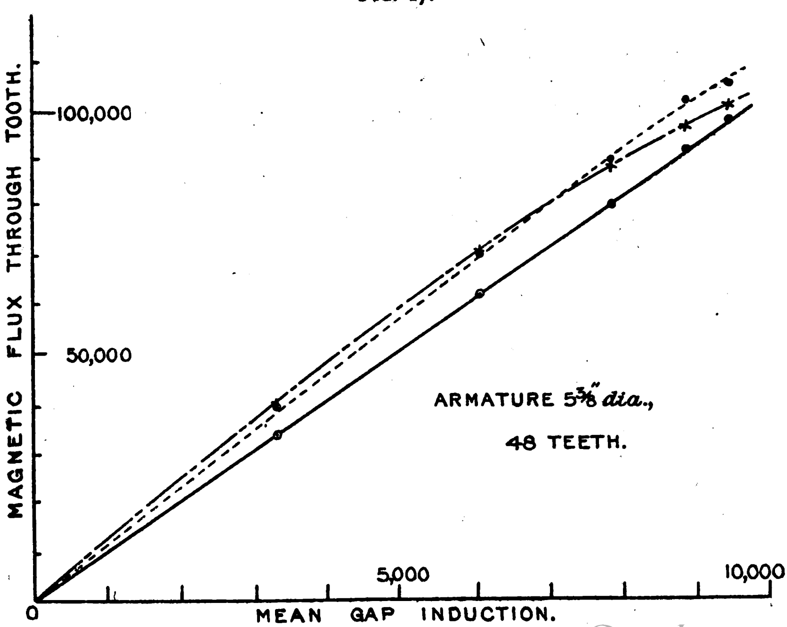

MAGNETIC FLUX THROUGH TOOTH.
100,000
50,000
ARMATURE 5⅜" dia.,
48 TEETH.
0
5,000
10,000
MEAN GAP INDUCTION.

FIG. 18.

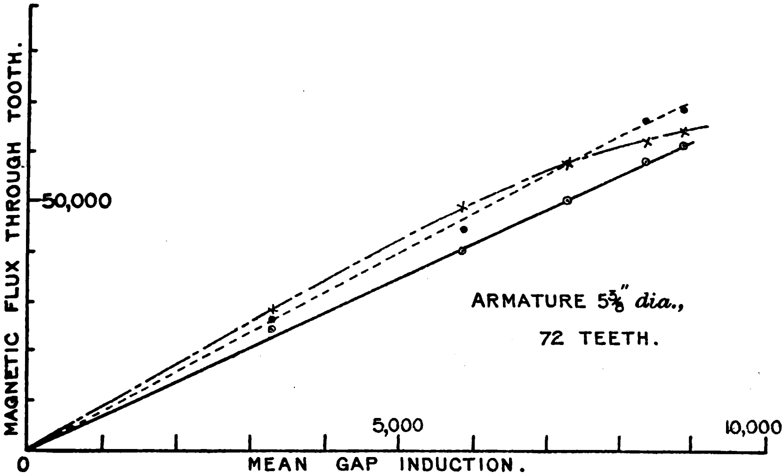

MAGNETIC FLUX THROUGH TOOTH.
50,000
ARMATURE 5⅜" dia.,
72 TEETH.
0
5,000
10,000
MEAN GAP INDUCTION.

FIG. 19.

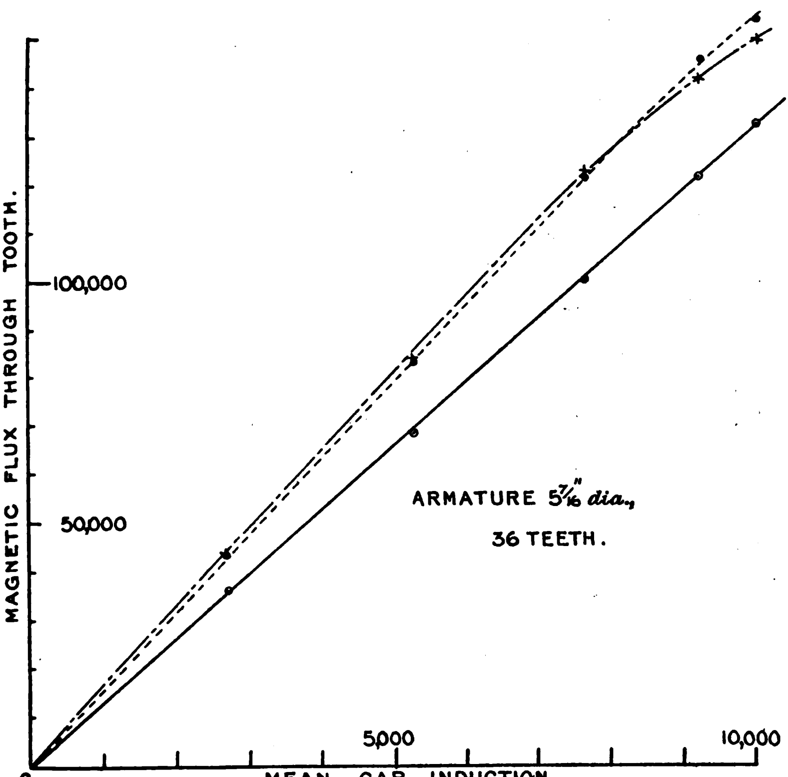

MAGNETIC FLUX THROUGH TOOTH.
100,000
50,000
ARMATURE 5 7/16" dia.,
36 TEETH.
0
5000
10,000
MEAN GAP INDUCTION.

FIG. 20.

TABLE III.

		Total Magnetic Flux.			Magnetic Induction.		
	Mean Gap Induction.	Top of tooth.	Middle of tooth.	Base of tooth.	Top of tooth.	Middle of tooth.	Base of tooth.
Armature 5¼" in diameter, 36 teeth.	2,890	37,000	44,400	43,800*	4,790	7,040	8,570
	5,010	65,800	79,100	75,900*	8,520	12,600	14,850
	6,650	81,900	102,500	97,400*	10,600	16,300	19,060
	7,610	98,500	118,900	111,400	12,750	18,870	21,800
	8,110	103,200	125,000	112,300	13,370	19,850	21,980
Armature 5⅜" in diameter, 36 teeth.	2,950	38,900	45,200	46,300	5,070	7,120	8,920
	5,540	71,200	84,600	86,000	9,280	13,400	16,600
	7,240	92,200	108,500	109,400	12,250	17,100	20,940
	8,550	107,800	126,400	121,800	14,060	19,900	23,500
	9,120	113,800	136,200	126,000	14,800	21,500	24,300
Armature 5⅜" in diameter, 48 teeth.	3,290	33,200	39,100	39,600	5,350	7,440	8,700
	6,020	61,800	69,300	70,000	9,950	13,200	15,350
	7,860	79,500	89,500	87,600	12,800	17,000	19,200
	8,880	91,000	101,900	96,200	14,700	19,400	21,100
	9,440	97,500	105,400	101,000	15,700	20,000	22,140
Armature 5⅜" in diameter, 72 teeth.	3,280	24,300	26,800	28,600	6,070	8,130	10,560
	5,810	40,300	44,900	46,000	10,080	13,600	17,000
	7,270	50,900	57,800	57,300	12,700	17,500	21,160
	8,380	58,400	66,200	62,300	14,600	20,100	23,000
	8,870	61,300	68,900	64,400	15,300	20,900	23,800
Armature 5 7/16" in diameter, 36 teeth.	2,680	36,100	43,400	43,500	4,280	6,020	7,450
	5,260	68,400	83,100	83,700	8,100	11,540	14,350
	7,630	100,700	122,000	122,700	11,900	16,950	21,000
	9,200	122,400	145,800	142,200	14,500	20,200	24,400
	10,000	133,300	154,100	150,200	15,800	21,400	25,700

the core had been dismantled before the discrepancy was noticed, and the readings could not be repeated.

A glance at the stream-line diagrams shows that the total flux through a cross-section of the tooth some distance below the surface of the armature is considerably greater than the flux through the top of the tooth. This is fully borne out by the ballistic measurements, which show clearly the steady increase in the magnetic flux as we proceed down the tooth—so long, at any rate, as the induction at the base of the tooth does not exceed about 20,000. When the induction is pushed beyond this limit, another effect, not indicated by the stream-line diagrams (in which the permeability is constant), comes into play. The flux, after reaching a maximum value at a certain depth, begins to decrease, lines of induction leaving the flank of the tooth and crowding into the slot. This effect is very strongly marked for all the higher values of the gap induction, and is indicated by the crossing of the curves in Figs. 16–20.

For the range of gap induction used, the curves connecting the total flux through the top and middle cross-sections of the tooth with the mean air-gap induction are almost indistinguishable from straight lines which necessarily pass through the origin. Such, however, is no longer the case for the curves relating to the base of the tooth, the initial portion only of which is straight. It is, of course, obvious that if it had been possible to push the induction far enough, the curves for the top and middle of the tooth would have commenced bending towards the horizontal axis in a manner similar to that of the curve for the base of the tooth—in fact, this tendency is clearly marked in the case of the dotted curves of Figs. 18 and 20.

CONCLUSION.

The experiments described in the present paper occupied the greater part of two years, and the expense of constructing the apparatus and photographing the stream-line slides was defrayed out of a research grant made by the Royal Society. We are indebted to Professor E. W. Marchant, D.Sc., and several of his students for valuable assistance in connection with the somewhat laborious experiments which form the subject of our paper.

Professor SILVANUS P. THOMPSON: Prof. Hele-Shaw has made us appreciate the importance of the analogies between magnetic phenomena and those of the flow of liquids. The first things he exhibited to us were the stream-lines, crossing over a shallow circular pond; he showed us how they went across in straight lines, and he depicted to us also from a text-book that old picture which you will find in Faraday's experimental researches of 1831. How Faraday knew—for he drew it—that the magnetic lines went across a circular piece of iron inside, straight from side to side across the section, I do not know; and I do not think anybody knows how Faraday knew it. But Faraday was one of those men who could see a thing instinctively, who had experimented so much, and so carefully, and so honestly, that he could

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see things that he did not himself understand, and could know them when there was apparently no means of knowing them. I know Prof. Hele-Shaw will not object to my saying one word that may seem like criticism—but I must say one word of caution as to taking these analogies too far; because, although magnetic lines do behave very much like the stream-lines of a flowing liquid, they are not the same thing; and they are not flowing. The magnetic lines are not shoving themselves along, nor are they in actual motion along the line; and certainly they do not do what we saw some of these doing: for at the moment when the coloured liquid was poured in, we saw a number of magnetic lines apparently with ends to them, poking their way through the field. No magnetic line has ever had an end or a beginning, and, what is more, magnetic lines do not move endways at all. They are always closed curves; and when they enter a magnetic field they always sidle in—they always move in laterally. This fact I hope to illustrate to you by some other analogies of a much cruder and more mechanical nature put upon the screen presently by the aid of the animatograph. But before I speak of the animatograph, I wish to say a little more about this extremely interesting paper. In the first place, there are certain rules which we know to obtain in the drawing of magnetic lines. And among those rules there is this, that when we are dealing with two substances of such diverse permeability as iron and air, where the ratio between the permeabilities is of the order of thousands, or at the very least of hundreds (and not, as in many of these diagrams, of the order to permeability of about twenty or something of that kind), we have to remember this, that the magnetic lines, where they pass from air to iron, or iron to air, undergo a kind of refraction such that they practically always enter from the air into the iron at right angles to the surface of the iron. If the permeability were lower they would not enter at right angles: they would enter at some other angle dependent upon the direction of the line inside the iron. Whatever the direction of the line inside the iron, it practically comes out of the iron into the air at right angles. There is indeed—I suppose it is known—a sort of law of refraction for the magnetic line which somewhat resembles the law of refraction for a ray of light. But there is this difference, that whereas the index of refraction is the ratio between the sines of the angles made by the ray in the two media, in the case of magnetism the permeability is the ratio of the tangents of the angles. Hence, as the permeability of iron is of the order of one thousand times as great as that of air, the angle in the air between the magnetic line and the normal to the surface must in general be exceedingly small.

The second rule that governs the drawing of magnetic lines is as follows:—Suppose that you are attempting to draw a whole group of lines representing the magnetic flux between two iron faces, as, for example, between the polar surface and the armature surface in a dynamo. Then if, as is usual, you assume these surfaces to be each a surface of magnetic equipotential, and proceed to draw lines across, to represent as best you can the flux, in order by them to estimate the magnetic reluctance of the air-gap, you may be sure that at the best

Professor Silvanus Thompson.

your estimate will be a rough one, whether you draw the lines straight or curved. Nevertheless, if you draw a number of alternative sets of lines and calculate out the reluctance in this way several times, you may always be sure which of them is the more accurate, because the rule is that the magnetic flux always tends so to distribute itself over the field as to make the reluctance a minimum, or the permeance a maximum. Of the three or four trial configurations, that one will be the most correct which gives the highest permeance or the lowest reluctance.

Now following out these principles, it is easy to see that in the case—as illustrated in these beautiful figures of Professor Hele-Shaw and Dr. Hay—where a magnetic line is passing from a point on a pole-piece to a point on the top of a tooth near one of the tooth-corners, it must have a point of inflexion in its curvature. For its general direction across the gap will be oblique, since the flux tends to concentrate itself fan-wise towards the tops of the teeth; and yet, by reason of the law of refraction, it must emerge from the iron pole-face on the one side and enter the iron tooth-face on the other side, almost exactly at right angles.

And now I think I may pass to the question on which I am announced to speak, which has some relation to these problems that Professor Hele-Shaw has studied. I wanted some years ago to make quite clear by diagrams how magnetic lines passed into a magnetic body at the time when it was being magnetized. For instance, you have a horseshoe magnet with its magnetic poles, and you have a piece of iron which is going to be put as a keeper across the poles. When it is there it is magnetized; when it is at some distance away it is not magnetized. How do the lines get into it as it moves up? I wanted to illustrate the fact that the magnetic lines are not stuffed in endways as pins into a pincushion, but come in sideways into the thing which is being magnetized. They do so in all cases. Further than that, there was a very important problem which some ten years ago some people regarded as not quite settled. In the case of the armature of a dynamo having the conductors embedded in slots, we knew that somehow or other the teeth between the slots protected those conductors in a way in which they were not protected in smooth-core armatures. We knew—at any rate, I for one knew, for I had a controversy with some people about it—that in such cases the drag on the armatures came on the teeth and not on the conductors—that the conductors were practically protected from any mechanical drag due to the magnetism, and that the drag came on the teeth. But there was this difficulty raised. If the magnetic lines go down through the teeth, and leave the slots comparatively free from magnetic field, the conductors therefore are practically protected, and are not in a field. Then how on earth could they generate electro-motive force by moving round? If the Faraday principle were true that electro-motive force were induced by the wire cutting the magnetic lines, and the wire was protected from the field by being put down in a slot between iron teeth that drew the lines into themselves, how could the wires when the armature revolved cut the magnetic lines? The answer to that was that it was true that they are

Professor Silvanus Thompson.

protected, and lie in a place where there are comparatively few magnetic lines. There are some few lines which go down into the slots, but the magnetic lines as the teeth move round snap across quickly from tooth to tooth, and they do not remain long in that position. They may pass comparatively slowly across from one corner to the other as the teeth go by, and then they suddenly snap across the space to the next tooth, and therefore there are very few in that space. I remember Mr. Swinburne pointing out that this had a kind of analogy with a certain optical phenomenon. In the old-fashioned window-panes the glass in the middle is sometimes made up into a lump of glass, a knot of glass. Now if you look through the window near one of those knots or lumps of glass, and watch some vertical object moving behind it, you see the object at first on one side of the knot slightly distorted; but as it moves on a little it seems suddenly to disappear, and then reappear at the other side of the knot, as though it snapped across. So do the magnetic lines snap across from tooth to tooth. I wanted to show that effect also. Professor Hele-Shaw and his colleague, Dr. Hay, have not yet exhausted the resources of the laboratory, and I hope they will show us next time they come here not only the stream-lines from side to side, but also those teeth moving up, going past the pole, and illustrate to us how the stream-lines snap from tooth to tooth as the teeth go by. I am going to show you this, not in their beautiful way, but by means of coarse diagrams put on the screen by the animatograph. I ought to tender my thanks to Mr. Paul for having so kindly brought his instrument here in order to show the films. Those films were made by myself and Mr. Dennis Coales, now of the University College, Nottingham, in the following way: We got a number of cards, each of them about the size of a piece of foolscap paper. These cards had two holes punched in the corners of them, so that they could be put upon two pegs so as to register exactly. We had series of eighty cards, or a hundred in some cases. On these we drew as best we could, after calculating them, the forms of the magnetic lines. We drew diagram after diagram of the magnetic lines, in different positions going from stage to stage. Some of these series required more than eighty successive pictures, and they had to be so arranged that in each series the cycle of changes should be complete, so that the last picture should come the same as the first, in order that the phenomena might be exhibited over and over again. When we had got a series drawn out, we then sent it to Mr. Paul, who photographed the diagrams one after another on animatograph films, that they might be put through the lantern, exactly as though they had been taken by a camera from the moving magnetic apparatus.

The first film is a mere illustration of a cross-section of an alternating current. The magnetic lines belonging to it are concentric circles. As the current grows, the circles widen out; as the current dies away, the circles all come back again; they then go out and in again.

Film II. illustrates a permanent steel magnet with a piece of iron dragged up to it. The magnet is shown with its lines of force spreading out into space. Then the piece of iron is pulled up.

Presently a hand comes and pulls the keeper off, and the magnetic lines spread out from the magnet once more into the air. The object of making that diagram was, of course, to emphasise the point that magnetic lines when they enter a body enter sideways, that they come in flankwise into the piece of iron, and permeate through it. Professor Silvanus Thompson.

Film III. deals with a dynamo problem, representing the flux of lines passing from a pole-piece into a toothed armature. I am quite ashamed of this film, because I made the drawings before I had before me the investigation of Professor Hele-Shaw and Dr. Hay as to the actual forms of the curves. Professors Hele-Shaw and Hay referred to the distorting effect on the flux within field magnet. This also is taken account of in my drawings. You will see the lines snap across from tooth to tooth: and you will also see how the bunching of the lines in the gap extends slightly back into the pole-piece, where the oscillations in the distribution of the flux will set up eddy-currents.

Film IV. represents the same dynamo with some windings on its armature. The previous diagram was simply an unwound tooth armature. If there is a winding, and that winding is carrying a current, the current thus carried by the wires in the slots distorts the magnetism of the field magnet, and these lines, instead of going straight across the gap into each tooth, are distorted. The resultant field drags upon the teeth and tends to drag the armature back. That is also shown in this film.

Film V. illustrates an electro-magnet supplied with an alternating current. A straight bar of iron is surrounded by a coil of only four turns, shown in section. When you turn the current on into the wire, the magnetic lines begin, as they did with the alternating current, as circles round each wire. They emerge like ripples from the wire, and then coalesce round the wires in wider curves. Then they spread out and move into the iron sideways; then they spread still further, and penetrate still further in, and more of them spread, always going out from the wires and entering the iron sideways. By and by the iron gets well magnetized; then the current begins to die out again, and the lines quit the iron as they entered it, and collapse back into the wires.

Film VI. has nothing whatever to do with magnetic lines. It shows what cannot be shown very well in any other way, how the oscillations in a Hertz oscillator originate electric waves which it throws off into the space all round.

Mr. ALEXANDER RUSSELL: I congratulate the authors on having brought the hydrodynamical method of mapping stream-lines to such a pitch of perfection. Solutions obtained by this method will be useful not only in magnetic theory, but also in the theories of heat, electrostatics, and the flow of electricity in a plane. In particular, they might be used for finding the stresses in the dielectric of a three-core cable—*i.e.*, the potential gradient at various points in the dielectric, a problem which does not lend itself easily to mathematical treatment. The useful table which the authors give for the fringing coefficient c when the armature is smooth and the poles are not too narrow, would Mr. Russell.

Mr. Russell. be more instructive if they had tabulated c for values of r less than 4. For example, c vanishes when r is nearly 1·1 and becomes negative for smaller values of r. The formula given, to enable a correction to be applied to the formula for the air-gap reluctance so as to take account of the slots in the armature, is really only applicable to the case of a single slot of infinite depth. It can be seen, by examining the figures 1–9, that very few of the stream-lines penetrate far into the slot, and so the formula applies with considerable accuracy to the hydrodynamical cases considered. The formula can easily be deduced from the formula given by Prof. J. J. Thomson in "Recent Researches in Electricity and Magnetism" for the case of the analogous electrostatic problem. The agreement between theory and experiment, in this case, is exceedingly satisfactory. The following extension of an electrical method, suggested by Mr. F. W. Carter,* gives a complete solution of the problem of finding the reluctance of the air-gap of a dynamo, when the problem can be regarded as being in two dimensions, and the distorting effect on the field produced by hysteresis and eddy currents, when the rotor is revolving, can be neglected. To this end, a sheet of tinfoil should be cut out to the shape shown in the figure. DLME represents to scale the outline of the pole and pole-piece, AB the armature, and CD and EF half the distance between adjacent poles. This piece of tinfoil should then be pasted on a plate of glass. The position of the armature has been so chosen that the lines of force on each side of the pole are symmetrical. Copper strips, connected with an accumulator (see Fig.), are then placed over CD and EF. The equipotential lines on the tinfoil can be plotted out easily by Kirchhoff's method. AL and BM are the equipotential lines through A and B. It follows at once from the general mathematical equations, that the equipotential lines in the current problem are the lines of force in the magnetic problem, and the lines of equal magnetic potential in the magnetic problem are the stream lines of current in the electrical problem. The equipotential lines at P and Q are practically parallel to CD. The number of equipotential lines between P and Q is proportional to the difference of potential between P and Q, and can be found by the potentiometer method indicated in the figure. This method was described in the classical paper by Prof. G. Carey Foster and Sir Oliver Lodge which was read to the Physical Society nearly thirty years ago.† The points p q, l and m are at the same potential as the

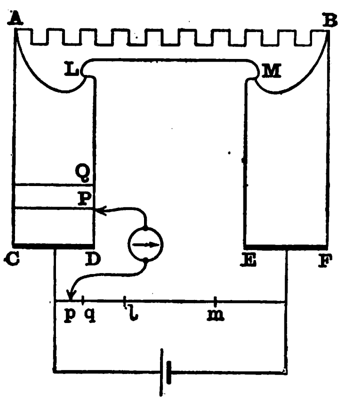

Fig. A.

* *Electrical World and Engineer* of New York, vol. xxxviii., p. 884.

† "On the Flow of Electricity in a Uniform Plane-Conducting Surface," Part II.; *Proceedings of the Physical Society*, Feb. 27, 1875.

points P, Q, L and M. The reluctance R of the air-gap, per unit thickness of pole, parallel to the axis of rotation can then be found from the formula $R=(c/a)\times pq/lm$, where $a=$ the length of PQ, $c=$ CD—that is, half the distance between adjacent poles—and pq and lm denote the lengths of the potentiometer wire between the points p and q, and l and m respectively. If the breadth of the pole, in centimetres, parallel to the axis of rotation be b, then, the reluctance will be R/b. It is to be noted that the size of the piece of tinfoil is immaterial. It has, however, to be made accurately to scale. In this manner it is easy to find the effect on the reluctance of slight alterations in the shape of the pole-piece. The flux-density at every point can also be found at once, not only on the surfaces of the pole-piece and the armature, but also at every point in the air-gap. Thus, in alternators, the armatures of which have a simple bar winding, the form-factor of the wave of E.M.F. on open circuit, can be found and the effective voltage predetermined in certain cases. By a slight extension of the method—namely, by cutting out in tinfoil a representation of the complete air-gap of a machine—the variations of the reluctance of the air-gaps due to differences in the relative positions of the stator and rotor can be ascertained. [The proof of the formula for the reluctance depends on the reciprocal relations between equipotential lines and stream lines. Let us suppose that the tinfoil is of unit thickness. If R_1 is the resistance to electrical flow from AL to BM, and R_2 be the resistance to electrical flow from LM to AB when LM and AB are lines of equal potential and AL and BM are stream-lines, then we have $R_1 R_2=\rho^2$, where ρ is the resistivity of the tinfoil. Again we have Mr. Russell.

$$R=\Sigma l/\mu s \text{ and } R_2=\Sigma \rho l/s.$$

Therefore, the reluctance can be deduced from R_2 by merely writing $1/\mu$ for ρ, and thus, since μ equals unity in our case, and R_1 can be found easily by Ohm's law, it is found that $R=(c/a)\times pq/lm$.]

Resumed Discussion at Meeting of December 8th, 1904.

Dr. E. W. Marchant: It is with a certain amount of diffidence that I propose to say anything on the paper that we heard a fortnight ago. One feels that every one taking part in it then reached such a high standard of excellence, both as regards clearness of explanation and brilliance of demonstration, that it is necessary to maintain that standard to-night. There are three points, however, to which I should like particularly to refer. On p. 24 a table is given showing the values of the correction coefficients necessary in order to take into account the fringing effect at the corners of the pole-pieces. There is one point there which is of a certain amount of interest, and that is, that when the distance between the pole-pieces becomes large the lines of force will leak, not simply from the exact tips of the poles, but from points further up the flanks of the poles on to the surface of the armature. Dr. Marchant.

Dr. Marchant.

The lines of force will leak from the point A down to a point B between the poles, and the result will be that if we are dealing with direct-current machines where the commutation is effected nearly under the pole-tip, as shown in the diagram, the effect of these lines in producing electromotive force will be negligible. The line will come in at B, and will cut the conductor *there* and produce a certain E.M.F. ; it will leak out at C, will cut another conductor and produce a similar E.M.F., but in the opposite sense, with the result that the total E.M.F. produced in the circuit will not be altered. So that as far as the correction coefficient goes, we have only to take into account in direct-current machines the leakage that takes place betweeen the pole-tips and the point where the brush effects commutation, *i.e.*, in line DD. The same thing holds for alternators with distributed winding.

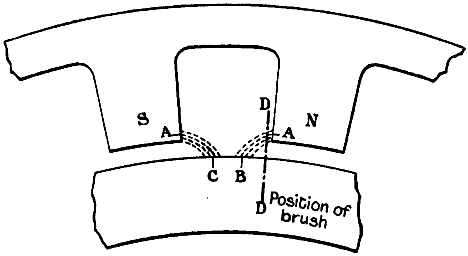

Fig. B.

The second point is in connection with the application of the very beautiful stream-line diagrams which we saw last time. I think they are particularly valuable in showing the path of the flux through teeth of rather irregular shape. It is of course obvious that in the first instance it is necessary to take the more simple form of tooth with straight sides ; but the method, I think, should be of very great value in demonstrating to dynamo designers the exact path of the lines of induction through teeth that are anything but straight, and thus enabling the correction coefficient for the gap to be determined for these teeth. Unfortunately there is one trouble—a difficulty which has been referred to in the paper—that when you get to the very high flux densities of 140,000 to 150,000 lines per sq. inch that are reached in American machines the variation in permeability of the iron comes into play, and this it is difficult to take into account by the stream-line method. Lastly, there is one point in connection with the non-uniformity of the distribution of the flux over the pole-face due to the presence of the teeth. In my laboratory, during the last six months, my demonstrator, Mr. Worrall, and a post-graduate student, Mr. Wall, have obtained very interesting results on this very point. They find quite extraordinary values for the E.M.F. induced in wires that are placed along the pole-pieces (*i.e.*, stationary wires fixed to the pole-pieces), as compared with the E.M.F. produced in corresponding wires placed on the surface of the armature. The actual value of the E.M.F. induced in a coil placed on a fixed pole with an armature having semi-closed slots, and with slot-width at top about equal to the width of the air-gap, is of the order of 20 per cent. of that placed on the moving armature. Further, they have found that the value of this E.M.F. induced is affected very largely by the current flowing in the armature. It is very largely increased when the current in the armature is in phase

with the applied pressure, it is reduced when the current lags, and it is increased still more when the current leads. These results will be communicated to the Institution, and I venture to hope may appear in the form of an original communication to the Journal. Dr. Marchant.

Mr. C. C. HAWKINS: Apart from the scientific interest of the present paper and the admirable way in which we have seen it illustrated, there is one feature to which attention might be drawn. We read in the last paragraph that the experiments extended over the greater part of two years, and that part of the expense was defrayed by a grant from the Mr. Hawkins.

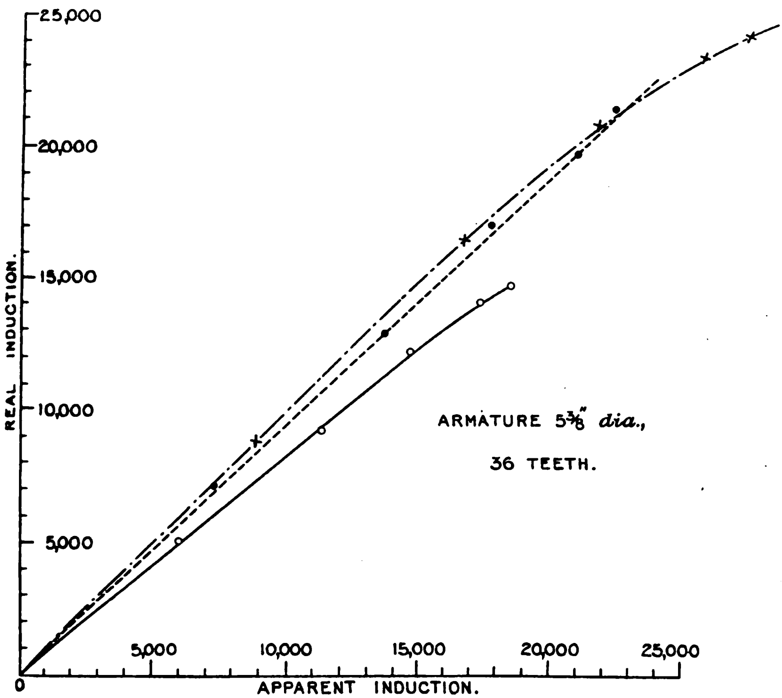

FIG. C.

Royal Society. I think that this emphasises the extreme importance of the endowment of research through the medium of our technical universities and colleges. The subject, although very interesting, is not of the first importance to the dynamo designer; yet the time and trouble required to bring to an experimental proof the results already calculated mathematically by Mr. F. W. Carter would have been beyond the means of any private experimenter.

Perhaps I may be allowed to refer for one moment to Mr. Carter's original communication. He there* compares, in a particular instance, the rough approximation which Mr. Wightman and I gave for the

* *Journal Institution Electrical Engineers*, vol. xxix., p. 933.

Mr. Hawkins.

interpolar fringe from the edge of a pole with his own more correct mathematical expression. But the comparison is hardly fair to the former, since the fraction of the length of the air-gap which is to be added to the width of the pole-face is given as 1·76 on our basis, as against his own 1·07. But in his case the edge of the pole is assumed to make a right angle with the armature surface instead of being inclined to it, and on this assumption our approximate figure becomes 1·96. But then, further, Mr. Carter's figure requires to be multiplied by 2 in order to take into account both edges of the pole, which has

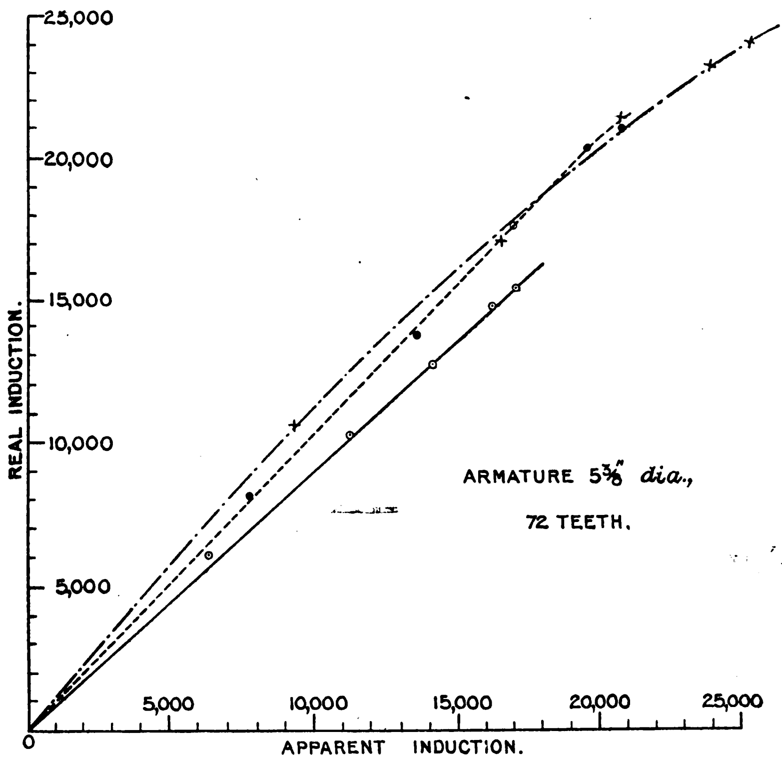

FIG. D.

already been done in the approximate figure, so that the true comparison is between 1·96 and 2·14. As a matter of fact, in the expression $(\xi a + l_g)$ for the length of path of the fringe which was the basis of our approximation, where $\xi = k \times$ the angle between the pole-edge and the armature expressed in circular measure, the coefficient k for any distance away from the pole between twice and thirteen times the air-gap only varies from 0·78 to 0·93 in order to obtain the true value, so that for ready calculation of the induction close to a pole-edge, if the average figure of 0·9 is taken for k, a very fairly correct result is obtained.

Figures 16–20 of the present paper, although not of such æsthetic

beauty, are yet of great interest, and, I think, would convey more information if converted to express the relation between the apparent induction (on the assumption that all the lines pass through the tops of the teeth) and the real induction as given in Table III. On the ordinary assumption that the lines pass down the teeth in parallel straight lines, and that on each side of a tooth there is a certain fringe also passing downwards parallel to those in the tooth, the curves connecting apparent and real induction for various ratios of width of tooth and slot fall in the order of the depth from the top of the tooth at which the section is taken. But the actual curves obtained by transforming the figures of the paper in the way suggested above do not fall in this symmetrical order; the curve for the top of the tooth is always the lowest, and the curves for the centre and bottom sections cross one another at a certain degree of saturation. It is thus evident that the assumption that is usually made in working out the ampere-turns required over the teeth is at best only a makeshift. Mr. Hawkins.

(*Added.*)—At the suggestion of Dr. Hay, the curves given in Figs. 17 and 19, when converted to represent the connection between the apparent and real inductions are appended in their modified form (see Figs. C and D). The fact that some of the curves for moderate inductions lie above a line at 45° to the horizontal is to be explained by the difficulty in measuring the area of the air-gap coil with a high degree of accuracy, and this difficulty was greatest in the case of Fig. 19, where there are 72 teeth, corresponding to a very narrow width of the search-coil.

Dr. W. G. RHODES (*communicated*): The paper which the authors have given us is undoubtedly a most valuable contribution to literature bearing upon the design of dynamos. The diagrams 3 to 9 give a most vivid picture of magnetic leakage in a toothed armature. For the paper to have its greatest value it is necessary for it to appeal as strongly as possible to designers of dynamos. For this purpose I think it would be useful to add a Table IV. calculated from Table III., in which the magnetic flux and magnetic induction at the base of the tooth are represented by unity and those at the middle and top of the tooth by fractions of unity. A glance would then suffice to show the percentage leakage for different induction densities. Dr. Rhodes.

A close scrutiny of Table III. makes it appear that the figures with an asterisk are not the only results which may be too low. It is a great pity that there should be any doubt as to the accuracy of this table, it being the most important part of the whole paper.

Mr. F. W. CARTER (*communicated*): Designers of electrical machinery are much indebted to the authors of this paper for their painstaking investigation of a difficult subject, and all interested in magnetism will admire the beautiful diagrams of lines of magnetic force with which the paper is illustrated. Personally I wish to thank the authors for their appreciation of my own work on this subject, which I had imagined had been passed unnoticed by this practical world, although I was aware that my conclusions were perfectly reliable so far as they went. Mr. Carter.

It is a mistake to suppose that Professor Hele-Shaw's stream-lines are mere analogies to the magnetic lines of force, in the sense in which

Mr. Carter. we usually use the word, as for instance when we regard the flow of water in a pipe as analogous to the flow of electricity in a wire, and the head of water to electromotive force. They are analogies so close that all calculations made on them will apply equally to the ideal magnetic cases which they represent. The sum total of all experimental knowledge of magnetism is embodied in certain laws represented by appropriate mathematical expressions. Professor Stokes has shown

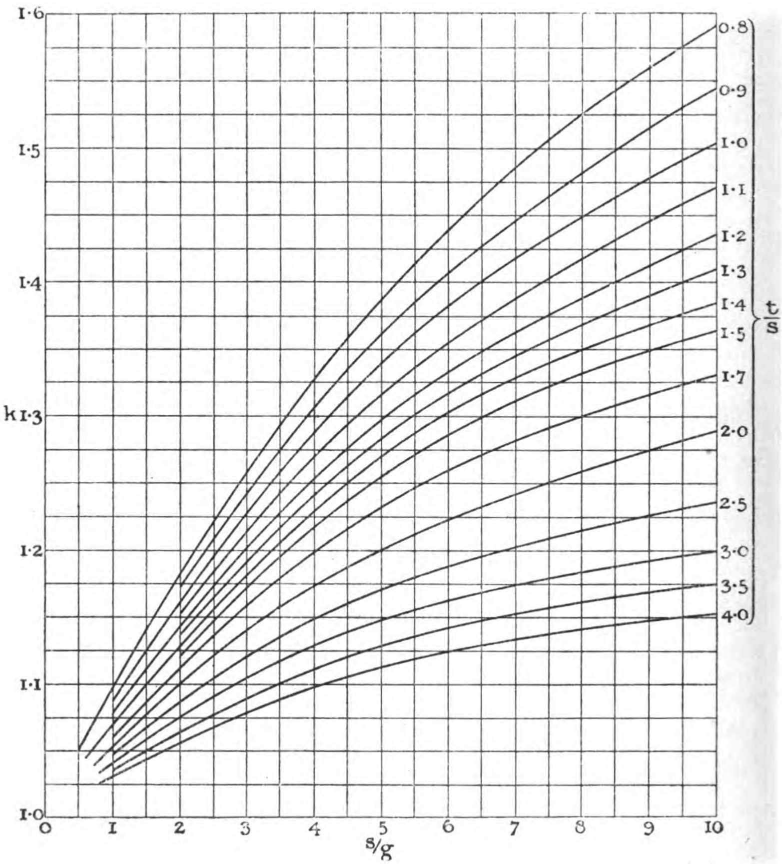

FIG. E.

that the motion of a viscous fluid in a thin film between two near plates must be investigated by means of the same mathematical analysis. The stream-line problem is therefore not merely analogous to the corresponding magnetic problem, but is mathematically identical with it. The great value of Professor Hele-Shaw's work lies in the fact that it leads to exact knowledge—if only of certain ideal cases.

If I may pass a criticism on the paper, it is that the results are not expressed so as to render them immediately available for practical use,

Mr. Carter.

Table I. gives the correction to be applied to find equivalent air-gap in certain specific cases, and other cases can only be estimated by interpolation. I venture to supply a system of curves (Fig. E) which will immediately give the correction factor to take account of slots and teeth, including the most usual cases. The curves are calculated from the formulæ quoted by the authors on page 25 of their paper, and are substantially the same as those which appeared in the article referred to by the authors.

I would like to suggest that Professor Hele-Shaw applies his stream-line method to determine the equivalent air-gap when both pole-face and armature are slotted, as for instance in the induction motor, or the alternating-current railway motor. I see that the experiment would be a very difficult one, which would have to be made for several different relative positions of armature and field slots. The only case in which I perceive an approximately correct solution for such a problem is when one set of slots is narrow, as in an induction motor with partly closed rotor slots (Fig. F). In this case the multiplier giving the apparent air-gap is $(s + t)/(s + t - \sigma s - n \sigma' s')$, where (using the same notation as the authors of the paper) s, t, and σ apply to one set of slots, s' σ' to the other set, and n is the number in the second set for each one of the first. The case of two equal slots when directly opposite to one another follows from the case of a single slot, and the appropriate multiplier can be obtained from the curves of Fig. E by taking g, in the abscissæ, to be one-half of the actual gap.

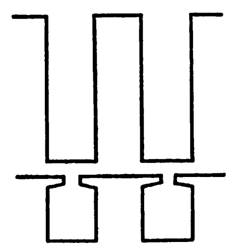

FIG. F.

It is worthy of note that the calculation giving the effect of a slot serves immediately to give the effect of a ventilating duct in the armature. If s in the formula of page 25 is the width of the duct instead of that of a slot, the effect of the duct on the reluctance of the air-gap is the same as if the length of the pole were diminished by an amount σs.

The variation of flux density at the pole-face, which is plainly seen in several of the diagrams of the paper, is the cause of a loss of energy by eddy currents which can easily be calculated. Calling the mean flux density B (in lines per square inch), I find the amplitude of the variation to be* —

$$\frac{1}{2} k \mathrm{B} \left(1 - \frac{1}{\sqrt{\left(1 + \frac{s^2}{4g^2}\right)}}\right)$$

and the mean loss in watts per square inch of pole-face—

$$2 \times 10^{-13} k^2 \mathrm{B}^2 \left(1 - \frac{1}{\sqrt{\left(1 + \frac{s^2}{4g^2}\right)}}\right)^2 d^2 r^{\frac{3}{2}} n^{-\frac{1}{2}} \mu^{-\frac{1}{2}},$$

where k is given by the curves of Fig. E, d is the diameter of the arma-

* *Electrical World and Engineer*, vol. 38, p. 885.

Mr. Carter. ture in inches, r its speed in revolutions per minute, n the number of armature slots, and μ the permeability of the iron of the pole-face at the density at which it is being worked. This calculation of course applies only to solid pole-faces.

Mr. Goldschmidt. Mr. RUD. GOLDSCHMIDT (*communicated*): With reference to the subject of air-gap correction-coefficients dealt with in the paper by Professor Hele-Shaw, Dr. Hay, and Mr. Powell, I should like to suggest that account be taken of the influence of the slots by adding an additional air-gap c to the actual air-gap g, instead of multiplying the latter by a coefficient: Apparent air-gap $= g + c$. The additional air-gap c is greater the wider the slot, and I found that it can be considered independent of the actual air-gap in sufficiently wide limits and for practical purposes. If $s =$ slot width; $p = s + t =$ slot pitch, then:

$$c = \text{Const.} \times \text{slot width} \times \frac{\text{slot width}}{\text{slot pitch}}.$$

$$c = \mathrm{C} \times \frac{s^2}{p}.$$

The coefficient C was found to be about 0·23 as an average of a large number of actual tests. According to Table I. of the paper, the coefficient C varies between 0·12 for very small air-gaps and 0·17 for very large ones.

Dr. Hay. Dr. ALFRED HAY (*in reply*): Very few remarks will, I think, be sufficient by way of reply to the comments on the paper. In connection with the remarks made by Professor Thompson, I should like to emphasise the fact that the diagrams which are generally given of the magnetic-flux distribution in most text-books appear to indicate that the authors are either ignorant of the law of refraction for the magnetic lines, or else deliberately ignore it. In a number of books which I have examined recently, books published in the course of this year, I find that a large number of diagrams are given in which no attempt is made to show the refraction of the lines as they pass from air into a mass of soft iron. We hope that one good effect produced by our paper will be the disappearance of diagrams of this class from text-books. With regard to Mr. Russell's remarks on the method which is referred to by Mr. Carter, the method which depends on the plotting of the equipotential lines by means of the galvanometer in the usual way, although this method may be made to yield valuable results, yet, as compared with the stream-line method, it is an exceedingly laborious one, and I think any one who has attempted the plotting of equipotential lines by that method will agree with us in maintaining that the method is too laborious for ordinary use. Professor Marchant's remarks with regard to the differences in the E.M.F.'s induced in a coil which is on the surface of the armature and a coil placed against the pole-piece are most interesting, because they show that the rotation of the armature causes a considerable swaying of the lines inside the pole-piece. Referring to Mr. Hawkins' remarks, there would be no difficulty in plotting the curves connecting the maximum induction with the mean induction. The method of calculating the reluctance of the

air-gap and teeth referred to by Mr. Hawkins is that used by Parshall and Hobart in their treatise on electric generators, but I think that, as Mr. Hawkins himself has pointed out, a reference to the curves given in our paper will show that this method is by no means a legitimate one. We hope to be able to extend the method to the irregular shapes of teeth which are employed in connection with induction motors. In this case an exact knowledge of the air-gap reluctance is of particular importance, as the behaviour of an induction motor is largely determined by the reluctance of its air-gap. In conclusion, I should like to thank you all on my own behalf and that of my colleagues for the kind way in which the paper has been received. Dr. Hay.

[*Communicated.*] In reply to Dr. Rhodes, who draws attention to some inaccuracies in Table III., we may point out that the inaccuracies refer solely to the values of the mean gap induction. There is *no doubt* about either the relative or absolute values of the flux through the various cross-sections of the tooth. In the case of the gap search-coil, however, great difficulty was experienced in measuring the width of this coil with a high degree of accuracy. Hence the values given for the mean gap induction must be regarded as only approximate.

The set of curves supplied by Mr. Carter, and the formula given by him for the loss in the pole-pieces due to the swaying of the lines, should prove extremely useful to the dynamo designer.

With reference to Mr. Goldschmidt's interesting communication, the fact that his coefficient C has, according to our Table I., values ranging from 0·12 to 0·17, would appear to indicate that the method proposed by him can hardly be considered satisfactory except in cases where only very rough values are required. The value 0·23 given by Mr. Goldschmidt is much higher than any contained in our table, and errs on the side of safety. Mr. Goldschmidt does not state whether the 0·23 includes the effect of ventilating gaps; this latter is not, of course, included in the values given in our Table I., but may be determined by the method explained by Mr. Carter in his contribution to the discussion.

Prof. H. S. HELE-SHAW (*in reply*): I think, from the Press reports, that there was a certain amount of misunderstanding in connection with my account of the stream-line discovery at the last meeting. Perhaps it will be worth while to state that the mathematical verification by the late Professor Stokes as to the accuracy of my method was set forth in a paper simultaneously with one of my own, in which that method was communicated to the British Association at Bristol.* In this case, as in so many other cases, the experiments were made first. The mathematician afterwards demonstrated that it was what might have been expected. I believe it was not, however, conjectured that it would ever be possible to produce perfect stream-line motion by using a viscous fluid, and that the more viscous the fluid the more perfectly would the stream-line motion accord with the motion of a perfect fluid. There is a remark I should like to make about time expended on these Professor Hele-Shaw.

* This method I had, by the use of mathematical methods, in my papers before the Institution of Naval Architects previously proved to be an exact one. Professor Stokes showed that it might have been expected.

Professor Hele-Shaw.

experiments. It must be understood that for a method to be of practical value, which is required to represent exactly the lines of force under various conditions by the flowing of a viscous liquid, it is highly important that there should be no doubt whatever, before the results are published, as to their accuracy. This is the reason why so long a period was expended in verifying and examining the action of various liquids in order to make perfectly sure that the results were exact and exactly corresponded to the hypothetical lines of force. Once having established this fact, the method is remarkably simple and rapid. As I explained, it only needs the preparation of a layer of paraffin of a certain thickness, the paraffin being free of air bubbles. This latter may seem a small thing, but it happens to be one of those things which experimenters find often baffle them. These air bubbles appeared at first in almost innumerable numbers and obscured the figure. It was only when the sheet of glass was itself boiled in the paraffin for some time, instead of pouring the paraffin on the glass, and then allowing it to cool underneath, that we were able to get the bubbles away. After this we scraped off the superfluous paraffin and then had a clear surface free from air bubbles. Professor Silvanus Thompson, who as we all know is an extremely fertile experimenter, has suggested that it might be possible to make a model in which the armature was actually rotated and he produced figures which would change and demonstrate various points to students, and help the observer to understand what was going on. Thus I think we all felt we understood probably more by his beautiful cinematograph slides of what was actually going on in the neighbourhood of a magnet. I have already made some experiments of this kind, and I hope that may be done, and that every technical school will be able to have a very simple form of apparatus to demonstrate to the students problems of this kind. I am not advocating this to benefit myself personally or my colleagues, because it is open to anybody to construct this apparatus. Mr. Paul, a member of your Institution, is likely to put on the market a very cheap, effective and simple form of this apparatus, so that any one who wishes to examine for themselves the particular way in which these lines are formed may be able to do so. I thank you for the extremely kind—indeed, I may say enthusiastic—way in which you have been good enough to receive this paper and the experiments. It has taken a long time and much work, a great deal of the latter having been done by my colleagues. I was in Africa when they put in my name to this paper, and although I had made many experiments before I left England, it is only fair to say that they are mainly responsible for the details of the present contribution.

The President.

The PRESIDENT: It is my pleasant duty to move a vote of thanks to the authors of this paper. The only thing I have to regret is that I was not here last time to see all the experiments. Still, the discussion to-night and the paper itself gives some idea of the importance of the whole subject, and I think that the members of the Institution who have been present can congratulate themselves on having heard a paper which certainly opens out quite a new prospect of research. As Professor Hele-Shaw himself said, even in other branches of

The President.

applied science the method can be usefully applied. I am sure you would also like to include Professor Thompson's name in this vote of thanks for the excellent way in which he opened the discussion, and for exhibiting so very interesting a set of animatograph films. I have great pleasure in moving a hearty vote of thanks to the authors of this paper.

The resolution was carried by acclamation.

Proceedings of the Four Hundred and Fourteenth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday, December 8, 1904, Mr. Alexander Siemens, President, in the chair.

The minutes of the Ordinary General Meeting held on Thursday, November 24th, were taken as read, and confirmed.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associates to that of Associate Members—

Percival James Robinson. | George Stevenson.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Griffin & Co. and the Scientific Publishing Co., to whom the thanks of the meeting were duly accorded.

Messrs. A. C. Heap and C. C. Hawkins were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Member.

Alfred Clough.

Associate Members.

Bertram H. Lamb.
Charles R. Palairet.
John N. Stephens.
Wilbur H. Williams.

Student.

Arthur J. Langhorne.

The Secretary read a letter from Professor Elihu Thomson, thanking the members of the Institution for his election as Honorary Member.

The discussion on Messrs. Hele-Shaw, Hay, and Powell's paper was concluded (see p. 43), and the following paper was then read :—

STUDIES IN MAGNETIC TESTING.

By G. F. C. SEARLE, M.A.

Cavendish Laboratory, Cambridge.

(*Paper read December 8th, 1904.*)

SUMMARY:—Introduction—Magnetic units—Effect of continued reversals of the magnetic force—Apparent induction and permeability—Demagnetisation by reversals—Normal induction and permeability—The critical magnetic force—Apparent and normal intensity of magnetisation—Magnetic history—Method of demagnetisation—Determination of the normal induction and permeability—Transformer iron used in the experiments—The magnetic square—Tests of the magnetic square—Preliminary magnetometric experiments—Preliminary ballistic experiments—Effects of many reversals of a force X upon the apparent permeability of a smaller force H—Influence of many reversals of a force X upon the after-effects of a force Y—Influence of partial demagnetisation on the after-effects of a large force—Effect of a constant force X on the apparent permeability for a force H—Experiments on virgin iron—Magnetometric study of the action of effective demagnetisation on virgin iron—Ballistic study of the action of effective demagnetisation on virgin iron—Experiments on virgin, demagnetised and magnetised iron—Rowland's experiments—Influence of partial demagnetisation on the permeability of virgin iron—Constancy of the specimens—The preparation of specimens—Want of uniformity of specimens—Conclusion.

APPENDIX:—Mr. Campbell's Experiments.

INTRODUCTION.

§ 1. I was led to make the investigation described in the following paper by noticing, in August, 1902, that the magnetic treatment of specimens of transformer plate may exert so great an influence upon the change of induction subsequently found on the reversal of a given magnetic force as to make ballistic tests of transformer iron liable to great uncertainties. I therefore decided to investigate the effects of magnetic treatment with the hope of finding how to obtain a definite value for the change of induction due to the reversal of a given force, and of gaining some information as to the effects produced by some selected modes of magnetic treatment upon the

change of induction subsequently found on the reversal of a given force.

The investigation has, however, gone a little beyond these limits, for I have, in addition, examined the effects of the mechanical disturbances involved in the preparation of the specimen for testing, and have also considered how far the results obtained with a single specimen may be taken as representative of the magnetic quality of a whole batch of sheets of transformer iron. The work may therefore be looked on as a rough attempt at a critical examination of the use of the ballistic method in magnetic tests of transformer iron.

In ballistic tests of permeability a ring-shaped specimen is uniformly wound with a magnetising coil and is also encircled by a secondary coil. After the magnetic force has been brought to some desired value, H, the magnetising current is reversed and the change of induction due to the reversal is deduced from the throw of a ballistic galvanometer in the secondary circuit.

But the ballistic method, when applied to rings, gives no information as to the actual values of the induction, before and after the reversal, corresponding to + H and − H respectively. Such information, in the absence of a special device,* could only be obtained by measuring and recording, with their proper signs, all the changes of induction which have occurred since the iron was first magnetised—a plan generally impracticable.

The magnetometric method does indeed enable us to find approximately the absolute value of the induction at any stage, when the iron can be tested in the form of a long, thin rod, but this method introduces so many difficulties and uncertainties of its own that it has no place in commercial testing. The ideal plan of using a long ellipsoid is obviously impracticable in the case of transformer sheet.

We are left, then, to make the best use we can of the information, given by the ballistic method, that the reversal of a given magnetic force H produces a known change of induction.

Some results stand out so prominently in my experiments that I have felt justified in employing definite terms in connexion with them. These terms have proved useful in the course of the investigation, and I shall use them in the present paper because they will enable me to give a precise description of the experiments far more briefly than would otherwise be possible. I shall therefore begin with an account of these results, introducing, as I proceed, the definitions of the terms corresponding to them.

Magnetic Units.

§ 2. Throughout the paper I employ C.G.S. units, and I adopt the recommendation† of the International Electrical Congress, which met

* I have shown in §§ 33, 34, how by taking account of the magnetic properties of the iron under test, the actual value of the induction at any stage can be approximately determined.

† *The Electrician*, vol. 45, p. 822.

at Paris in 1900, and use the word *Gauss* to indicate the C.G.S. unit of magnetic force, and the word *Maxwell* to indicate the C.G.S. unit flux of magnetic induction through a circuit. Thus, if the total flux of induction through a circuit at any time be N maxwells, the E.M.F. induced in the circuit is $-\frac{dN}{dt}\cdot 10^{-8}$ volts. The unit for the measurement of the magnetic induction at any point is thus one maxwell per square centimetre. These recommendations have been adopted by the Electrical Standards Committee of the British Association.*

The Congress recommended that the word *Gauss* should be used to indicate an induction of one maxwell per square cm. as well as a magnetic force of one C.G.S. unit, but I regret to be unable to accept this last recommendation, since magnetic induction is physically distinct from magnetic force, though in air the numerical value of the magnetic induction on the Electromagnetic system may be equal to that of the magnetic force which gives rise to it. The recommendation, if adopted, would only lead to confusion.

I am indebted to the secretary to the Congress, Mr. E. Hospitalier, for his kind assistance in the matter of these units.

Effect of Continued Reversals of the Magnetic Force.

§ 3. The ballistic method involves the reversal of the magnetic force acting upon the specimen, and a confirmation of the value of the change of induction due to a reversal of the force can only be obtained by making a second reversal. This at once brings us to an effect which compels us to count these very reversals of the magnetic force, which are necessary for the measurement of the corresponding changes of induction, as part of the magnetic treatment of the iron. For continued reversals of a definite magnetic force generally cause, at least in soft iron, a diminution of the corresponding changes of induction, which at first is rapid but after a few reversals is very slow, one hundred reversals being sufficient to bring the iron to a practically "steady" state for any fresh value of the force.† This result is the basis of the practice, adopted by those who make magnetic tests, of making observations for the change of induction due to the reversal of a given magnetic force only after many reversals of that force. Of course no other magnetic operation must intervene between this series of reversals of the magnetic force H and those reversals of the same force H which are made for the measurement of the change of induction.

In recording many of the experiments described in the present paper I used those values of the changes of induction which I obtained

* *Report of the British Association*, 1900, p. 54.

† Mr. T. G. Bedford and I made a series of direct experiments to illustrate the gradual diminution of the changes of induction due to continued reversals of a given magnetic force. We found that the effect was very much more conspicuous with small magnetic forces than with great ones. See G. F. C. Searle and T. G. Bedford "On the Measurement of Magnetic Hysteresis," *Phil. Trans. Royal Soc.*, vol. 198 A, p. 70.

after 200 reversals of the corresponding forces. In several experiments I determined the change of induction after 100 as well as after 200 reversals, and found that, though the second century of reversals generally caused an additional diminution in the change of induction, yet the effect was small; it seldom exceeded 1 per cent., and was generally much less.

Though a large number of reversals of the force is necessary for obtaining a steady value for the change of induction due to the reversal of a given force, yet the series of reversals will not, of itself, in all cases make the steady value of the change of induction a definite quantity depending only upon the magnetic force employed. To obtain a definite result in all cases, we must subject the specimen to a special preliminary magnetic process, viz., demagnetisation by reversals.

Apparent Induction and Permeability.

§ 4. It will be convenient to give a name to the steady value to which half the change of induction, due to the reversal of a given magnetic force H, settles down after many reversals of that force, and I shall employ the term *apparent induction* * for the purpose, and shall use the symbol B′ for the quantity in question. Extending the nomenclature, I shall give the name *apparent permeability* to the ratio of the *apparent induction* to the corresponding magnetic force, and I shall use the symbol μ' to denote the ratio; thus $\mu' = B'/H$.

Demagnetisation by Reversals.

§ 5. Throughout the work special attention was paid to the demagnetisation of the specimens by the method of reversals.† In this process the specimen is subjected to a large number of reversals of a magnetic force, whose amplitude is slowly and continuously reduced from some maximum value D to zero; thus, during the process the specimen is not subjected to any force outside the limits $\pm$ D. If the maximum amplitude of the force be 10 gausses, we may, for brevity, describe the whole process by saying that we demagnetise the iron with D = 10. The object of the process is to reduce the iron to a standard state such that the apparent induction, B′, subsequently found for a given magnetic force, H, depends only upon that force, and not upon any magnetic operations which may have been done before the demagnetisation. It is to be understood that no magnetic operation is to intervene between the demagnetisation and the test with the force H. With any brand of transformer iron the desired result will almost certainly be obtained, provided D be not less than

* The apparent induction is the same quantity as that which Mr. T. G. Bedford and I formerly called the "mean maximum induction." See *Phil. Trans. Royal Soc.*, vol. 198 A, p. 39.

† Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 46.

10 gausses. The process is then called "effective demagnetisation." As we proceed, it will be found that, for the iron used in the experiments, effective demagnetisation was secured with values of D much smaller than 10 gausses; but for the present it will suffice to say that transformer iron is effectively demagnetised if D be not less than 10 gausses.*

In my experiments the alternating magnetic force required for demagnetisation was always produced by an alternating current, of about 90 cycles per second, and the value of D was ascertained from the readings of an electro-dynamometer. The details of the method are given in §§ 10–12 below.

Normal Induction and Permeability.

§ 6. Since it is only after effective demagnetisation of the specimen that the apparent induction B′ depends only upon H and not upon any magnetic operations performed before the permeability test, for all, even small, values of H, it is clear that we must employ this definite value of B′ when we have to give an account of the magnetic quality of a given specimen. I therefore call the apparent induction found with a force H, after effective demagnetisation, the *normal induction* for that force, and I denote it by B_0; I also call B_0/H the *normal permeability*, and denote it by μ_0.

The details of the method employed in finding the values of B_0 and μ_0 are given in § 13 below.

The Critical Magnetic Force.

§ 7. From the experiments described in the present paper it appears that, corresponding to each specimen of iron, there is a certain critical magnetic force, which I denote by M_0. The importance of at least an approximate knowledge of this critical force may be judged from a brief summary of some of the results obtained in the course of the work.

When H at the least somewhat exceeds M_0, the iron is quite indifferent to its magnetic history, with the result that we find the same apparent induction for a given value of H whether the specimen is in a virgin state and has never previously been magnetised, or whether it has been powerfully magnetised, or, again, whether it has

* "It is clear that this process leaves the piece symmetrically conditioned as regards subsequent magnetisation in either of the two . . . directions. The condition of a piece demagnetised in this way is no doubt different from that of a piece never before magnetised (indeed, the curves of I and H taken before and after the process show that it is somewhat, though not very, different as regards susceptibility); but the neutral state produced by this process forms an exceedingly convenient starting point, to which we may recur over and over again, when we wish to determine the susceptibility or the retentiveness of the same piece under different conditions of stress, temperature, etc."—Ewing, "Experimental Researches in Magnetism," *Phil. Trans. Royal Soc.*, vol. 176, part ii. p. 539.

been effectively demagnetised by reversals, and thus permeability tests for values of H exceeding M_0 are quite simple. But, on the other hand, when H is less than M_0, the apparent induction for a given value of H depends greatly upon the magnetic history, and thus we find, for a given specimen and for a given force, one value for the apparent induction when the iron is in the virgin state, a greater value when it has been effectively demagnetised, while, within limits, we can obtain any value we please by suitably choosing the magnetic history.

If we were given a ring of iron and knew nothing as to its magnetic quality or its magnetic history, the first step towards obtaining the "normal" permeability curve would be to make the following set of observations:—Wind the ring with primary and secondary coils in the usual manner and subject it to 100 reversals of a magnetic force of 0·2 gauss, and find the apparent induction B'. Then increase the magnetic force to 0·4 gauss, and after 100 reversals find B' for $H = 0{\cdot}4$, and continue this process, step by step, until B' reaches 15,000 maxwells per sq. cm., or thereabouts. Calculate the apparent permeability, μ', for each value of H and draw a curve showing μ' in terms of H, when it will be found that μ' rises to a maximum, when H has some value M', and then diminishes. The magnetic force M', corresponding to the maximum of μ', has a nearly definite value for a given specimen, for it is found that its value is but little affected by the magnetic treatment experienced by the specimen before this series of permeability tests. In fact, in my experiments the highest and lowest values of M' did not differ by more than 0·5 gauss in the case of any specimen.

My experiments show that when M' has been found in this way we shall be able to obtain effective demagnetisation by reversals if we make certain that at the least D somewhat exceeds M'. For transformer iron we shall be quite safe if we make D greater than M' by 3 or 4 gausses.

For any brand of transformer plate effective demagnetisation will probably be secured if we make D not less than 10 gausses. We need not then go to the trouble of drawing a preliminary $\mu' - H$ curve.

I shall now define the critical magnetic force M_0, corresponding to a given specimen, as the magnetic force for which μ_0, the normal permeability for that specimen, attains its maximum value. Though, on account of the narrowness of the limits within which M' lies, *any* value of M' is good enough as a guide in practical testing, yet it is convenient, for the sake of precise descriptions, to have a definite specification of the critical magnetic force.

Apparent and Normal Intensity of Magnetisation.

§ 8. In §§ 4, 6, I have defined the apparent induction and the normal induction, and now I shall explain what I mean by the two analogous terms *apparent intensity of magnetisation* and *normal intensity of magnetisation.*

The *apparent intensity of magnetisation* is the steady value to

which half the change of intensity, due to a reversal of a given magnetic force H, settles down after many reversals of that force; I use the symbol I′ to denote it. The ratio of I′ to H is called the *apparent susceptibility* and is denoted by κ'.

The *apparent intensity of magnetisation*, found after effective demagnetisation, for a given force H, is called the *normal intensity of magnetisation* for that force, and is denoted by I_0. The ratio of I_0 to H is called the *normal susceptibility*, and is denoted by κ_0.

Magnetic History.

§ 9. The fact that effective demagnetisation by reversals enables us to obliterate completely the after-effects of any magnetic treatment, to which the iron was subjected before demagnetisation, is exceedingly important in the study of the magnetic properties of iron; indeed, but for this fact, we should find it practically impossible to obtain any definite results with transformer iron which had once been magnetised. But by effective demagnetisation we are enabled to reproduce a definite state of the iron to form the starting point for each experiment, and are thus able to study the effects of any magnetic operations done *after* the demagnetisation without any disturbance arising from magnetic operations done *before* the demagnetisation. It is therefore permissible to use the words *magnetic history* to indicate the magnetic treatment to which the specimen has been subjected after effective demagnetisation and before the series of reversals of the force H necessary for reaching a steady value for the apparent induction for the force H. I shall use the words *magnetic history* only in this sense; in some cases it will suffice to use the single word *history.*

On account of the effects of "ageing," if we wish to study the after-effects of any particular magnetic treatment, we must take care that the apparent induction is measured so soon after the effective demagnetisation that the effects of "ageing" are insensible.

Each series of my experiments occupied two or three days at most. As the iron was subjected only to the temperature of the laboratory, I may fairly hope that no serious error has been introduced by any possible "ageing" of the specimens. This point must be borne in mind in all magnetic testing, for, even at the temperature of an ordinary room, "ageing" may, in the case of some specimens, cause a serious loss of permeability and a corresponding increase in the hysteresis loss for cycles with a given range of induction.* In the case of two specimens, however, I verified that the normal permeability curves remained unchanged for several months. (See § 57.)

Method of Demagnetisation.

§ 10. In demagnetising the iron by reversals, I used an alternating

* Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 193. G. Stern, "Über das Altern deutscher Eisenbleche," *Electrotechnische Zeitschrift*, vol. 24, p. 407. See also a letter from Prof. Ewing on "The Standards in Ewing's Hysteresis Tester," *The Electrician*, vol. 49, p. 684.

current, derived from the Cambridge town supply, to give the necessary reversals of the magnetic force; I thus obtained about 90 cycles per second. I diminished the amplitude of the alternating magnetic force very gradually by means of a liquid resistance slide. This consisted simply of a narrow glass trough containing dilute $CuSO_4$ solution and furnished with two copper electrodes, one of which was movable; by tilting the trough so that one end was dry the current could be reduced to a very small value before the circuit was actually broken. A commutator allowed either the continuous current from storage cells or the alternating current to pass through the magnetising solenoids. This method of demagnetising the iron has the advantages of being speedy and of reducing the amplitude of the magnetic force in the very gradual manner which is necessary if the demagnetisation is to be really successful.

§ 11. To estimate the maximum amplitude of the magnetic force, I measured the "effective" current* in some cases by means of an ampere-meter made for use with alternating currents, and in some other cases by means of a sensitive electrodynamometer, which I standardised with a Weston continuous-current ampere-meter. I had no means of finding the "wave form" of the current, and I had therefore to be content to treat the current as a pure sine function of the time and to take the maximum current to be $\sqrt{2}$ times the effective current.

For the "magnetic square" (§ 15) the magnetic force is 75·15 gausses per C.G.S. unit current [10 amperes], and hence, in this case, if the maximum amplitude of the alternating magnetic force be D gausses and the maximum "effective" current be C units on the C.G.S. system, then

$$D = 75 \cdot 15 \times \sqrt{2}\, C = 106 \cdot 2 \times C.$$

§ 12. The electrodynamometer employed for estimating the maximum "effective" current was the instrument which Mr. T. G. Bedford and I described in our paper "On the Measurement of Magnetic Hysteresis."† In the present experiments the alternating current, which was to be measured, passed round the fixed coils of the instrument and also traversed a pair of conductors R and S, arranged in parallel. One of these, R, was a coil of $\frac{1}{10}$ ohm, and the other, S, was made up of the suspended coil connected in series with a suitable resistance taken from a box; the total resistance of S varied from a minimum of 76 to several thousand ohms. By varying S a wide range of currents can be measured with the one dynamometer. To standardise the instrument a measured continuous current was substituted for the alternating current.

Let L be the coefficient of self-induction of the suspended coil and M the coefficient of mutual induction between the suspended and the fixed coils. Then, if the current in the fixed coil be $I \sin pt$ and if γ be

* The "effective current" is the square root of the time-average of the square of the current.

† *Phil. Trans. Royal Soc.*, vol. 198 A, p. 54.

the steady current, which produces the same deflexion as the alternating current of the "effective" value C or $I/\sqrt{2}$, then

$$C = \gamma \left[1 + \frac{L^2 p^2}{(R+S)^2} \right]^{\frac{1}{2}} \left[1 - \frac{LMp^2}{R(R+S)} \right]^{-\frac{1}{2}}$$

I had $R = 10^8$ cm. sec^{-1} [$\frac{1}{10}$ ohm], $R + S = 7{\cdot}3 \times 10^{10}$ cm. sec^{-1} [73 ohms] at least, while by Maxwell's method, Mr. E. Smart and I found $L = 1{\cdot}3 \times 10^6$ cm. [$1{\cdot}3 \times 10^{-3}$ henry]. The mutual coefficient M depends upon the relative positions of the two coils but is always positive, since the suspended coil is so deflected that the lines of force due to both currents pass through it in the same direction; we have approximately

$$M = 2\pi^2 Nn\,(a^2/A) \sin\theta,$$

where A, a and N, n are the mean radii and the numbers of turns on the fixed and suspended coils and θ is the deflexion. In the dynamometer, $A = 5{\cdot}0$ cm., $a = 1{\cdot}7$ cm., $N = 500$, $n = 190$, while $\sin\theta$ never exceeded $\frac{1}{5}$. Hence M was always less than $2{\cdot}2 \times 10^5$ cm. [$2{\cdot}2 \times 10^{-4}$ henry]. The alternating current made about 90 cycles per second, so that $p = 2\pi \times 90 = 5{\cdot}6 \times 10^2\ \text{sec}^{-1}$.

In the most unfavourable case, in which S has its least and θ its greatest value, C does not differ from γ by more than about 0·6 per cent., and when S exceeds 1,000 ohms the corrections become quite inappreciable.

The deflexion of the suspended coil was observed by aid of a lamp and scale; by so setting out the scale that the distance of the nth division from the zero was proportional to n^2 we were able to take the scale reading as proportional to the effective current.

DETERMINATION OF THE NORMAL INDUCTION AND PERMEABILITY.

§ 13. The normal induction B_0 and the normal permeability μ_0 were very frequently found in the course of the work, and so, to avoid repetition, I shall describe, once for all, the method employed in determining them. Having settled upon the magnetic force H, for which B_0 was to be found, I adjusted the continuous current, by means of a rheostat, so as to obtain the desired force. By means of a two-way key, I then stopped the continuous current and allowed the alternating current to flow through the magnetising coils. After bringing this current to its full strength, I gradually diminished it to (practically) zero by means of a liquid resistance slide (§ 10), so demagnetising the iron. In order to obtain effective demagnetisation (§ 5) I took care that D, the maximum magnetic force due to the alternating current, exceeded M_0, the critical force corresponding to the maximum normal permeability (§ 6).

By putting the two-way key back into its first position, I stopped the small residual alternating current and re-started the continuous current, and corrected by the rheostat, if necessary, any very slight deviation from the value required to give the chosen magnetic force H.

After the force H had been reversed many times, the number varying from 50 to 200, the ballistic effects of at least one pair of reversals of H were noted. From the mean of these effects the value of B_0 was calculated, while μ_0 was found by dividing B_0 by H.

This process, including the preliminary adjustment of the current and the demagnetisation of the iron was repeated for each value of H used in the experiments, unless the contrary is stated.

TRANSFORMER IRON USED IN THE EXPERIMENTS.

§ 14. For testing transformer iron I used the "Magnetic Square" described in §§ 15, 16. The specimens used in this instrument were strips 4 cm. wide and about 68·8 cm. long, cut by shears out of wider strips. I employed three brands of iron, viz., "Tagger Plate," "Schultz," and "Sankey."

The sheet iron, called "Tagger Plate," was supplied by Messrs. C. A. Parsons & Co., Heaton Works, Newcastle-on-Tyne. The density of the iron was 7·853 grammes per cc., and the thickness of the sheets was about 0·034 cm. or 0·0134 inch.

The "Schultz" iron had a density of 7·715 grammes per cc., and the sheets were about 0·037 cm. or 0·0146 inch thick.

The "Sankey" iron had a density of 7·801 grammes per cc., and the sheets were about 0·034 cm. or 0·0134 inch thick. The "Sankey" iron and the "Schultz" iron were supplied by the British Electric Transformer Manufacturing Co., Ltd., Hayes, Middlesex.

THE MAGNETIC SQUARE.

§ 15. In all but one or two experiments I used specimens of the thin iron or steel plates manufactured for the construction of transformers and dynamo armatures, and the apparatus was specially designed for testing such specimens.

The magnetising arrangement consisted mainly of four solenoids, which I shall denote by A, B, C, D, each 52·25 cm. in length and 7·66 cm. in diameter, wound on wooden tubes, the mean number of turns per cm. being 5·693; the diameter of the wire was 0·127 cm. The four solenoids were arranged symmetrically with their axes along the four sides of a square, each side being 62·4 cm. in length.

The sheet metal was cut by shears into strips, each 4 cm. wide and about 68 cm. long, which were placed in the solenoids with their planes parallel to the plane of the square. The length of the strips was sufficient to allow the parts of them projecting from two adjacent solenoids to overlap at the corresponding corner of the square. Here the strips from the two solenoids were arranged alternately, and were clamped together by a non-magnetic clamp in order that the magnetic circuit might be as perfect as possible. By first putting one strip in A, then one in B and so on in cyclical order, the strips were easily assembled in the required manner. The number of strips in each solenoid was, of course, the same.

When I used only the main solenoids for magnetising the iron, I

found that the induction near the ends of the strips was seriously less than at their centres. To remedy this defect I arranged 12 extra coils in series with the main solenoids, and by roughly adjusting the number of turns on them I was able to make the inductions at the ends and at the centres of the strips nearly equal. At each corner, one coil of 30 turns was placed with its plane making angles of 45° with the two adjacent sides of the square, and a coil of 20 turns was wound at each end of every solenoid over the main winding. The total resistance of all the coils in series was about 5·5 ohms.

Making a small correction * of about 0·3 per cent. on account of the finite lengths of the solenoids, and on account of the 12 extra coils at the corners, I found that the magnetic force at the centre of any solenoid was 75·15 gausses per unit C.G.S. current.

All the magnetising coils were arranged in series and the magnetising current was measured, when desired, by a Weston millivoltmeter used with a suitable shunt. I found it more convenient, however, to put a suitable extra resistance in series with the voltmeter so as to change its effective sensitiveness, as I could then read directly from the voltmeter, without calculation, the value (in gausses) of the magnetic force produced by the current at the centres of the sides of the square.†

Within the wooden tubes were placed four non-magnetic tubes, each 53·9 cm. in length, of rectangular section, the sides of the section being 5·0 and 0·8 cms., and on the central parts of these tubes the secondary coils were wound. These coils, which were connected in series, were so arranged that I could use 3, 9, 27, or 81 turns on each side of the square, and suitable resistance coils were arranged to make the resistance of the secondary circuit independent of the number of turns. The secondary circuit included a ballistic galvanometer and an earth-inductor by which the galvanometer was standardised. These were the instruments used by Mr. T. G. Bedford and myself in previous experiments.‡

The magnetic square is shown diagrammatically, but roughly to scale in Fig. 1, where A indicates the terminals of the primary coil and B those of the secondary coil. The arrangement of the four corner coils and of the eight extra coils wound on the ends of the four solenoids will be apparent from the figure.

* This correction was easily found by aid of a simple magnetometer, consisting of a pivoted magnet with a pointer moving over a circular scale. The square was placed with its plane horizontal and with two solenoids at right angles to the magnetic meridian. The magnetometer was placed over the centre of one of these solenoids, and the magnetic force at the centre of the magnetometer due to a measured current was determined. This was due to the extra coils at the corners of the square and to the fact that the lengths of the solenoids were only finite. Since the centre of the magnetometer was only about 5 cm. from the axis of the solenoid, the force due to these causes was nearly the same as at the centre of the solenoid. I found that the mean value of the correction was 0·22 gauss per C.G.S. unit current, and that it was to be *added* to the force 74·93 calculated from the formula $4\pi ni$.

† I have explained this plan in detail in an article "On the Measurement of Magnetic Force," *The Electrician*, vol. 51, p. 319.

‡ *Phil. Trans. Royal Soc.*, vol. 198 A, pp. 52, 53.

To reduce the disturbing effect of the earth's magnetic field to a minimum, I followed Ewing's* plan and tilted the square so that its plane was normal to the earth's resultant magnetic force. The earth's field then produced only a very feeble magnetisation at right angles to the plane of the strips.

§ 16. In a recent article on "The Ballistic Measurement of Hysteresis," † I suggested that the magnetic square, ‡ just described, might prove convenient in magnetic tests of sheet iron, as a practically

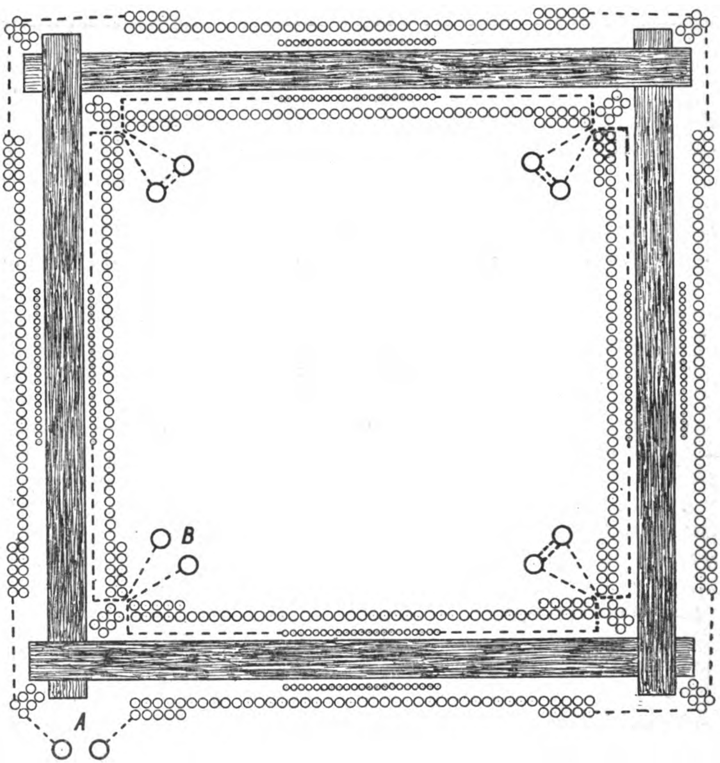

FIG. 1.—Magnetic Square.

perfect magnetic circuit can be built up by simply slipping the iron strips into the four solenoids, without the least interference with either the primary or the secondary circuits; the labour of winding a new

* Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 67.

† *The Electrician*, vol. 49, p. 100.

‡ Somewhat similar arrangements have been employed in wattmeter methods of measuring the loss of energy caused by eddy currents and hysteresis in iron by I. Epstein (Die magnetische Prüfung von Eisenblech), *Elektrotechnische Zeitschrift*, vol. 21, p. 303, and by L. W. Wild ("The Testing of Transformer Iron"), *The Electrician*, vol. 54, p. 128.

primary and a new secondary on each fresh specimen is thus avoided, and the resistances, which have been adjusted to allow the magnetic force to be read directly from the milli-voltmeter, require no fresh adjustment.

It is found, in practice, that specimens cut from the same batch of plates, and even from a single plate, may vary greatly in permeability, the "best" specimens being sometimes twice as permeable as the "worst" ones when tested with magnetic forces less than the critical force M_0 (see § 7). By the magnetic square strip specimens can be readily tested for uniformity in magnetic quality, for it is easy to insert them one at a time into one of the solenoids and to observe the ballistic effect of a reversal of a small constant magnetic force after effective demagnetisation. Though the disturbing effects of the ends of the strip prevent the results from furnishing an accurate value of the normal permeability for each strip, yet the test is useful since it enables us to sort a number of strips into sets so that all the strips in each set shall be of approximately the same magnetic quality. The labour of winding the rings would make it difficult to make this test with ring specimens (see also §29).

Tests of the Magnetic Square.

§ 17. The magnetic square proved to be so convenient for experiments on transformer iron that it seemed proper to test how nearly the arrangement approaches ideal efficiency. The chief points to be considered are (1) how far the square can be trusted to yield definite results for a given set of strips of iron; and (2) if the results are definite, what accuracy can be claimed for them. The tests were made in August 1903, with the help of Mr. E. Smart. I had made similar tests in July and August, 1902, but I did not then fully appreciate the influence of "magnetic history" and the benefits of effective demagnetisation, and thus these old tests were of little value.

§ 18. To test the definiteness of the results I made a series of measurements upon the sixteen strips of "Tagger Plate" described in § 29. The strips were divided into four sets, *a*, *b*, *c* and *d*, of four strips each, and the four sets were arranged in A, B, C and D, the four solenoids of the square, so that *a* was in A, *b* in B, *c* in C, and *d* in D. The normal induction was then found, by the method of § 13 for a series of six values of the magnetic force. In demagnetising the iron, the maximum magnetic force used was 14·4 gausses, a value much greater than M_0, the critical force of the 1·6 gausses, for which μ_0 is a maximum. In each case the ballistic effects of two pairs of reversals were observed after 100 reversals of H, and special care was taken to maintain the magnetic force accurately at the values required.

The strips were then taken out of the square and were re-arranged so that *b* was in A, *c* in B, *d* in C, and *a* in D, and the values of B_0 for the six values of H were again determined. The same was done for the other two arrangements in which we had *c* in A, and *d* in A.

The values of B_0, in the four arrangements of the strips, were

calculated from the observations by four-figure logarithms with the following result :—

H	0·5	1·0	1·5	2·0	2·5	3·0
a in A	512·7	2,817	6,060	7,940	9,069	9,909
b in A	513·1	2,817	6,044	7,902	9,080	9,940
c in A	514·4	2,834	6,073	7,944	9,090	9,947
d in A	510·6	2,817	6,048	7,895	9,083	9,920
Mean	512·7	2,821	6,056	7,920	9,081	9,929

The agreement between the values of B_0 for the four arrangements is very good, the average departure of any value from the mean of its set being only 0·18 per cent. Slight differences in clamping the strips together at the corners of the square might be expected to render the "magnetic resistance" of the iron circuit uncertain, but these experiments show that this effect is practically insensible.

§ 19. In order to test the accuracy of the results obtained by means of the magnetic square, I made two sets of experiments using the same strips as in § 18. In the one case (§ 20), perhaps the more important one, I found *h*, the demagnetising force at the centres of the sides, arising from the magnetic leakage at the corners of the square, and in the other case (§ 21) I found ΔB_0, the excess of the normal induction at the centres of the sides above that near the corners of the square. I also compared *h* and ΔB_0 with B_0, the normal induction at the centres of the sides, for various values of H_c, the magnetic force at the centres of the sides due to the current C itself. In most of my experiments I have taken H_c as the force acting at the centres of the sides, but in the present discussion we must distinguish between the two parts of the total force which are due to the current and to the magnetic leakage respectively. If H be the total force, we have $H = H_c - h$.

§ 20. In finding *h*, the demagnetising force, I used a method previously employed by Mr. T. G. Bedford and myself.* The square was so placed that two of its sides were at right angles to the magnetic meridian and its plane was normal to the earth's resultant magnetic force, and a magnetometer, with a pointer moving over a graduated circle, was placed on each of the two horizontal solenoids immediately over its centre. Since the magnetometer magnet was only about 5·2 cm. from the axis of the corresponding solenoid, the magnetic force indicated by the magnetometer was nearly equal to the force at the centre of the solenoid arising both from magnetic leakage at the corners

* *Phil. Trans. Royal Soc.*, vol. 198 A, p. 98.

of the square, and from the finite length of the solenoids and the current in the extra coils.

In taking the observations for each value of H_c, the iron was first demagnetised, the maximum force D employed being 14·1 gausses, and then the normal induction B_0 was found after 50 reversals of the current. The two magnetometers were also read, both when the current was positive and when it was negative. In finding h, the demagnetising force arising from magnetic leakage, a proper correction, proportional to the current, was applied to the force indicated by the magnetometers to allow for the part due to the current itself. The results are given in the following table and are exhibited in Fig. 2, by the curve marked "100 h," where the ordinate represents 100 h and the abscissa B_0. In the table, H_c denotes the magnetic force at the centres of the solenoids as calculated from the current :—

H_c	B_0	h	ΔB_0	H_c	B_0	h	ΔB_0
0·5	520	— ·0046	— 22·2	3·0	10,250	+ ·0521	+ 354
0·75	1,272	— ·0084		4·0	11,480	+ ·0742	+ 492
1·0	2,840	— ·0085	— 16·7	5·0	12,300	+ ·0925	+ 602
1·25	4,795	— ·0002		6·0	12,890	+ ·1060	+ 690
1·5	6,250	+ ·0096	+ 93·0	7·0	13,300	+ ·1165	+ 774
2·0	8,175	+ ·0265	+ 189	8·0	13,710	+ ·1253	+ 846
2·5	9,400	+ ·0415	+ 274	9·0	13,920	+ ·1320	+ 903

The demagnetising force is negative for the first few values of H_c, so that in these cases the magnetic leakage at the corners gives rise, at the centres of the sides, to a force in the same direction as that due to the current itself, thus assisting the current's force instead of opposing it. This result indicates that the extra coils at the corners are a little too efficient for small values of H_c. As H_c increases, h soon becomes positive and then increases, more and more rapidly; the ratio of h to H_c does not, however, increase continually but reaches a maximum value 0·0185 when $H_c = 5·0$. It is, therefore, at this point that the greatest proportional error in the magnetic force occurs, with the present specimen. The force estimated from the current is 5·0, but the actual force operative at the centres of the sides is not 5·0 but only 5·0 — ·0925 or 4·9075.

The error of 0·0925 in the estimation of a force of 5·0 is at first sight rather serious, but, before the magnetic square is condemned on account of it, a comparison should be made with an ellipsoid of revolution. Now the demagnetising force for an ellipsoid of small equatorial

diameter $2a$ and large polar diameter $2c$, magnetised to intensity I, is NI, where *

$$N = 4\pi \frac{a^2}{c^2} (\log_\epsilon \frac{2c}{a} - 1).$$

If the magnetic state of the ellipsoid be the same as that of the iron at the centres of the sides, we have

$$4\pi I = B_0 - H = 12300 - 4{\cdot}9075 = 12295,$$

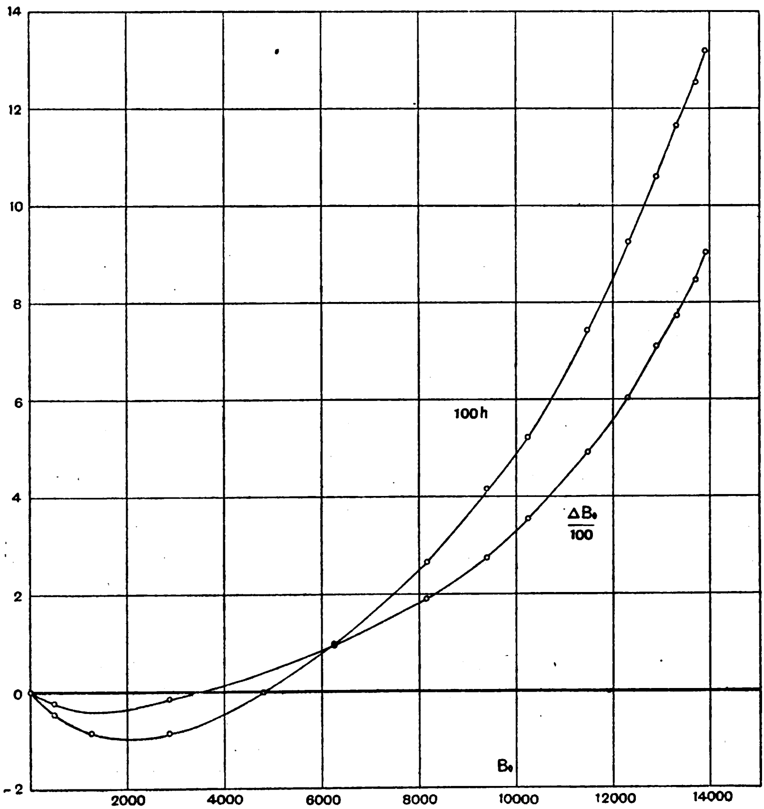

FIG. 2.—Tests of Magnetic Square.

so that $I = 978{\cdot}4$. In order that the ellipsoid may have the demagnetising force $0{\cdot}0925$ when I is $978{\cdot}4$, we must have $N = 0{\cdot}0925/978{\cdot}4 = 0{\cdot}0000945$, whence we find $c/a = 932$.

Now the total cross-section of the strips forming each side of the square is $0{\cdot}5474$ cm.^{2}, an area equal to that of a circle $0{\cdot}834$ cm. in diameter. Hence an ellipsoid, with an equatorial section of $0{\cdot}5474$ cm.^{2}, must have its polar diameter at least $0{\cdot}834 \times 932$ or 777 cm. in length

* Maxwell, *Electricity and Magnetism*, 3rd ed., vol. 2, p. 69.

if the demagnetising force due to it is not to exceed that due to the square.

It might be possible, by a more suitable arrangement of coils, to reduce the demagnetising force further, but the accuracy already attained is probably amply sufficient for commercial purposes in view of the methods of building up transformer cores out of stampings from plates.

With a smaller number of strips in each solenoid the demagnetising force would be smaller in about the same proportion, provided that the strips were all equal in magnetic quality. But there are such considerable variations in the magnetic quality of strips from the same batch, that, unless some attempt is made to build up the square out of strips of the same quality, the inequalities in the four sides of the square may give rise to magnetic forces greater than those recorded in the present instance.

In the case of a uniformly magnetised ellipsoid, h is proportional to I, but the experiments of Mr. T. G. Bedford and myself show that for a cylinder the effects of hysteresis quite prevent h having any simple relation to I. In the case of the magnetic square, I studied the relation of h to B, as B went through a cycle, and I obtained an h–B loop similar to that which Mr. Bedford and I obtained* with a cylinder, but as I am not now concerned with the measurement of hysteresis, I shall not refer further to the matter.

§ 21. To find ΔB_0, the excess of the normal induction at the centres of the sides above that near the corners of the square, I wound *two* secondary coils of 10 turns on each of the four sets of strips, one coil being at the centre of the strip and the other 27 cm. from the centre. The 8 coils were connected in series, but so that the 4 coils at the centres of the strips were in opposition to those at the ends. With this arrangement the ballistic effect of reversing H_c gave ΔB_0 directly, and with great accuracy since the "throws" were easily made large by reducing the secondary resistance. For each value of H_c, ΔB_0 was found after 50 reversals of H_c following on demagnetisation with $D = 14{\cdot}1$. The results are given in the table in § 20, and are exhibited in Fig. 2 by the curve marked "$\frac{\Delta B_0}{100}$", where the ordinate represents $\Delta B_0/100$ and the abscissa B_0.

The form of the curve for ΔB_0 is similar to that for h, so that there is a close connexion between h and the magnetic leakage. The negative value of B_0 for the smaller values of H_c indicates that the extra coils at the corners are a little too efficient for small values of H_c, a result already noticed in § 20.

§ 22. It was my original intention to make a careful investigation of the special characteristics which the magnetic square possesses in virtue of its special form, but I soon found that the effects of magnetic history were so important that any investigation of the properties of the square would be of comparatively little value until the effects of magnetic history had been studied. This study has ranged over so

* *Phil. Trans. Royal Soc.*, vol. 198 A, p. 102,

wide a field that time has failed for a full investigation of the square itself. This defect is, however, of the less consequence since the general character and significance of the results recorded in the present paper are not seriously affected by errors of 1 or 2 per cent. in the value of the magnetic force.

Preliminary Magnetometric Experiments.

§ 23. Although the present paper is intended to illustrate the influence of the magnetic treatment of a specimen upon the results subsequently obtained in ballistic tests, I shall first of all give an account of some experiments made by the magnetometer method, because in this method the absolute value of I, the intensity of magnetisation, is known, at least approximately, at every stage. These experiments form an introduction to the subject, and will, I think, assist the reader in understanding many of the ballistic experiments with which the paper chiefly deals.

§ 24. The specimen was a straight iron wire 45·6 cm. in length and 0·0114 cm.^{2} in cross-section. The wire was magnetised in a solenoid, which was placed along the east and west line through the mirror magnetometer, a compensating coil being arranged in series with the solenoid so as to balance the magnetic action of the solenoid itself upon the magnetometer. The magnetometer needle was 15·4 cm. from the nearer end of the wire.

For demagnetising the specimen I arranged the alternating current so that the maximum magnetic force due to it was 7 or 8 gausses. By drawing through the origin a tangent to the "normal" I_0–H curve in Fig. 3, it will be seen that the maximum susceptibility is reached when $H = 3·5$ gausses, and thus the maximum magnetic force used in the demagnetisation satisfied the conditions laid down in § 7; I found that the demagnetisation was completely successful under these conditions.

I then placed the iron in the solenoid and made, with the help of Mr. T. G. Bedford, a series of observations for the "normal" I_0–H curve in the following manner. The magnetic force having been adjusted to some desired value H, the circuit for the continuous current was broken, and the wire was demagnetised by the alternating current. The force H was then re-applied and, in order to avoid initial effects, was reversed twenty times before we took the readings of the two magnetometer deflections, which corresponded to the two directions of the magnetising current. This process was repeated for each value of the magnetic force employed in obtaining the curve. By demagnetising the iron before taking the readings for each value of H we gained the great advantage that we could always get the same value of I_0 (the "normal" value) for a given value of H, whatever the previous magnetic treatment of the specimen. The curve showing how I_0 depends upon H is given in Fig. 3, and is marked "I_0." We determined also, for each value of H, the value of the residual intensity of magnetisation left on breaking the circuit, and have indicated this value by a point on

the axis of I, the number in parentheses—thus (2·5)— giving the value of H before the circuit was broken.

§ 25. After this preliminary work we examined the effects of the "magnetic history" (§ 9) of the iron upon the I–H loop* formed when H goes through a cycle with the limits ±1·5 gausses. To carry out the process rapidly, we adjusted the necessary resistances beforehand, so that we could apply forces of ± 1·5, ± 1·0, ± 0·5 or 0 gausses at will.

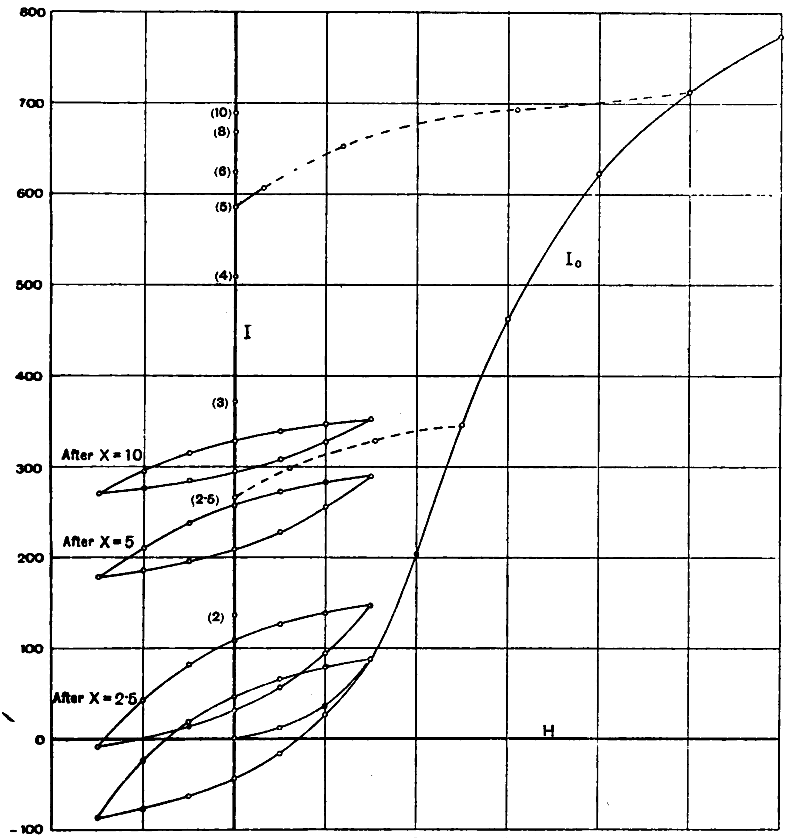

FIG. 3.—Iron Wire.

We then demagnetised the iron and, after 100 reversals of the force of 1·5 gausses, made observations for the I–H loop. Since this loop was obtained after effective demagnetisation, the definitions of § 8 lead us to call it the "normal" I–H loop for the range ±1·5 gausses.

* I find it convenient to use the word *loop* to describe the line traced out on the I–H diagram when H goes through *cycles* after the cyclic state has been established.

We again demagnetised the iron, and afterwards subjected it to 50 reversals of a magnetic force X, breaking the circuit finally in such a way that the force sank from + X to zero. The force of 1·5 gausses was then applied and, after it had been reversed 100 times, the observations for an I–H loop were made.

This process was carried out with the three values 2·5, 5·0, and 10·0 for the force X, which gave the magnetic history, and the resulting loops are shown in Fig. 3, where they are marked "After X = 2·5," "After X = 5," and "After X = 10," respectively.

We notice, first, that the force X has left the iron so highly magnetised that, even after 100 reversals of the force 1·5, the corresponding I–H loops are moved up along the axis of I through distances which increase with X. It will next be seen than the change in I, due to the change of H from + 1·5 to − 1·5 gausses, becomes smaller as X increases. It is the half of this change which I call the "apparent" intensity of magnetisation and denote by I′. We may also note that the difference between the two values of I for H = 0, *i.e.*, the length of the axis of I included within the loop, diminishes as X increases, with the effect that the energy dissipated in each cycle through hysteresis also diminishes. If W ergs be the energy dissipated through hysteresis per c.c. per cycle, then

$$W = -\int I dH,$$

so that W is numerically equal to the area of the I–H loop, when it is drawn on the proper scale.

The numerical details for the four I–H loops are as follows:—

	I. Normal loop.	I. After X = 2·5	I. After X = 5·0	I. After X = 10·0
H = + 1·5... ...	+ 86·1	+ 145·2	+ 288·0	+ 351·8
H = − 1·5... ...	− 86·7	− 9·0	+ 177·9	+ 270·0
H = 0	+ 46·8	+ 107·7	+ 257·1	+ 327·6
H = 0	− 44·7	+ 30·6	+ 208·5	+ 294·0

	Normal loop.	Loop after X = 2·5	Loop after X = 5·0.	Loop after X = 10·0.
Apparent Intensity I′...	86·4	77·1	55·0	40·9
Mean Intensity	− 0·3	+ 63·1	+ 233·0	+ 310·9
Hysteresis loss W ...	180·0	150·8	94·5	66·2

	H = 1·5	H = 2·5	H = 5·0.	H = 10·0.
Intensity I	86·4	345	712·5	894·6
Residual Intensity left on removing H ...	46·8	265·8	581·4	689·1

The removal of the forces 2·5, 5·0, and 10·0 left the iron highly magnetised, the actual values of the residual intensity of magnetisation being 265·8, 581·4, and 689·1. A good deal of this was shaken out of the iron by the 100 reversals of the force of 1·5, which were made before the observations for the I–H loops. Thus, if, in any displaced loop, a straight line be drawn joining the tips of the loop, these three straight lines cut the axis of I in the points $I = 63·1$, $I = 233·0$, and $I = 310·9$.

To illustrate the relation between (*a*) the course followed by I, when H diminishes from a large value X to zero, and (*b*) the I–H loop, with the limits $\pm$ 1·5 for H, which is subsequently traced out after many reversals between those limits, I made observations for I while H diminished from 2·5 to zero and from 5·0 to zero; the resulting curves are shown as broken lines in Fig. 3.

§ 26. In another series of magnetometric experiments upon the same wire, I took a large number of values for the magnetic force X, which gave the wire its magnetic history (§ 9), and made observations to find how (*a*) the "apparent" magnetisation, I′, which is subsequently found for cycles with the limits $H = \pm 1·5$, and (*b*) the displacement of the I–H loop depend upon X. The wire, after demagnetisation, was subjected to 50 reversals of X, and the circuit was finally broken when I was positive. The force of 1·5 gausses was then applied, and, after it had been reversed 100 times, the value of I with $H = + 1·5$ and its value with $H = - 1·5$ were taken. Half the difference between these two values gives the "apparent" magnetisation I′, while half the sum gives the mean value of I, and indicates how far the I-H loop has been displaced from its normal position by the after-effects of X. This process, including the demagnetisation, was repeated for each fresh value of X, and the resulting curves are shown in Fig. 4.

From the curve marked "Mean I for $H = 1·5$," it will be seen that no value of X less than 1·5 gausses displaces the I–H loop, but that, as soon as this limit is passed, the displacement increases, at first rapidly and afterwards more slowly, as X increases. From the curve marked "I′ for $H = 1·5$," it appears that X has no effect upon the value of I′ until X slightly exceeds 1·5, but that as X increases from 2·0 upwards I′ diminishes at first rapidly, and afterwards more slowly. From the pair of curves it is clear that the diminution in the "apparent" intensity of magnetisation is closely connected with the displacement of the I–H loop.

§ 27. The experiments of §§ 23–26 show how important it is to

demagnetise thoroughly a specimen before making a test of its magnetic quality, at any rate when the magnetic force for which it is to be tested is less than the critical force. They also show how careful the observer must be not to expose the iron under test to strong magnetic forces unless he has at hand some efficient means of demagnetising it. In the magnetometric method any want of symmetry due to permanent magnetisation is at once detected, but in the ballistic method, as usually employed, there is no means of detecting permanent magnetisation, and the observer, who was unaware of the influence of

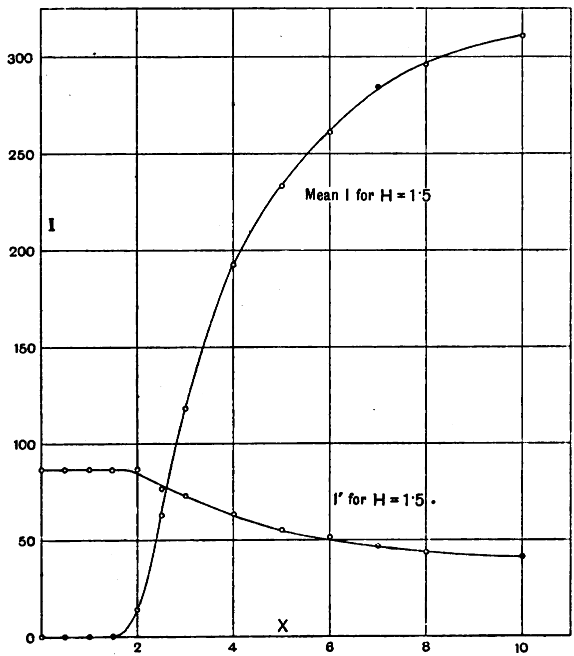

FIG. 4.—Iron Wire.

the magnetic history of iron upon ballistic tests with small magnetic forces, might easily condemn a specimen as having low permeability for small forces, when really the fault lay, not in the iron, but in his own carelessness in exposing the iron to a large magnetic force. If, for instance, he were using the iron of § 24, the apparent permeability for $H = 1{\cdot}5$ would be more than halved if the iron had been subjected to a force of 10 gausses.

Preliminary Ballistic Experiments.

§ 28. In most of the ballistic experiments described in the present paper I confined myself to the examination of the effects of its magnetic history (§ 9) upon the apparent induction, or the apparent permeability

of a specimen. But I think it may be useful, in opening the subject, to give an account of some experiments made to find the effect of previously applied magnetic forces upon the actual forms of the B–H loops in cycles with the limits ± 1 for H.

The iron, which was tested by means of the magnetic square (§ 15), was in the form of 16 strips, each 4 cm. wide, which were cut by shears out of wider strips of "Tagger Plate" (§ 14).

§ 29. In order to obtain a set of strips of nearly uniform magnetic quality, I cut off 30 strips and tested them one at a time. For this purpose I put 3 strips, each 3·7 cm. wide, along 3 sides of the square, and completed the square by the 4 cm. strip under test. Only one secondary coil was used, and this was fitted to the fourth side of the square. I measured the apparent induction for $H = 2{\cdot}5$ gausses for each of the 30 strips, and found that it varied from about 9,200 for the most permeable to about 4,500 for the least permeable. From the 30 strips I selected 16, which agreed pretty well among themselves, the greatest and least values of B' being 9,160 and 8,550 maxwells per sq. cm. respectively, according to this rough method of testing. These 16 strips, which I used in a large number of experiments, were equally divided among the 4 sides of the square.

§ 30. After I had learned the importance of effective demagnetisation, I tested the 16 strips for permeability, and found that the "normal" permeability reached a maximum value of about 4,200 when $H = 1{\cdot}6$. The observations for permeability were taken in the following manner. The magnetic force having been adjusted to some desired value H, the circuit for the continuous current was broken, and the iron was demagnetised by the alternating current, the maximum magnetic force used in the operation being about 4·8; this was found to give effective demagnetisation. The force H was then re-applied, and was reversed 200 times before the two ballistic effects of a pair of reversals of H were observed. This process, including the demagnetisation, was repeated for each value of H used in obtaining the normal B_0–H curve which is shown in Fig. 5 as a dotted line.

§ 31. In making the observations on the B–H loops I used a key similar to that described by Professor Ewing,* and I was careful to work the key in such a manner as to make the representative point on the B–H diagram always keep to the B–H loop. Thus, when I had observed the ballistic effect of changing the magnetic force from its positive limit $+1$ to any value H, I made the force go from H to -1, and then from -1 to $+1$; I did not make it pass directly from H to $+1$.

In obtaining the "normal" B–H loop, I first demagnetised the iron and then applied the force $H = 1$, and reversed this 200 times before taking the observations for the loop. This loop is shown in Fig. 5; it lies symmetrically with respect to the origin.

To show the effects of magnetic history I made two other loops with the same limits (± 1) for the magnetic force. In these cases,

* Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 357.

after demagnetising the iron, I subjected it to 50 reversals of a magnetic force X, breaking the circuit finally in such a way that the force sank from +X to zero. I then applied the force of 1 gauss and, after reversing it 200 times, made observations for the B–H loop. The two loops obtained in this way are shown in Fig. 5, where they are marked "After X = 3" and "After X = 8" respectively.

§ 32. In order to place the B–H loops in their true positions on the diagram it is necessary to know the absolute value of the induction for some one point on each loop. This value was obtained by the devices which will be described in §§ 33, 34. Meanwhile, it will be seen

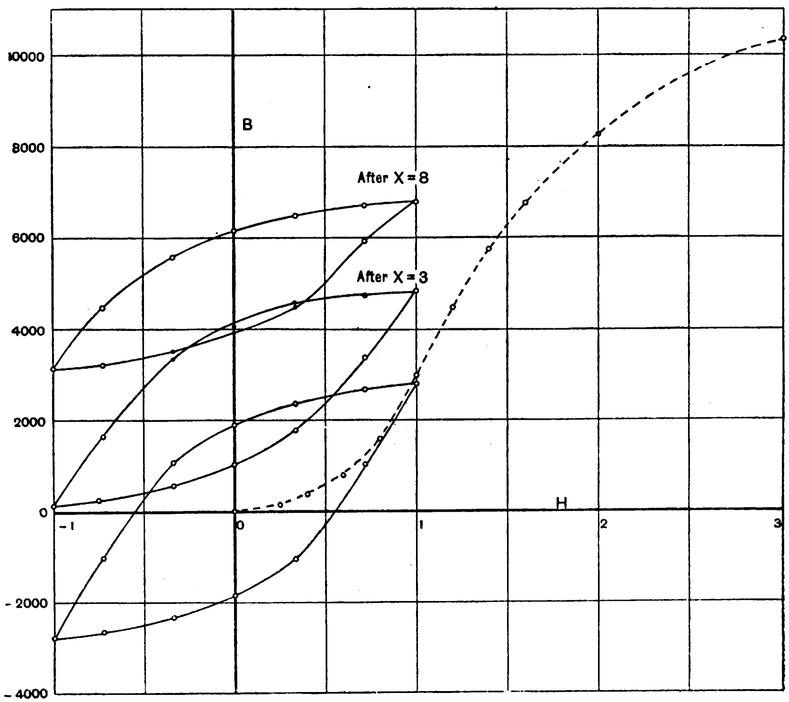

Fig. 5.—"Tagger Plate."

that the ballistic experiments agree with the magnetometric experiments of § 25 in showing that the application of the force X has resulted in reducing the range of B for cycles with the limits ±1 for H, in displacing the B–H loop along the axis of B and in diminishing the area of the B–H loop. The three following tables give the numerical details. The first table gives the value of B for H = +1 and its value for H = −1. Half the difference of these values gives the apparent induction B′, while half their sum gives the "mean induction" and indicates how far the B–H loop has been displaced along the axis of B by the after-effects of the magnetic force X. The energy dissipated

through hysteresis per c.c., per cycle has been calculated in ergs from the areas of the curves by the formula—

$$W = \frac{1}{4\pi}\int H d B.$$

	B Normal loop.	B After X = 3·0.	B After X = 8·0.
H = + 1·0	+ 2,780	+ 4,824	+ 6,780
H = − 1·0	− 2,780	+ 114	+ 3,106
H = 0	+ 1,872	+ 4,040	+ 6,125
H = 0	− 1,872	+ 1,026	+ 3,917

	Normal B–H loop.	Loop after X = 3·0.	Loop after X = 8·0.
Apparent Induction B′	2,780	2,355	1,837
Mean Induction.........	0	2,469	4,943
Hysteresis Loss W ...	390	318	222

	H = 1·0.	H = 3·0.	H = 8·0.
Absolute Induction B......	2,780	10,300	13,680
Residual Induction left on removing H	1,872	8,550	10,480

§ 33. I will now explain the method employed in the determination of the absolute value of the induction at any point on the B–H loops.

In the case of the normal loop I made the usual assumption that the values of B at the two tips of the loop are equal in magnitude and opposite in sign. To test the legitimacy of this assumption I made the following experiment. I demagnetised the iron and then applied a magnetic force of 1 gauss, and noted its ballistic effect. On the assumption that the demagnetisation was complete, this first application of the force raised B from 0 to + 2,842. On reversing H there was a change of induction of 5,738, so that the value of B for H = − 1 was 2,842—5,738 or — 2,896. Continuing the reversals, I found in succession

the values $+2{,}896$, $-2{,}903$, $+2{,}909$ for B.* These numbers agree so closely with each other that we may fairly suppose that after 200 reversals the values of B at the two tips of the loop are equal in magnitude and opposite in sign. We may thus take the absolute induction at the tip of the normal loop for $H = 1{\cdot}0$ as equal to the normal induction. (See § 6.)

With the larger forces of 3·0 and 8·0 we may safely take the absolute induction at the tip of the normal loop as equal to the normal induction.

The residual induction left when H was removed was found from the absolute value of B for the force H by subtracting from it the change of induction calculated from the ballistic effect of removing H.

§ 34. In finding the absolute values of B for the two displaced loops, I proceeded on the assumption that, provided the iron has not been subjected, since its demagnetisation, to any magnetic force numerically greater than a "large" force Z, the value which B assumes, when Z is applied, is independent of the particular force, within the limits $\pm Z$ from which the step to Z is made. I use the adjective "large" to denote a force which is considerably greater than the critical force M_0.

Of the two points of intersection of the B–H loop with the axis of B, I selected that one where B has the algebraically greater value as the point for which the absolute value of B was to be determined. In the case of the loop marked "After $X = 8{\cdot}0$," I first demagnetised the iron and then subjected it to 50 reversals of a force of 8·0 gausses, breaking the circuit finally in such a way that H sank from $+8{\cdot}0$ to zero. I next applied a force of 1·0 gauss and reversed it 200 times, breaking the circuit finally in such a way that H sank from $+1{\cdot}0$ to zero. I then applied a force of $+8{\cdot}05$ gausses and found that the ballistic effect, which Mr. Bedford observed, corresponded to an increase of induction of 7,750 maxwells per square cm. The magnetic force was next reversed from $+8{\cdot}05$ to $-8{\cdot}05$ and the corresponding change of induction was 27,760. On again reversing H, the change in B was 27,820, the two numbers agreeing within the errors of observation. Assuming that the force of 8·05 is relatively so large that its reversal causes B to change in sign but not in magnitude, we may take 13,900 as the value attained by B when the force of $+8{\cdot}05$ was applied for the first time after the small cycles. Hence the absolute value of the residual induction left, when the force of 1 gauss was removed at the end of the 100 cycles, is $13{,}900 - 7{,}750$ or 6,150.

A verification of the value 13,900 was obtained by demagnetising the

* The 200 reversals, which were made before B′ was found, caused half the change of induction, due to the change of the force from $+1$ to -1 or *vice versa*, to diminish from about 2,900 to 2,780. (See § 3.) The observations for the normal loop and for the (dotted) normal $B_0 - H$ curve were made on different days. Some small error, probably arising from the re-standardisation of the galvanometer, or perhaps from change of temperature, has caused the value of B for the normal loop, at $H = 1{\cdot}0$, to differ slightly from the value of B_0 for the normal $B_0 - H$ curve at $H = 1{\cdot}0$.

iron and observing the ballistic effect of the first application of a force of 8·05, when it was found that B rose from zero to 13,890.

We repeated the observations with a force of 6·10 in place of 8·05 and found that the induction increased by 6,980 when the force + 6·10 was applied for the first time after the magnetic force had been allowed to sink from + 1·0 to zero at the conclusion of the 100 cycles. Two reversals of H gave 26,000 and 26,340 as the changes of induction. Taking the mean, the induction when H = + 6·10 is 13,080. Hence the absolute value of the induction at the selected point is 13,080 — 6,980 or 6,100, agreeing closely with that found by the aid of the force 8·05.

On applying the force of 6·10 for the first time to the previously demagnetised iron, the induction rose from zero to 13,090; we thus verified the number used in the calculation.

In a similar manner, using a force 8·05, we found 4,040 as the absolute value of the induction at the selected point on the loop marked "After X = 3."

On comparing the values for the "Mean Induction" with those for the residual induction after H = 3 and H = 8, it will be seen that the 200 reversals of the force of 1·0 gauss, which were made before B′ for H = 1·0 was measured, had the effect of removing a large part of the residual induction left after the strong forces. A similar result was found in § 25.

§ 35. In the experiments described in the remainder of this paper I made no attempt to determine either the true positions of the B–H loops on the diagram or the areas of the loops. I confined myself to an examination of the effects of magnetic history upon the apparent induction and the apparent permeability. The magnetometric and ballistic experiments of §§ 23–34 will perhaps give the reader a general insight into what happens in the less complete experiments.

Effects of many Reversals of a Force X upon the Apparent Permeability for a Smaller Force H.

§ 36. To illustrate in a systematic manner the effect of a magnetic history (§ 9) of many reversals of a magnetic force upon the apparent induction subsequently found for a smaller magnetic force, the following experiments were made upon the same 16 strips of transformer iron as were described in § 29.

The iron, after demagnetisation by reversals, was subjected to 50 reversals of a force X for the purpose of giving it a magnetic history; it was then subjected to 200 reversals of a force H and at the end of this series of reversals the apparent induction B′ (§ 4) for the force H was found. This constituted one observation. The operation, including the demagnetisation, was then repeated with other values of H ranging from zero to X, the force X remaining fixed. A new value was then taken for X and the process of finding the apparent induction for a series of values of H was repeated, the values assumed by X being 8·0, 4·0, 2·5, and 1·0 gausses. To obtain a standard for comparison the normal induction B_0 (§ 6) was found for each value of H employed.

The alternating current used in demagnetising the iron was measured and the maximum value of the magnetic force used in the process was found to be about 4·8 gausses. This exceeded considerably the critical magnetic force—1·6 gausses—and was found to be sufficient for effective demagnetisation.

The results of the experiments are recorded in Fig. 6, which shows how the values of the normal permeability μ_0* and of the apparent permeability μ' depend upon H. In order to show all the results on a single diagram a fresh origin has been taken for each fresh value of X, the step from one origin to the next one being 1·0 gauss. To make the diagram more complete, the curve for each of the smaller values of X has been continued beyond the point H = X by the data obtained for the normal permeability in the experiments with X = 8; the continuations of the curves are shown as broken lines, thus—

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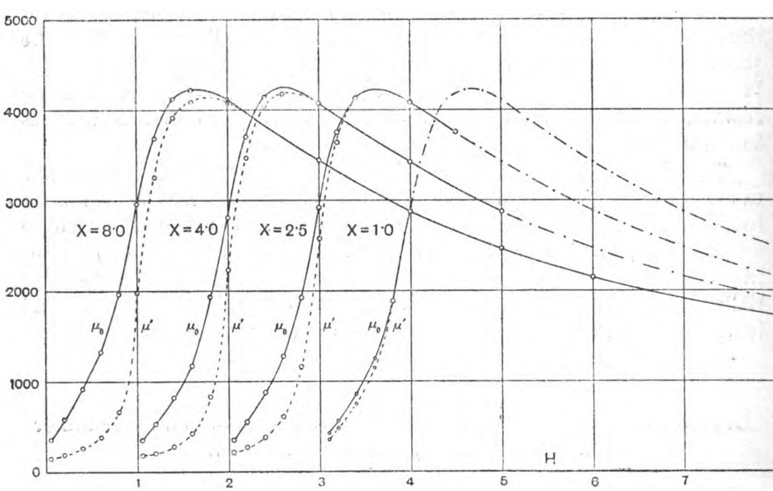


FIG. 6.—"Tagger Plate."

§ 37. It will be seen that in each case the apparent permeability after the action of the force X, is, for small magnetic forces, less than the normal permeability for the same forces, the effect being the more marked the greater the force X which gave the magnetic history. The greatest proportional reduction of permeability occurs for H = 0·5 after the action of X = 8, when the value of μ'/μ_0 is as small as 0·28.

* I have drawn the μ–H curves instead of the B–H curves because in the case of the B–H curves the range of values of B is so great that it is impossible to show the smaller values with any accuracy on a diagram of moderate size. In the case of the μ–H curves, however, μ does not vanish with H, and the range of values of μ is not too great to allow useful readings to be taken from the curves for *all* the values of H employed in the experiments.

But even for the largest value of X there is very little difference between μ' and μ_0 when H exceeds 1·6, the critical force for which μ_0 attains its maximum value; for H = 3·0 and for larger forces up to H = 8·0 I could detect no difference between μ' and μ_0. The value of H, for which the two curves begin to be identical, diminishes as X diminishes, and in each case the two curves begin to coincide before H reaches X.

In the curves for X = 8·0, X = 4·0, and X = 2·5 it will be seen that the maximum of μ' occurs at nearly the same value of H as the maximum of μ_0. In this circumstance is found a justification of the statement made in § 7, viz., "The magnetic force M′, corresponding to the maximum of μ', has a nearly definite value for a given specimen, for it

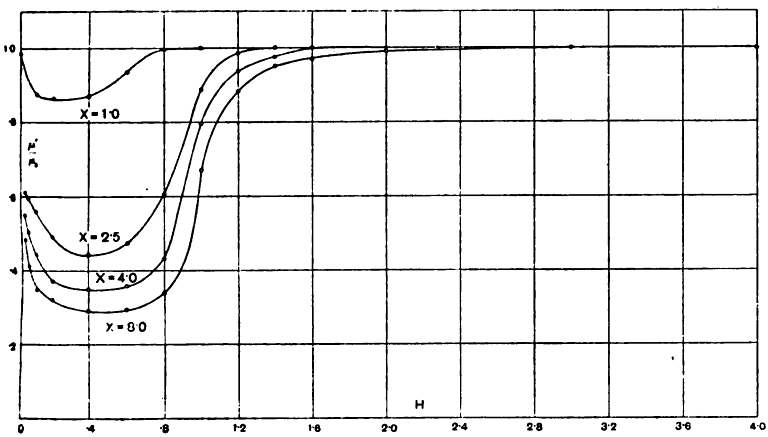


FIG. 7.—"Tagger Plate."

is found that its value is but little affected by the magnetic treatment experienced by the specimen before this series of permeability tests."

§ 38. The results of these experiments are exhibited in another way in Fig. 7, where each curve shows how the ratio of μ' to μ_0, for one value of X, depends upon H. Here we see that each curve has a well-marked minimum in the neighbourhood of 0·4 gauss. In the case of the three larger values of X we notice that μ'/μ_0 differs greatly from 1·0 when H = 0·025, the smallest force used in the experiments, while for X = 1·0, μ'/μ_0 is equal to 1·0 within the error of observation, when H = 0·025.

This figure shows more clearly than any words the need of care in magnetic testing, especially when the magnetic force employed is less than the critical force M_0—at least in the case of some brands of transformer iron. We see that in the case of the present specimen a few reversals of so moderate a magnetic force as 2·5 gausses are sufficient to make the apparent permeability for H = 0·5 less than half the normal permeability. It might be hoped that, by reversing

H a sufficient number of times before making the observations for the apparent permeability, we could restore the iron to a highly permeable state, but, as far as my experience goes, this hope is vain for I have never found continued reversals of H produce a rise of apparent permeability. Effective demagnetisation, on the other hand, at once restores the iron, and restores it with so great an accuracy that the results obtained for a given magnetic force agree within the limits of experimental error whatever the magnetic treatment of the iron. In later parts of the paper I describe experiments by which I studied the gradual restoration of iron from a state of low permeability, due to strong magnetisation, to one of high per-

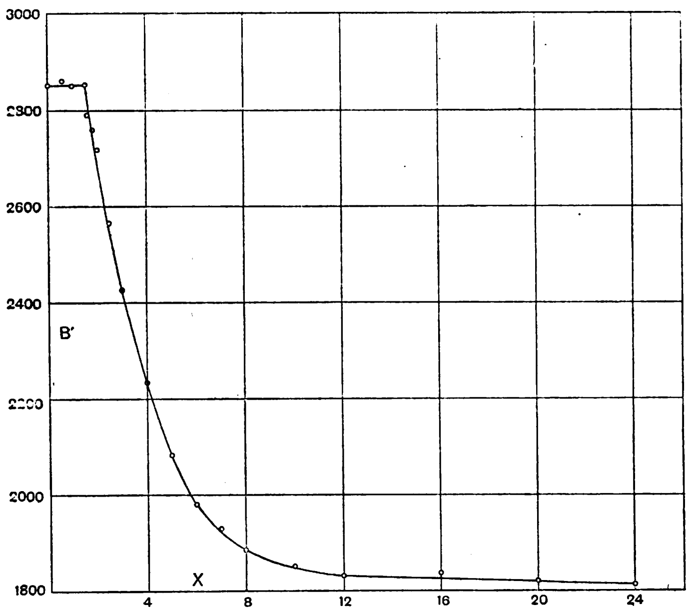


FIG. 8.—"Tagger Plate."

meability by making a gradual increase in the maximum amplitude of the force used in the demagnetisation.

§ 39. In the experiments described in §§ 36–38, I traced the effects of 50 reversals of a force X upon the apparent induction for a force H, and in the diagrams recording those experiments, each curve corresponds to a fixed value of X, and shows the course of the phenomenon as H *varied* from 0 to X.

The experiments, now to be described, were the first systematic experiments I made upon the effects of magnetic history. In them the force H, for which I found the apparent induction, was kept at the constant value of 1·0 gauss, and the effects of a continuously varying magnetic history upon the value of B′ for H = 1·0 were

observed. The 16 strips of transformer iron described in § 29 were used in these experiments.

§ 40. In the simpler experiments, after the iron had been demagnetised by reversals, it was subjected to 50 reversals of a force X, the batteries and resistances having been so adjusted before the demagnetisation as to give the desired value of X when the continuous current was applied after the demagnetisation. After the 50 reversals of X, the circuit was broken and the batteries and resistances were rearranged in such a manner (ascertained before the demagnetisation) that on completing the circuit the magnetic force should be as nearly as possible 1·0 gauss. A piece of platinoid wire included in the circuit enables me to make a fine adjustment of H to its standard value; a simple device allowed this to be done without any risk of breaking the circuit.* The force of 1·0 gauss was now reversed 200 times and then the ballistic effects of a pair of reversals were noted; from them the apparent induction B′ was deduced. This completed the observations for the particular value of X. The whole process, including the demagnetisation, was repeated for each fresh value of X.

When I made these experiments I had no means of measuring the alternating current employed for demagnetising the iron, but from subsequent observations I estimated the maximum magnetic force used in the process at about 4·8 gausses. This was sufficient to give satisfactory demagnetisation.

The results are shown in Fig. 8, and also, in part, with a more open horizontal scale, by the curve "Y = O" in Fig. 9. In Fig. 8 the abscissa is the value of X employed to give the magnetic history, while the ordinate is the value of the apparent induction B′ subsequently found for the standard magnetic force of 1·0 gauss. It will be seen that, until X reaches 1·5 gausses, the fifty reversals of X have no appreciable effect upon the value of B′ for $H = 1·0$. As X increases from 1·5, the value of B′ for $H = 1·0$ diminishes, at first very rapidly, but afterwards more slowly, and when high values of X are reached the value of B′ appears to be nearly constant. The highest value of X used in these experiments was 24 gausses.

The normal induction, B_0, for $H = 1$, was about 2,860, and the apparent induction, B′, for $H = 1$, continued at this value till X reached 1·5, but when X was twenty-four the value of B′ was reduced, to 1,812, *i.e.*, to a little less than two-thirds of B_0.† Notable as is this reduction in the value of B′, the curves of Fig. 7 show that, had the experiment been tried with $H = 0·6$ instead of $H = 1·0$, much more striking results would have been obtained, for in the

* In the experiments done in 1903 I adjusted the current to the values I desired by means of the "Simple Rheostat" which I described in the *Philosophical Magazine*, vol. 6 (6th series), pp. 173–175. The instrument proved itself perfectly reliable, and was very convenient for my purpose, as it enabled me not only to set the current accurately to any given value very quickly but also to correct the small falling off of the current due to the gradual heating of the coils. Its action left nothing to be desired.

† See footnote to § 41.

experiments of §§ 36–38 upon the same specimen, the normal induction for H = 0·6 was 789, while the apparent induction for H = 0·6 after 50 reversals of a force of 8·0 was only 232, *i.e.*, less than one-third of B_0.

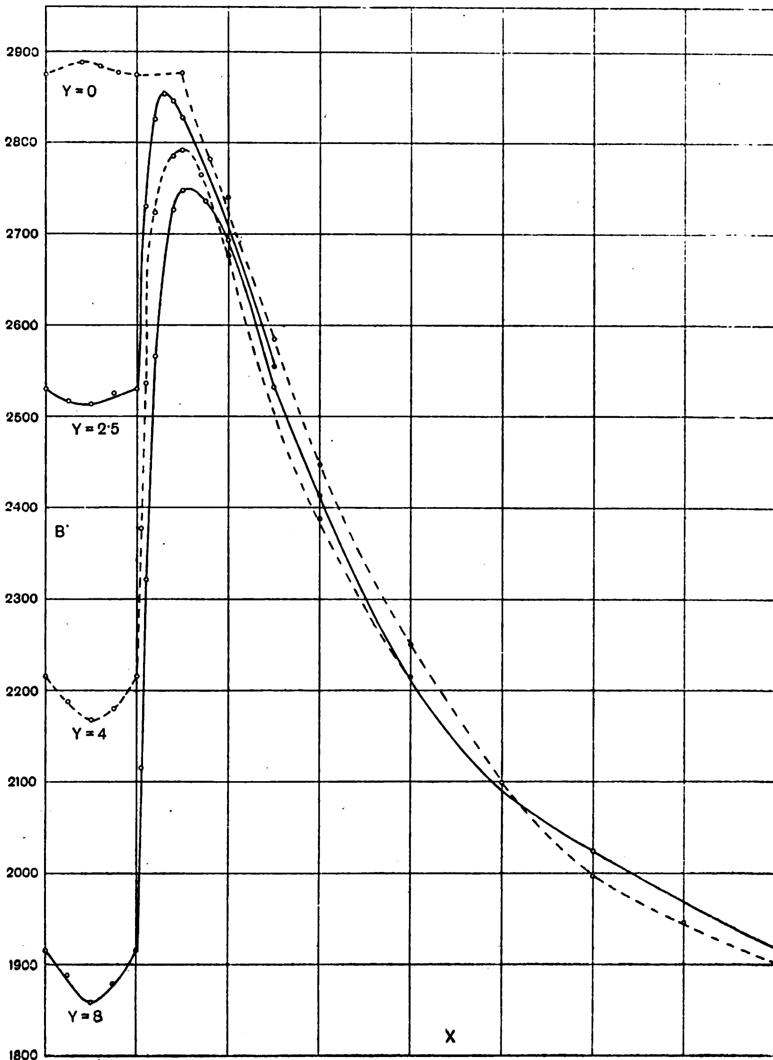


FIG. 9.—"Tagger Plate."

On the other hand, if H had been as great as 2·0, Fig. 7 shows that the effect of 50 reversals of X would have had no sensible effect upon the apparent induction for H = 2·0.

INFLUENCE OF MANY REVERSALS OF A FORCE X UPON THE AFTER-EFFECTS OF A FORCE Y.

§ 41. In all the foregoing experiments the specimen had a magnetic history (§ 9) of many reversals of a single magnetic force X. I now go on to describe a set of experiments, in which, after many reversals of a force Y, the iron was subjected to many reversals of a second force X before the reversals of the force H for which the apparent induction was determined. The 16 strips described in § 29 were again used.

The iron, after demagnetisation, was subjected first to 50 reversals of a force Y, and next to 50 reversals of the second force X, the batteries and resistances having been so adjusted before the demagnetisation as to give the desired values for X and Y. Then, after 200 reversals of a standard force of 1·0 gauss, the apparent induction for H = 1·0 was found. Keeping Y constant, this operation, including the demagnetisation, was repeated for each one of a series of values of X, ranging from zero upwards. A fresh value of Y was then taken, and the whole process was repeated, the standard force H retaining the value 1·0 gauss. Four values were taken for Y, viz., 8·0, 4·0, 2·5 and 0·0 gausses. In the case of the first three values, X ranged only from zero to Y, but in the case when Y was zero, the range of X was from zero to 8·0.

The results of the investigation are shown by the 4 curves * marked "Y = 8," etc., in Fig. 9, where the abscissa represents X and the ordinate B′ for H = 1·0. It will be noticed that, when Y = 0, the experiment is identical with that described in § 40 and recorded in Fig. 8, and in fact the data for the curve marked "Y = 0" in Fig. 9 are the same as those used for Fig. 8.

§ 42. It will be noticed, in Fig. 9, that, when X exceeds 2·0, the curves marked "Y = 8," "Y = 4," and "Y = 2·5" are practically identical with the curve marked "Y = 0." It thus appears that, when X exceeds 2·0, the 50 reversals of X so far wipe out the effects of the 50 previous reversals of the larger force Y that the apparent induction subsequently found for H = 1·0 is practically as great as would have been found if the 50 reversals of Y had been omitted and the only magnetic history (§ 9) had been one of 50 reversals of X. As was shown in § 40, the apparent induction for H = 1·0, found after a magnetic history of 50 reversals of a large force, is much smaller than the normal induction for H = 1·0. But here we see that, when 50 reversals of a sufficiently

* The experiments recorded in Fig. 9 extended over several days, and the values found for the normal induction B_0 with H = 1 differed slightly on different days, but never by so much as 1 per cent. from the value 2,875. The steepest part of the B_0-H curve for this specimen occurs near H = 1, and there the value of dB_0/dH is about 7,000. Thus errors of only 0·4 per cent. in the value of H would account for the discrepancies in B_0, and the effects of variations of temperature upon the millivoltmeter and the resistances used for measuring H (see § 15) may account for at least portions of the discrepancies. To refer all the results to a common standard, I have assumed 2,875 as the true value of the normal induction, and have multiplied all the values of B′ found on any one day by the factor required to bring B_0 for that day to the value 2,875. The small correction thus introduced does not appreciably affect the forms of the curves. The values of B′ shown in Fig. 8 have *not* been corrected in this way.

large force X follow 50 reversals of a larger force Y, the apparent induction for H = 1·0 is raised from the low value occurring after a history of 50 reversals of Y, and is partially restored to the high value reached by the normal induction for H = 1·0. Thus, in the present experiments, the apparent induction for H = 1·0, found after a history of 50 reversals of a force of 8·0, was only 1,900 maxwells per square cm., but when the history was one of 50 reversals of 8·0 followed by 50 reversals of 2·0, the apparent induction for H = 1·0 rose to 2,693, a value but little inferior to 2,740, the apparent induction for H = 1·0 after a history of 50 reversals of 2·0 without the 50 reversals of 8·0.

We notice also that provided X exceed about 1·6, the restoring effect of 50 reversals of X increases as X diminishes.

In the neighbourhood of 1·6 gausses—the critical force for this specimen—the restoring effect of the 50 reversals of X reaches a maximum. The maximum value attained by the apparent induction for H = 1·0 depends upon the value of Y, and diminishes as Y increases.

As soon as X becomes smaller than about 1·6 gausses, a new stage is reached, which lasts till X reaches 1·0. In this stage the restoring effect of 50 reversals of X diminishes very rapidly as X diminishes.

When X was 1·0, there were 50 reversals of Y and then 50 reversals of X (= 1·0) followed by the 200 reversals of the force of 1·0 which were made before the apparent induction for H = 1·0 was found, and, when X was equal to Y, there were 50 reversals of Y and then 50 reversals of X (= Y) followed by the 200 reversals of the force of 1·0.

When X is zero, the apparent induction for H = 1·0 is, of course, identical with that found when, after a history of only 50 reversals of a force equal to Y, there were 200 reversals of a force of 1·0 before the apparent induction for H = 1·0 was found.

Since, after 50 reversals, additional reversals of a given force produce very small effects, it follows that the values of B′ for H = 1·0 are practically the same for X = 1·0, X = Y, and X = 0.

As X diminishes from 1·0 to zero, the apparent induction for H = 1·0 remains nearly constant, though there are traces of a minimum B′, in the neighbourhood of X = 0·4. The minimum value differs but little from the value of B′ when X = 0, but the difference was unmistakable for the two larger values of Y. It is perhaps worth noting that the minimum of μ'/μ_0 in Fig. 7 occurs near H = 0·4.

It is remarkable that the small force 0·4 should increase the after-effects of the larger forces 4·0 and 8·0.

§ 43. If the practical magnetician values the experiments described in §§ 41, 42, it will be chiefly because they aid him in understanding what goes on in the process of demagnetisation and in appreciating the conditions necessary for the success of that process. From these experiments we may conclude that, when iron has been magnetised by a large force, much exceeding the critical force M_0, a few reversals of a force only slightly exceeding M_0 will bring about a great increase in the apparent permeability for small forces. These reversals do not, however, completely restore the apparent permeability to the high value of the normal permeability; they only bring it up (practically) to the value which it would have had if the "history of

the iron had only included the operation of the smaller force. Thus, though the smaller force removes the effects of the larger one, it leaves its own effects behind.

Suppose then that the iron has been subjected to a few reversals of a force X, slightly greater than the critical force. Its apparent permeability for small forces depends (practically) only on X, and not on any other forces which may have preceded X, but it is considerably smaller than the normal permeability. The question is now how to restore completely the permeability to its normal value. It is not sufficient to apply a force somewhat less than the critical one, for Fig. 9 shows that, though this force may effect some improvement, it does not work a complete restoration. It appears that the iron must be subjected to reversals of a force which is reduced by very small stages, so that one value of the force may be practically identical with the critical force. And the reduction of the alternating force must go on by small stages until the force is at least as small as H, the force for which the permeability is to be found. If the demagnetisation be stopped before the force falls to H, the apparent permeability for H may be considerably less than the normal permeability.

Influence of Partial Demagnetisation on the After-Effects of a Large Force.

§ 44. In all the preceding experiments, the maximum magnetic force, D, used in the process of demagnetisation by reversals, exceeded considerably the critical magnetic force, M_0, and the demagnetisation was thoroughly effective, so that, when the process was carried out after many reversals of a large force, it completely wiped out the effect of the large force in reducing the apparent induction subsequently found for a small force.

I now turn to some experiments in which the iron was first subjected to 50 reversals of a force of 8·0 gausses and was then demagnetised by reversals, but not always effectively, the maximum magnetic force employed in the process being D. After this partial demagnetisation, the iron was subjected to 200 reversals of a force of 0·4 gauss, and then the apparent induction for $H = 0{\cdot}4$ was found. These operations were repeated for each of a series of values of D ranging from zero to 4·78.

In these experiments the 16 strips described in § 29 were tested in the magnetic square, and the force 0·4 was chosen because the experiments described in §§ 36–38 showed that for this specimen the effect of 50 reversals of a large force upon the apparent induction for a force H is most strongly marked when H is about 0·4 gauss.

The iron was first effectively demagnetised by reversals, the maximum magnetic force employed in the process being 4·78. It was then subjected to 50 reversals of a force of 8·0 in order to give it a magnetic history, and next it was more or less effectively demagnetised by reversals, the maximum force, D, used in the process, being calculated from the maximum "effective" current C by the formula $D = 106{\cdot}2\ C$ (§ 11). Then, after 200 reversals of the force of 0·4, the apparent induction, B′, for $H = 0{\cdot}4$ was found.

The alternating demagnetising current was taken from the secondary coil of a small transformer with a core built up of a number of armature rings, the primary coil being supplied with current from the laboratory mains. The alternating current could be gradually reduced from C to practically zero by the liquid resistance slide described in § 10. For measuring effective currents from 0·450 down to 0·125 ampere, I borrowed a low reading alternating ampere-meter from Messrs. Millington, Everett and Co., but, for smaller currents down to 0·025 ampere, I varied the number of turns in the secondary coil from 50 to 10, and assumed that the effective current was proportional to the number of secondary turns. I adopted this rough plan because I could not find in Cambridge any commercial alternating amperemeter giving a readable deflexion with less than 0·10 ampere. As a rough justification of the plan I found that with 166 turns the current per turn was 0·0027 ampere, and with 50 turns 0·0025 ampere. In later experiments I used a sensitive electro-dynamometer in the manner described in § 12.

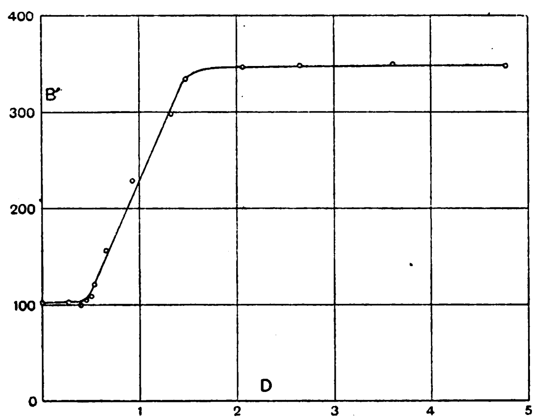


FIG. 10.—"Tagger Plate."

§ 45. The results of the experiments are shown in Fig 10, where the abscissa represents D, the maximum force employed in the more or less effective demagnetisation, and the ordinate represents B′, the apparent induction for H = 0·4 gauss.

The iron was certainly effectively demagnetised by D = 4·78 and the *apparent* induction subsequently found for H = 0·4 was therefore the *normal* induction for H = 0·4; its value was 348·2 maxwells per square centim. When the demagnetisation was totally ineffective, *i.e.*, when D was zero, so that the 50 reversals of the force of 8·0 were immediately followed by the 200 reversals of the force of 0·4, which were made before the apparent induction B′ for H = 0·4 was found, the value of B′ was only 102·0. The value of 102·0/348·2 is 0·293, which is practically identical with the value 0·291 found for μ'/μ_0, when H was 0·4 and X was 8·0, in the experiments of §§ 36–38.

The figure shows that the course of the phenomenon may be divided

into three well-defined stages, the points of transition being approximately $D = 0{\cdot}45$ and $D = 1{\cdot}60$.

In the first stage, from $D = 0$ to $D = 0{\cdot}45$, B' is independent of D, and is equal to the apparent induction found immediately after 50 reversals of the force of 8·0.

In the second stage, from $D = 0{\cdot}45$ to $D = 1{\cdot}60$, the restoring effect of the demagnetisation increases rapidly as D increases; the relation between B' and D in this stage is represented by an approximately straight line.

In the third stage, from $D = 1{\cdot}60$ to $D = 4{\cdot}78$, B' is again independent of D, and is equal to the normal induction. We thus see that *any* value of D greater than 1·60 gives effective demagnetisation, and completely wipes out the effects of a history of 50 reversals of the large force 8·0.

§ 46. On account of the roughness of the measurement of D, we may perhaps identify the value 1·60 with 1·60, the critical force for this specimen, and the value 0·45 with 0·40, the force for which B' was found. If this be allowed, we see, on the one hand, that demagnetisation by reversals is fully effective so soon as D, the maximum magnetic force used in the process, exceeds the critical force M_0, and, on the other hand, that demagnetisation by reversals produces no restoring effect at all and is practically useless unless D exceeds the magnetic force for which the apparent induction is to be found.

It is here supposed that the apparent induction B' is to be found for some force H less than the critical force M_0. When H somewhat exceeds the critical force, the experiments of §§ 36–38 show that a history of 50 reversals of a stronger force have no effect in reducing the value of B' below the normal value B_0.

Effect of a Constant Force X on the apparent Permeability for a Force H.

§ 47. As stated in § 15, the effect of the earth's magnetic force upon the magnetic square was minimised by tilting the square so that its plane was at right angles to the earth's resultant magnetic force. But I found, on trial, that, as near as I could tell, the apparent permeability for a given force was the same in this position of the square as it was when the plane of the square was horizontal and two sides were parallel to the magnetic meridian. The magnetic force which acts along the two sides in question in consequence of the earth's action is difficult to estimate, though it is probably small on account of the demagnetising force called into play when the two sides become magnetised by the earth's force. To gain definite information, I made a series of direct experiments upon the 16 strips of "Tagger Plate," described in § 29, to investigate the effect of a constant force X upon the apparent permeability for a force H. In this work I was assisted by Mr. E. Smart.

Lord Rayleigh* made a similar investigation many years ago, but

* *Phil. Mag.*, vol. 23, p. 242; or *Scientific Papers*, vol. 2, p. 596.

he was chiefly concerned with the case in which X was large and H small, while the initial object of my experiment was the study of the case in which X was small and H considerable.

In our experiments the magnetic force was made to vary between the limits $X+H$ and $X-H$, and half the change of induction due to the step from $X+H$ to $X-H$ or *vice versa*, after 50 cycles, was called the apparent induction for H. The change of induction is that due to a total step of 2H about a mean force X. The change from $X+H$ to $X-H$ was easily made by means of a key similar to that employed by Professor Ewing* for obtaining hysteresis loops; *two* rheostats enabled us to give the magnetic force the values $X+H$ and $X-H$ in the two positions of the key. The magnetic forces $X+H$ and $X-H$ were measured directly by the reading of a single Weston milli-voltmeter. If the square had had *two* primary coils, it would have been better, as Lord Rayleigh pointed out, to produce X by a current in one coil and H by a current in the other coil, measuring the two currents by separate instruments. The lack of a second primary coil on the square prevented us using values of H less than 0·1 gauss.

The results of the experiments are given in the form of curves in Figs. 11, 12. For each curve in Fig. 11, H was kept constant, while X varied. The normal permeability μ_0 for each value of H was taken as the standard of reference for the corresponding curve, and was found by the method of § 13, after effective demagnetisation with $D=14{\cdot}4$ gausses. At this stage X was, of course, zero. When X was finite, the processes performed for each determination of μ', the apparent induction, were (1) preliminary adjustment of the two rheostats, (2) demagnetisation with $D=14{\cdot}4$, and (3) 50 cycles in which the force went from $X+H$ to $X-H$ and *vice versa;* for the sake of definiteness the force $X+H$ was always applied first after the demagnetisation.

§ 48. In Fig. 11 the abscissa is X and the ordinate the value of μ'/μ_0, where μ' is the apparent permeability for H while X is acting and μ_0 is the normal permeability for the same value of H. To avoid confusion, the origin for the curves for $H=2{\cdot}0$ and $H=4{\cdot}0$ is placed at the *right-hand* end of the diagram, and for the sake of comparison the curve for $H=1{\cdot}0$ is shown twice. The smallest value of H for which satisfactory readings were obtained was 0·1 gauss.†

The effect of X in diminishing the value of μ' for a given value of H is the most marked in the case of the curve for which $H=1{\cdot}0$. Here, when $X=0{\cdot}2$, the apparent permeability is less than four-fifths of μ_0, and hence it is clear that, when long thin rods are tested ballistically, care must be taken to place them at right angles to the magnetic meridian.‡ When H is less than 1·0, the effect of X in diminishing μ'/μ_0 becomes less as H diminishes, a result clearly in general agreement with Lord

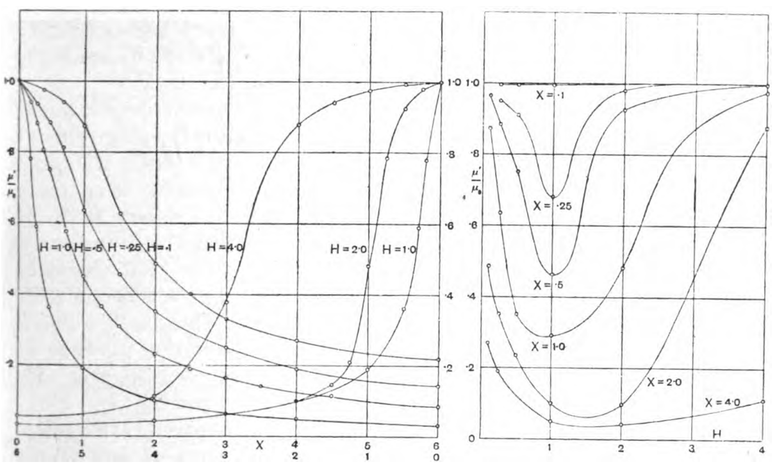
* Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 357.

† In Fig 11, reading from left to right, the values of H are 1·0, 0·5, 0·25, 0·1, 4·0, 2·0, 1·0.

‡ Compare Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, § 41.

Rayleigh's conclusion "that the value of the susceptibility to small changes of force is approximately independent of the initial condition as regards force . . . until the region of saturation is approached." It will be seen from Fig. 11 that values of X less than the critical force 1·6 produce considerable effects upon μ', but any close comparison of our results with those of Lord Rayleigh is difficult, as he does not state the value of M_0 for his specimen of Swedish iron; yet, as he found the value of μ' for $H = 0{\cdot}14$ was independent of X up to $X = 5{\cdot}0$, it is probable that in his case M_0 was considerably greater than 5·0 gausses.

§ 49. The results are exhibited in a different way in Fig. 12, where each curve now refers to a constant value of X, and the abscissa represents H. In each curve μ'/μ_0 has a minimum value in the neighbourhood of $H = 1{\cdot}6 = M_0$. For the smaller values of X, the form of the curves



FIGS. 11 and 12.—"Tagger Plate."

for small values of H suggests that μ' is probably equal to μ_0 for infinitely small values of H.*

On account of the interest attaching to this conclusion, if true, I made an attempt, with Mr. C. Chittock, to test it when H was 0·005 gauss. For the specimen I used the core of the choking coil, used by Mr. Bedford and myself in a previous research.† The core was furnished with two primary coils and a large number of secondary windings, and, as the section of the iron was 34 square centims., no difficulty was experienced through lack of sensitiveness of the apparatus. But the induction in some cases responded so slowly to the steps from X + H to X − H and from X − H to X + H, that I did not obtain any definite results as to the effect of X upon the value of μ'.

* In Fig. 12, reading downwards, the values of X are 0·1, 0·25, 0·5, 1·0, 2·0, 4·0.

† *Phil. Trans. Royal Soc.*, vol. 198 A, p. 58.

Experiments on Virgin Iron.

§ 50. In all the experiments hitherto described in this paper, the effects could be reproduced, as often as was desired, by effectively demagnetising the iron before subjecting it to the magnetic process, the effects of which I wished to study. I now come to some experiments which can be made only once upon a given specimen, for the reason that they require that the iron should never have been magnetised—at least not since its annealing.* This is the theoretical specification; in dealing with specimens cut from transformer sheets in their commercial condition, we must be satisfied to specify that the iron has not been subjected, since its annealing, to any magnetic force exceeding that due to the terrestrial magnetism; the maximum resultant force due to this cause in England is rather less than 0·5 gauss.

These experiments show that the normal permeability for a piece of iron, found (as the definition of "normal" requires—see § 6) after effective demagnetisation, is, for low values of H, much higher than the permeability which is found when the iron has never since its annealing been exposed to any magnetic force greater than H.

When the iron has never, since its annealing, been subjected to any magnetic force greater than the force H for which it is being tested, I shall, for the sake of brevity, speak of it as being in a virgin state, and shall call the value of the apparent induction, found after many reversals of H, the *virgin induction* for H, and shall denote it by B_v. The quantities μ_v, I_v and κ_v are defined in a similar way. It is necessary, in order to secure definiteness, to specify that there shall be many reversals, for, when H is small, the ballistic effect of a reversal of H diminishes rapidly during the first few reversals. Thus, with a specimen of soft iron wire, Mr. T. G. Bedford and I found that for $H = 2·5$ the average change of induction for the first pair of reversals was 2,220 and for the forty-first pair 1,840, a diminution of 17 per cent.†

In the earlier experiments in this direction I compared (1) magnetometrically, and (2) ballistically the forms of (1) the κ–H and (2) the μ–H curve for virgin iron and for the same iron after effective demagnetisation. In later experiments I chose a definite magnetic force H, and I then varied D, the maximum force employed in the demagnetisation, and watched the effect of a gradual increase in D in changing the apparent permeability from its value for the virgin iron.

Magnetometric Study of the Action of Effective Demagnetisation on Virgin Iron.

§ 51. In the magnetometric experiments I employed the same apparatus, and used a piece of the same iron wire as in the experiments of §§ 23–27, and I made a preliminary note of the batteries and

* From the footnotes to §§ 55, 5 it will be seen that both the late Professor Rowland and also Professor Ewing noticed long ago that virgin iron is in a peculiar magnetic state, which is altered as soon as the iron has once been magnetised. But I have not found any record of any systematic examination of the question.

† *Phil. Trans. Royal Soc.*, vol. 198 A, p. 71 1902.

resistances required in order to obtain a number of convenient values of the magnetic force. The wire was first annealed by heating it in a flame to a bright red heat. It was then put into the solenoid when no current was flowing and was found to be very slightly magnetised, although care had been taken to keep it, during and after the annealing, at right angles to the earth's resultant force; the small resulting deflexion was compensated by means of a small magnet. The circuit was then completed and a force of 0·5 gauss was applied, and, after this had been reversed 50 times, the observations for the virgin intensity of magnetisation for $H = 0·5$ were made. A force of 1·0 was then applied and was reversed 50 times before the virgin intensity for $H = 1·0$ was found. This process was continued step by step for larger and larger forces, care being taken never to expose the iron to any force greater than that for which I_v was to be found. The resulting virgin susceptibility curve is shown in Fig. 13 by the dotted line marked "κ_v."

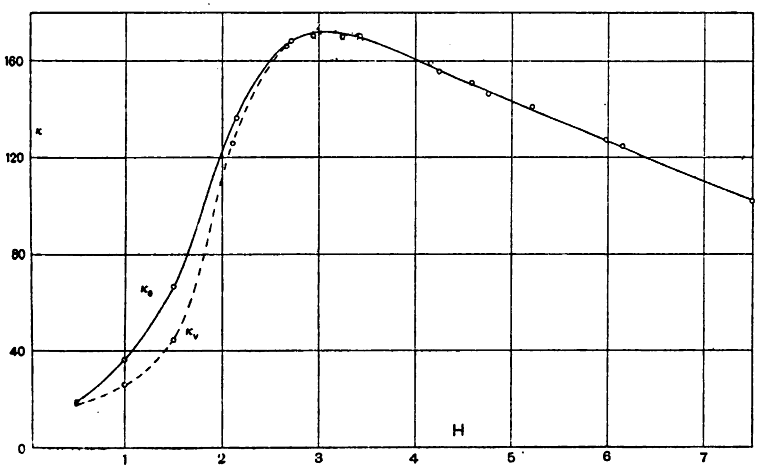


FIG. 13.—Iron Wire.

The iron was then demagnetised by reversals, the maximum force used being about 10 gausses; the magnetometer showed that the wire was left quite free from magnetism. The wire was next subjected to 50 reversals of a force of 0·5 and then the normal intensity of magnetisation (§ 8) for $H = 0·5$ was found. This process, including the demagnetisation, was repeated for each value of H employed. The resulting normal susceptibility curve is marked "κ_0" in Fig. 13.

For the small forces the curve for the virgin iron lies considerably below that for the demagnetised iron. The value of κ_0/κ_v reaches a maximum of 1·5 when H is about 1·5 gausses. As H increases from 1·5 the two curves draw more and more closely together and begin to coincide just before H reaches the critical value 3·1, for which κ_0 is a maximum. From this point up to $H = 7·5$, the highest force used, the two curves are indistinguishable.

Ballistic Study of the Action of Effective Demagnetisation on Virgin Iron.

§ 52. A set of ballistic experiments, analogous to the magnetometric set described in § 51, was made upon four strips of transformer iron. The strips, which were 4 cm. wide, were cut from wider strips of Schultz iron (§ 14).

Preliminary tests were made to find the batteries and resistances required for definite magnetic forces such as 0·25, 0·50 . . . gausses.

The strips, which had never been magnetised by any force exceeding the earth's force, were fitted into the magnetic square (§ 15), the circuit being broken. The resistances, &c., were then so arranged that a force of 0·25 gauss was applied on completing the circuit. After this force had been reversed 200 times, the ballistic effects of a pair of reversals were observed, and from them the virgin induction B_v and the virgin

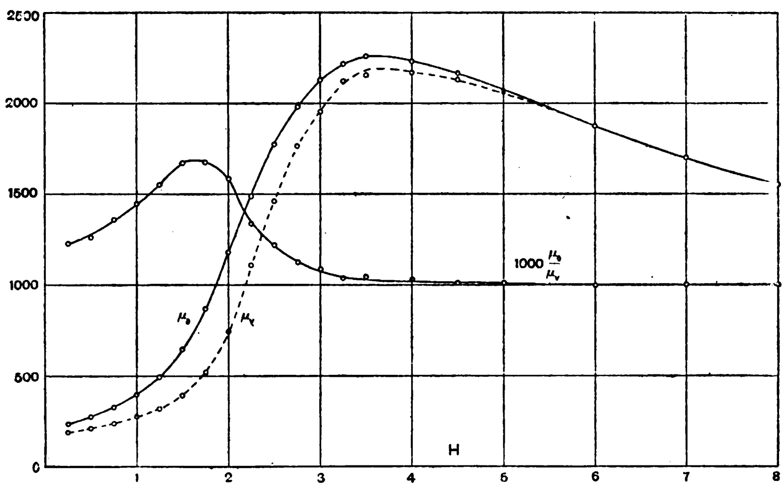


Fig. 14.—Schultz Iron.

permeability μ_v for $H = 0·25$ were found. The force was then increased to 0·50 and, after 200 reversals, B_v and μ_v for $H = 0·5$ were found. This process was continued step by step up to $H = 8·0$, care being taken never to expose the iron to any force greater than that for which B_v was to be found. The resulting virgin permeability curve is shown in Fig. 14, where it is shown by the dotted line marked "μ_v."

The iron was now demagnetised by reversals, the maximum force used in the process being 7·75 gausses; this considerably exceeded 3·60, the critical force for this specimen, and so caused effective demagnetisation. The normal induction, B_0, was then found for a series of values of H in the manner described in § 13. The resulting normal permeability curve is shown in Fig. 14, where it is marked "μ_0."

As in the magnetometric experiments (§ 51), the curve for the

demagnetised iron lies above that for the virgin iron for small values of H. This action of effective demagnetisation in raising the permeability of virgin iron was verified for other specimens of transformer iron, as will appear in Figs. 15 and 20 below. To illustrate the relation between the two curves I have plotted a third curve, in which the ordinate represents 1,000 μ_o/μ_v.

The value of μ_o/μ_v reaches a maximum of 1·65 when H is about 1·65 gausses, and afterwards falls off as H increases. The quantity begins to reach the constant value 1·0 when H is about 5·5, a value somewhat greater than the critical force 3·60. Thus, when H is greater than 5·5, there is no advantage in demagnetising the iron, for we obtain the same value for the induction whether the iron is in the virgin state or whether it has been demagnetised.

We have already seen that, when H exceeds the critical force M_o, the apparent induction is independent of the magnetic history (§ 9) of the specimen and is equal to the normal induction. The present experiments serve to show still more plainly than before that the behaviour of iron under the action of the forces greater than M_o differs widely from its behaviour under the action of forces less than M_o. When H exceeds M_o, B′ seems to depend only on H, but, when H is less than this value, B′ is greatly influenced by many circumstances.

§ 53. It is evident from the experiments of § 52 that a permeability test of virgin iron, in which H is increased step by step from zero, is of little practical value, for it can never be repeated. What is required is a method which will always give the same value for B′ for a given value of H, and thus will furnish us with a value for B′ which is a function of H only and not of the previous magnetic treatment of the iron. The experiments already described in this paper make it, I think, abundantly clear that the simple method of effectively demagnetising the specimen before each test of permeability satisfies every demand the practical magnetician can reasonably make.

Experiments on Virgin, Demagnetised and Magnetised Iron.

§ 54. In the experiments of § 36, which were made upon the 16 strips of "Tagger Plate" described in § 29, I compared the apparent permeability for a variable force H, found after a history of many reversals of a constant strong force X, with the normal permeability, recording the results in Fig. 6. In another set of experiments, made upon four strips of Schultz iron, and described in § 52, I compared the normal permeability with the "virgin permeability" (§ 50), recording the results in Fig. 14.

An experiment of the late Professor H. A. Rowland led me to see that it would be of interest to repeat these observations, using, however, the same specimen throughout.

In carrying out this project, Mr. E. Smart and I tested four strips of the Schultz iron in the magnetic square, and first made observations for the "virgin permeability" in the manner described in § 52, except that we only made 50 reversals of H instead of 200. The resulting permeability curve is shown by the dotted line marked "μ_v" in Fig. 15,

For the purpose of these experiments, we adjusted a coil of copper wire to the same resistance as that of the primary coil of the magnetic square. By means of a two-way key the current could be made to pass either through the magnetising coils or through the resistance coil. This device was very convenient, for we were able to "set" the current to the desired value, without magnetising the iron, by simply substituting the resistance coil for the magnetising coil by means of the two-way key, and thus the preliminary tests described in § 52 were rendered unnecessary.

We then made observations for the normal permeability curve by the method of § 13, making 50 reversals of H at each stage, and obtained the curve marked "μ_0" in Fig. 15. The maximum magnetic force (D) used in the demagnetisation was found by the electro-

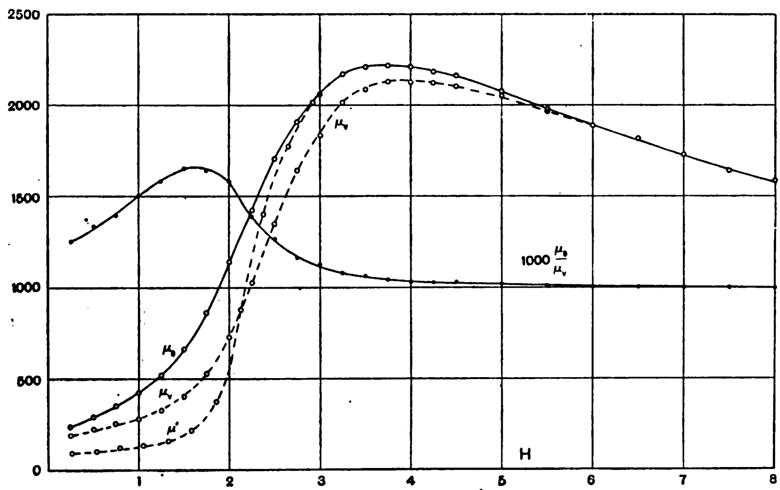


FIG. 15.—Schultz Iron.

dynamometer* to be about 16·4, a force much greater than the critical one of 3·75 gausses.

The dotted curve marked "μ'" in Fig. 15 was obtained in the manner described in § 36. For each value of H the iron was first demagnetised with D = 16·4, and was next subjected to 50 reversals of a constant force of 8·0 gausses. Then the force H was applied, and after 50 reversals, B′ was determined.

The general relation between the μ_0 and the μ_v curves for the Schultz iron was noticed in § 52; as in Fig. 14, I show 1,000 μ_0/μ_v.

Just as in Fig. 6, the curve for μ' lies belows that for μ_0 for the smaller values of H, but the two curves become identical for larger forces. In the present case the curves begin to coincide *before* μ_0 reaches

* In these experiments the alternating current was measured in the way described in § 12. But now we had R = $\frac{1}{10}$ ohm and S = 11,000 ohms, so that the corrections arising from the coefficients of self and mutual induction of the suspended coil were quite inappreciable.

its maximum, so that they are similar to the curves marked "X = 2·5" in Fig. 6.

The curve for μ' also lies below that for μ_v until H reaches 2·1; but from this point μ' is greater than μ_v. The two curves begin to coincide when H is about 6·0, so that from this point onwards all three curves coincide.

ROWLAND'S EXPERIMENTS.

§ 55. This seems the most fitting place to notice a result obtained by the late Professor H. A. Rowland. He made ballistic tests on a number of rings of iron, and remarks:* "To get the normal curve of permeability, the ring must only be used *once;* and then no more

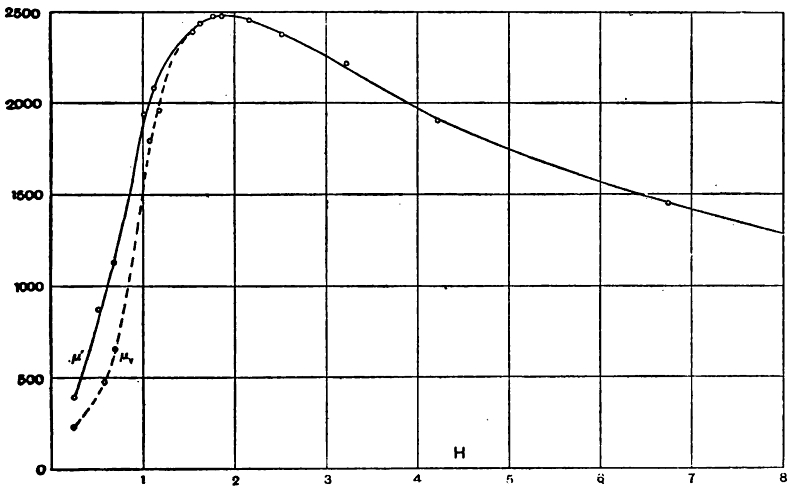


FIG. 16.—Rowland's Experiment.†

current must be allowed to pass through the helix than that with which we are experimenting at the time. If by accident a stronger current passes, permanent magnetism is given to the ring, which entirely changes the first part of the curve." Here it must be remembered that what Rowland calls the *normal* permeability I have called the *virgin* permeability.

From Rowland's numbers I have plotted the two curves in Fig. 16, marking them "μ_v" and "μ'" respectively.†

It will be seen that the dotted "magnetic" curve (μ') lies below the "virgin" curve (μ_v) until they coincide. Rowland's curves do not cross each other as my curves do; but any close comparison is useless, since Rowland gives no details of his method of obtaining the curves, and since the magnetic qualities of the "Burden best" iron used by him probably differed considerably from those of my Schultz iron.

* H. A. Rowland, *Phil. Mag.*, vol. 46, p. 149, or *Physical Papers*, p. 44. The experiments of §§ 51, 52 were done before I noticed this remark of Rowland's.

† In Fig. 16, μ' and μ_v should be interchanged.

Influence of Partial Demagnetisation on the Permeability of Virgin Iron.

§ 56. As the experiments described in §§ 51–53 showed that effective demagnetisation causes a very considerable increase in the permeability of virgin iron for small magnetic forces, it seemed of interest to trace the effect of a gradual increase in D, the maximum force used in the demagnetisation, upon the permeability for some constant small magnetic force. Mr. T. G. Bedford kindly assisted me in these experiments.

Since in experiments with virgin iron any false step is irretrievable, we employed a special device which enabled us to have the maximum amplitude of the alternating magnetic force under complete control. The alternating current for demagnetising was obtained by connecting the magnetising solenoids with a secondary coil wound upon an ebonite tube together with a primary coil, which was supplied with current from the laboratory mains. The effective value of the alternating current flowing through the solenoids was measured by our sensitive electrodynamometer in the manner described in § 12, the value of R being $\frac{1}{10}$ ohm, while that of S varied from 73 to 991 ohms. To obtain any definite value of D, we first calculated the corresponding deflexion of the dynamometer and then slowly inserted iron rods into the ebonite tube until the deflexion reached the calculated value.

In these experiments we used four fresh strips of Schultz iron (§ 14), and tested them in the magnetic square, the induction being found for the standard force of 1·5 gausses. We chose this value because we found, in the experiments of § 52, that for four other strips of the same brand the ratio of the normal to the virgin permeability reached a maximum when H was about 1·65.

The strips were put into the square when no current was flowing. The circuit was then completed and a force of 1·5 was applied. After this had been reversed 100 times the ballistic effects of a pair of reversals of the force were observed, and from them the value of B′ for H = 1·5 was found.* The connexions were now changed so as to allow the alternating current to flow through the solenoids, and an iron rod was slowly inserted into the tube transformer until the dynamometer indicated that D, the amplitude of the magnetic force, had reached 0·5 gauss. The rod was then slowly drawn out of the tube, and the current was further reduced to zero by means of the liquid resistance slide (§ 10). The connexions were next rearranged and the force of 1·5 was applied and reversed 100 times before the observations for B′ were made. The iron was again demagnetised, this time with D = 0·75, and then B′ was found for H = 1·5. This process was repeated, D being increased step by step up to 5·3. The resulting B′–D curve is shown in Fig. 17, where (for this curve) the abscissa represents D and the ordinate the value of B′ for H = 1·5.

Observations were then made for the normal permeability curve in the manner described in § 13. The iron was demagnetised with D = 4·43, and at each step 100 reversals of H were made before B_0 was

* In this single case, in which D = 0, B′ is identical with B_v.

found. The resulting μ_0–H curve is shown in Fig. 17, where (for this curve) the abscissa represents H and the ordinate μ_0.

The B′–D curve shows that the course of the phenomenon may be divided into three stages. In the first stage, from $D = 0$ to $D = 1{\cdot}0$, the demagnetisation has no effect, the apparent induction B′ retaining the virgin value 775. In the second stage, from $D = 1{\cdot}0$ to about 3·2, B′ increases with D, the rate of increase becoming smaller as D approaches 3·2. In the last stage, from $D = 3{\cdot}2$ onwards, B′ remains constant and is equal to 1,240, the value of the normal induction B_0. Since μ_0 attains its maximum value when $H = 3{\cdot}25$, we see that the demagnetisation brings the virgin specimen to the normal state as soon as D reaches M_0, the critical force for which μ_0 is a maximum.

The B′–D curve shows that the demagnetisation raises the apparent permeability for $H = 1{\cdot}5$ above the virgin value before D reaches 1·5; this result was obtained with two different specimens of Schultz iron.

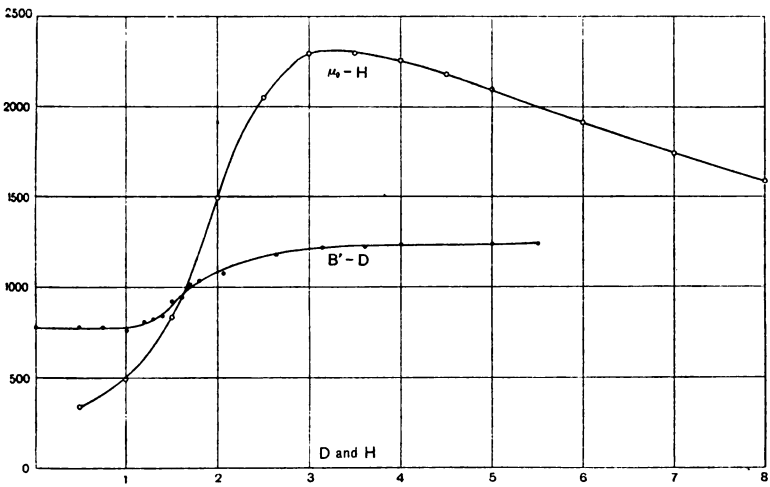


FIG. 17.—Schultz Iron.

The value of D is, of course, a little uncertain, as it depends upon the wave-form of the alternating current,* but it is unlikely to be in error by 50 per cent., and thus it is probable that very numerous reversals of a force less than 1·5 may raise the apparent permeability for $H = 1{\cdot}5$ above its virgin value. If this conclusion be true, the definition of the "virgin state" given in § 50 would require revision.

CONSTANCY OF THE SPECIMENS.

§ 57. As my experiments have extended over several months, I thought it would be well to take the opportunity of testing whether the normal permeability of the specimens had changed appreciably

* With reference to the measurement of D, it is perhaps worth while to record the fact that it was only in the experiments of § 56 that the transformer employed had an *unclosed* iron circuit.

with lapse of time, and two specimens were tested for this purpose. One specimen consisted of the 16 strips of "Tagger Plate," described in § 29, and two permeability tests were made on October 7, 1902, and on August 10, 1903. The other specimen consisted of four strips of Schultz iron, and the two tests were made on October 23, 1902, and on August 11, 1903. In the interval between the two tests on each specimen, the strips were subjected only to the temperature of the laboratory, and were not bent or otherwise roughly handled.

The Weston instrument used in measuring the magnetising current was standardised by Elliott Brothers on July 12, 1902. To test its constancy, I compared it, in August, 1903, with a Siemens and Halske instrument, which was standardised by Siemens Brothers on March 11, 1903, and found that the readings of the two instruments agreed to about $\frac{1}{10}$ per cent. We may therefore assume that the sensitiveness of the Weston instrument did not change appreciably in the course of a year.

The results of the experiments are given in the following tables:—

"TAGGER PLATE."

H.	μ_0. 1902.	μ_0. 1903.	H.	μ_0. 1902.	μ_0. 1903.
·2	579	589	1·6	4,212	4,243
·4	905	876	2·0	4,113	4,128
·6	1,308	1,258	3·0	3,436	3,450
·8	1,974	1,929	4·0	2,868	2,893
1·0	2,953	2,901	5·0	2,457	2,479
1·2	3,687	3,714	6·0	2,140	2,161
1·4	4,107	4,107	8·0	1,705	1,714

SCHULTZ IRON.

H.	μ_0. 1902.	μ_0. 1903.	H.	μ_0. 1902.	μ_0. 1903.
·5	464	467	3·5	2,780	2,813
1·0	765	768	4·0	2,628	2,651
1·5	1,466	1,491	5·0	2,318	2,349
2·0	2,400	2,443	6·0	2,058	2,063
2·5	2,850	2,881	7·0	1,836	1,855
3·0	2,850	2,908	8·0	1,655	1,667

The numbers of 1902 and 1903 are practically identical, though apparently the permeability of the "Tagger Plate" for small magnetic forces has diminished slightly. There is no evidence of any changes approaching that found by G. Stern* in the case of a specimen made up of rings stamped out of sheet iron. He found that though the specimen was kept as a "control," for the purpose of comparison, and was only subjected to the temperature of the laboratory, its permeability, for $H = 1{\cdot}257$, fell from 1,880 to 960 in the course of seventeen months.

The Preparation of Specimens.

§ 58. In the practice of magnetic testing the preparation of specimens for testing has naturally a very important place. The thin sheet iron manufactured for the construction of transformers is rolled in plates about 2 metres long by 1 metre wide, and is afterwards annealed by the maker. Unless the whole sheet be tested at once,† part of the sheet must be brought into a form suitable for ballistic tests. This is generally done by stamping rings out of the sheet and building up a test-piece out of several rings. But it has been recognised that the mechanical disturbance caused by the process of stamping has a considerable effect in lowering the permeability of the specimen, and thus it is usual to anneal the rings after stamping, along with a batch of plates of the same brand, in the hope that tests on the ring will furnish information as to the magnetic qualities of the batch of plates.

Though, as my own experiments show, the mechanical treatment involved in the preparation of the specimen may have a great influence upon the magnetic quality of the specimen, I have found scarcely any definite information on this point in the papers of previous workers. It seemed, therefore, profitable to examine how the normal permeability curve is affected by mechanical operations of the kind that

* G. Stern, "Über das Altern deutscher Eisenbleche," *Electrotechnische Zeitschrift*, vol. 24, p. 407.

See also a letter from Professor J. A. Ewing, *The Electrician*, vol. 49, p. 684.

† Rudolph Richter, in an article on "Eisenprüfapparat für ganze Blechtafeln" (*Electrotechnische Zeitschrift*, vol. 24, p. 341), describes the apparatus constructed by Siemens and Halske for testing whole sheets of transformer iron. In principle, the magnetising coil is uniformly wound on a hollow ring of rectangular section, the axial length being rather more than 1 metre, and the radii of the two cylindrical surfaces about 30 and 35 cm. respectively. A slit is cut along one of the generating lines of the outer cylindrical surface, and through this slit the sheet to be tested is inserted, so that it is bent into the shape of a cylinder 1 metre in length and 2 metres in circumference. In this way a magnetic circuit is formed with the single sheet. An alternating current is used, and the induction is found from the readings of a voltmeter, while the loss of energy through hysteresis is measured by a wattmeter. The apparatus is very suitable for testing sheets of iron before they are cut into the forms required for building up transformer cores, as it enables inferior sheets to be rejected. The only objection is that, as the sheets are bent in being placed in the apparatus, their permeability will be slightly reduced. I have to thank Mr. Richter for very full information concerning the apparatus, and also the firm of Siemens & Halske for generously offering to place a set of apparatus at my disposal.

naturally occur in the preparation of specimens for testing. In these experiments I employed two brands of transformer iron, viz. "Schultz" and "Sankey" (§ 14), and I examined the effects arising from the process of cutting the iron into strips, as well as the effects due to roughly handling the strips after they had been cut.

§ 59. To study the effects of cutting the iron, I first cut, by means of hand shears, four strips 4 cm. wide out of 2 strips, each 10 cm. wide. A piece 1 cm. wide was cut off each edge of the 10 cm. strip, and the remaining 8 cm. strip was cut into two equal portions. The four strips so obtained were then tested for normal permeability. For each

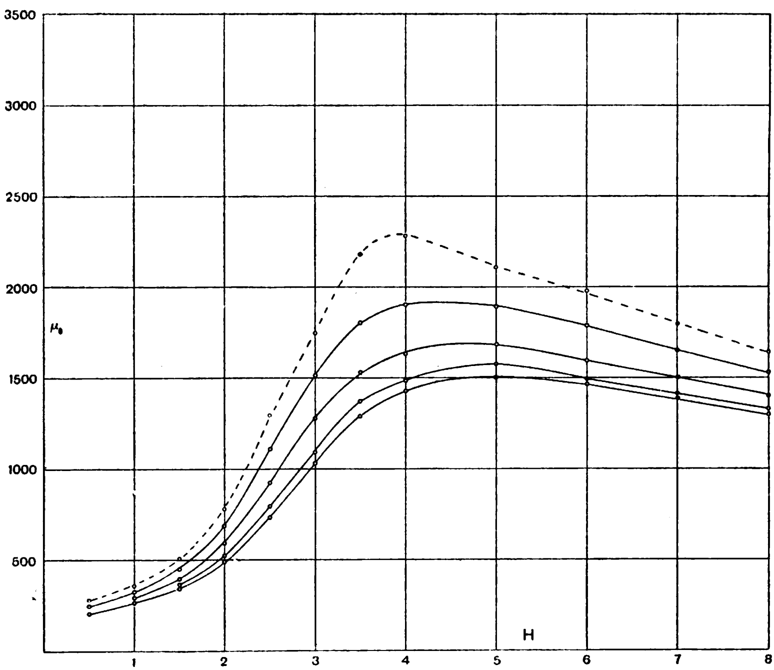


FIG. 18.—Schultz Iron.

value of H the iron was demagnetised with D = 16·4, and was then subjected to 50 reversals of H. The strips were next taken out of the square, and one cut was made along each strip to within 3 cm. of one end. The strips were then straightened only just enough to allow them to be replaced in the square, and a small piece of transformer sheet was placed at each corner of the square in order to improve the magnetic contact. The strips were then retested for normal permeability, and the process was repeated for each successive cut. In the case of the Schultz iron each strip was cut three times—first along the central line, and then once on either side of this line, so that

finally each strip had four fingers, each 1 cm. wide. The Sankey strips were cut only twice, each cut being $1\frac{1}{3}$ cm. from an edge.

The resulting μ_0–H curves are shown as continuous lines in Fig. 18 for the Schultz iron, and in Fig. 19 for the Sankey iron.

Each fresh cut lowers the normal permeability. In the case of the Sankey iron, when H exceeds 3 gausses, each cut has about the same effect, but with the Schultz iron the second and third cuts have much smaller effects than the first cut. It thus appears that with the Schultz iron the disturbance arising from the cutting extends somewhat more than $\frac{1}{2}$ cm. on either side of the cut. This is not surprising,

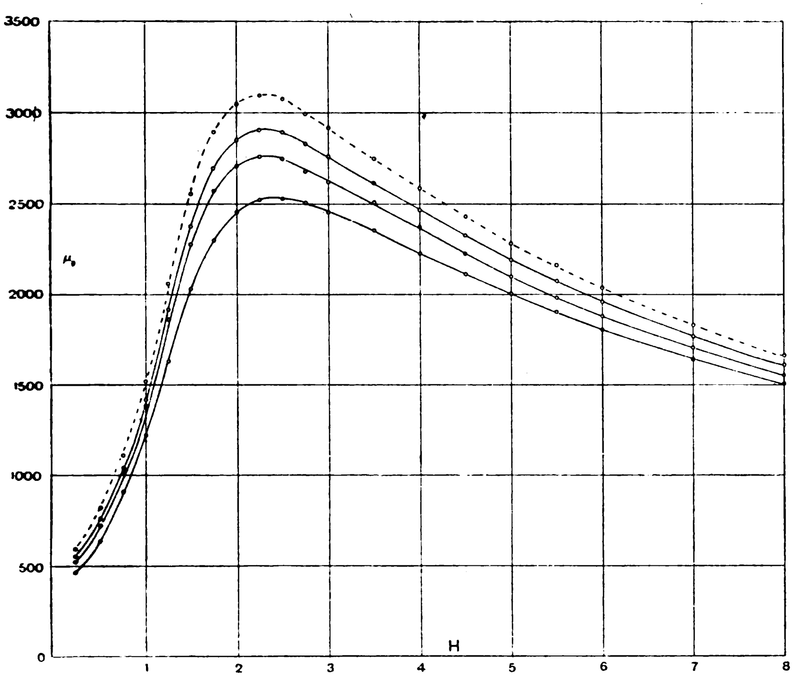


FIG. 19.—Sankey Iron.

for as the iron is cut by the shears the eye can easily detect a mechanical disturbance of the plate extending to a distance of something like $\frac{1}{2}$ cm. from the cut. On the other hand, with the Sankey iron, it appears that the disturbance does not extend to a distance of $\frac{2}{3}$ cm. from the cut.

If we assume that the effects of cutting do not extend to a distance of 1 cm. with the Schultz iron, or of $\frac{2}{3}$ cm. with the Sankey iron, we can use the curves of Figs. 18, 19 to enable us to find the true form of the μ_0–H curve. We have only to add to the value of μ_0 for the highest curve the difference between that value and the value of μ_0 for

the next lower curve, and I have done this for the Schultz iron. For the Sankey iron it seemed fairer to add to μ_0 for the highest curve half the difference produced by the two cuts. The two corrected μ_0–H curves are shown in Figs. 18, 19 as broken lines. The result of the

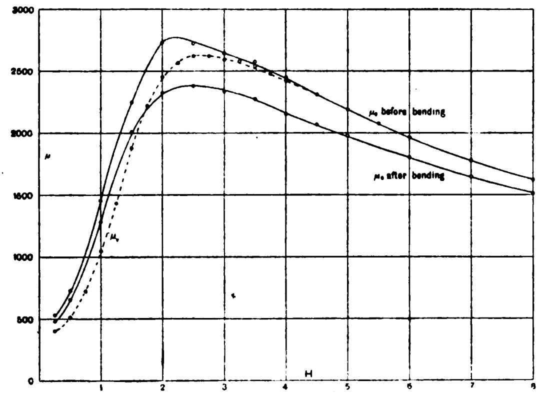


FIG 20.—Sankey Iron.

experiments shows that the process of cutting the sheet produces a serious change in its permeability.

§ 60. To study the effects of roughly handling the strips after they have been cut, I took 4 strips (*a*) of Sankey iron and (*b*) of Schultz iron

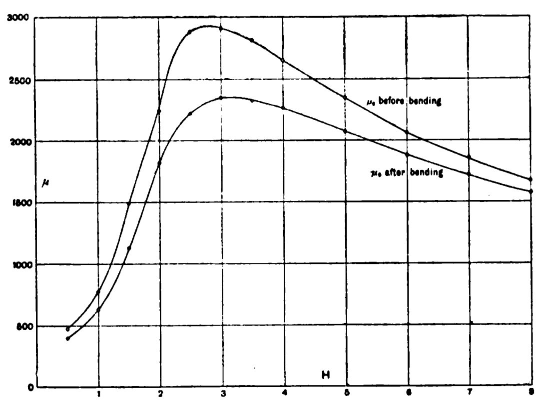


FIG. 21.—Schultz Iron.

and tested them for normal permeability, making 50 reversals of H after demagnetisation with (*a*) D = 16·4 and (*b*) D = 9·1 gausses. The strips were then bent round a cylinder of 5·6 cm. radius, and when they were taken off the cylinder they remained curved, having a radius of about 9·4 cm. After the strips had been straightened by stroking

them on a table with a piece of soft wood, they were put back into the square and were re-tested. The μ_0–H curve for the Sankey strips is shown in Fig. 20, and for the Schultz strips in Fig. 21. To make the record more complete I have added the virgin curve for the Sankey iron as a dotted line in Fig. 20; the observations for this curve were, of course, made before the strips were bent.

It will be seen that the bending has resulted, in each case, in a large decrease of permeability. It is thus evident that unless transformer sheets are handled carefully, their magnetic quality may be seriously damaged.

The relation of the virgin curve to the normal curve for the Sankey iron is of the same character as that found in the case of the Schultz iron in the experiments of § 52.

Want of Uniformity of Specimens.

§ 61. In most of the experiments described in this paper, it is the magnetic quality of particular specimens which has been the object of study. But when we consider the place which magnetic testing should have in the selection of iron for transformer building, we see at once that, unless makers of transformer iron can supply sheets which are all of a uniform quality, the information furnished by a magnetic test of a few hundred grammes of the material cannot be relied on to give a good idea of the average quality of the great mass of iron used in building a large transformer. It is therefore of great importance to examine the variations of magnetic quality which may occur among specimens of the same brand of iron. This examination would be most properly carried out at the rolling mills, where it would be possible to trace the history of each sheet sent to the testing-room. Though my own experiments perforce fall far short of this ideal, they may perhaps indicate the character of the results likely to be obtained by one whose office it is to test samples of iron submitted by the manufacturers.

§ 62. My attention was drawn to the matter by noticing that, with the first set of strips tested in the magnetic square, the magnetic forces, arising from magnetic leakage at the corners of the square, were greater than I had expected. As it was obvious that the magnitude of these forces, and therefore the accuracy of the results furnished by the square for a given set of strips, depended largely upon the uniformity of those strips, I examined a large number of strips of "Tagger Plate," of Schultz, and of Sankey iron for uniformity of magnetic quality.

As each strip was cut off, a number was stamped upon it and the strip was then placed in one solenoid of the square, the magnetic circuit being completed by three other strips of the same brand. These last three strips remained unchanged during each set of experiments. Only one secondary coil was used, and this, of course, encircled the strip under test. After effective demagnetisation, a suitable magnetic force, H, was reversed several times, and then the

ballistic effects of a pair of reversals of H were noted. As this process was repeated for each strip it was found that the ballistic effects varied considerably and far more than could be accounted for by the slight variations existing among the masses of the strips. It is evident,

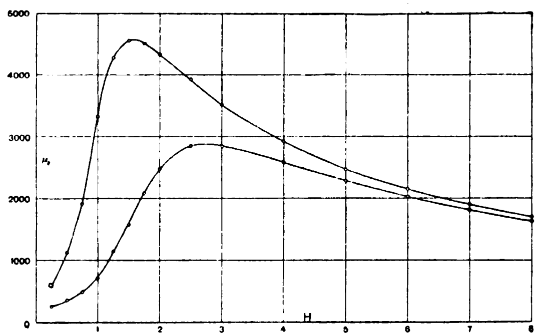


FIG. 22.—"Tagger Plate."

moreover, that the association of the three auxiliary strips with those under test tends to diminish the variations in the ballistic effect for that strip.

By this examination it was easy to select, from the strips tested,

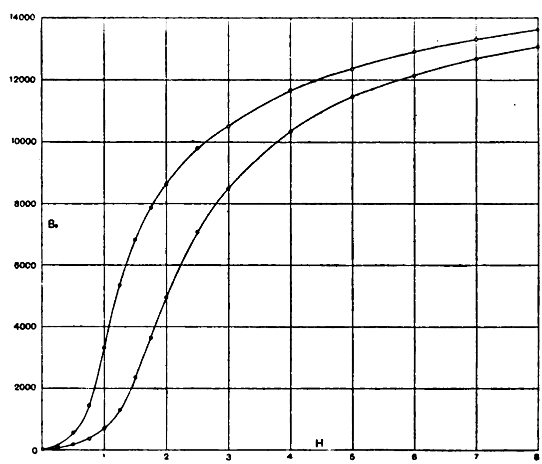


FIG. 23.—"Tagger Plate."

the four of highest permeability and the four of lowest permeability for each brand. The four most permeable strips were then placed in the magnetic square, and were tested for normal permeability, and the process was repeated for the four least permeable strips. The result-

ing μ_0–H curves are shown in Fig. 22 for the "Tagger Plate," in Fig. 24 for the Schultz iron, and in Fig. 25 for the Sankey iron. In the case of the "Tagger Plate," the B_0–H curves are also given in Fig. 23. For the Schultz iron three curves are given; the two highest were obtained in 1902 from two sets of strips selected out of twelve strips, while the lowest was found in 1903 for the four strips used in the experiments of § 59 before they were cut.

The mass of each set of strips was found as well as the average length, and from these quantities and from a knowledge of the density of the iron (*assumed constant for a given brand*) the average cross-section of the strips of each set was calculated.

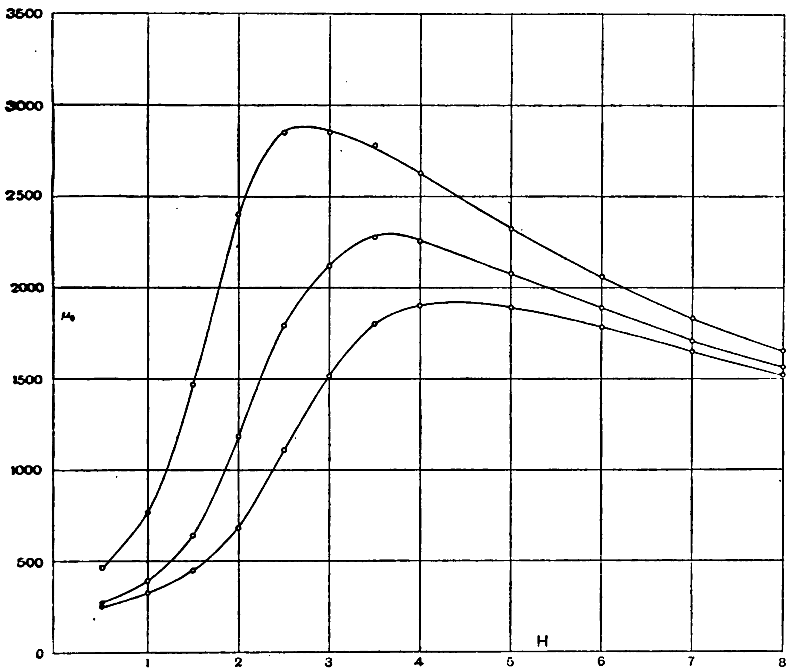


FIG. 24.—Schultz Iron.

In the results recorded in Figs. 22, 23, 24, 25, a proper allowance has been made for the variations of thickness of the sets of strips, but it is perhaps worth while to record those variations. The average thickness of the best "Tagger Plate" strips was 0·0336 cm., and of the worst 0·0331 cm. The best Schultz strips were 0·0342 cm. thick, the second best 0·0367 cm. thick, and the worst 0·0343 cm. thick. The thickness of the best Sankey strips was 0·0381 cm. and of the worst 0·0345 cm.

§ 63. The curves of § 62 show that very serious differences of magnetic quality may exist among transformer plates of the same brand. Differences of magnetic quality might be expected among

separate sheets, but the experiments made for the purpose of selecting the strips of highest and of lowest permeability showed that the permeabilities of two strips which lay side by side in the sheet may differ by as much as 10 per cent. This result is quite in accord with the experiments of G. Stern,* who investigated by Ewing's "Hysteresis Tester" † the hysteretic quality of small samples taken from various parts of a single large sheet, and found that large differences existed. It is therefore not surprising to find great variations among strips which have, possibly, been cut from different sheets of the same brand.

Each set of curves possesses the feature that the higher the maximum value of μ_0 the smaller the critical magnetic force M_0 at which the maximum occurs. Indeed, the product of the maximum value of μ_0 by the value of M_0 is nearly constant for any set of curves. This is equivalent to saying that the critical value of B_0, the normal induction corresponding to the maximum of μ_0, is nearly constant for each set of curves, as the table shows.

	Tagger Plate.		Schultz.			Sankey.	
Maximum μ_0 ...	4,570	2,870	2,880	2,290	1,920	3,335	2,675
Critical H ($= M_0$)...	1·60	2·75	2·75	3·65	4·40	2·00	2·50
Critical B_0 ($= \mu_0 H_0$)	7,310	7,890	7,920	8,360	8,450	6,670	6,690

In connexion with the approximate constancy of the critical value of B_0 for different strips of a given brand, it is worth while to draw attention to some results obtained by Ewing. From a series of experiments, described in his "Experimental Researches in Magnetism," ‡ he has drawn a number of figures showing how the I–H curve for a given specimen of iron wire is affected by a constant stress applied to the specimen during the magnetic observations or by the after-effects of stresses applied before the magnetic observations. These figures are numbered 46, 50, 55, 57 and 59 in his paper. By drawing a straight line through the origin so as to touch any I–H curve, I picked out the value of I, corresponding to the maximum susceptibility for that curve. It then appeared that, though the mechanical treatment caused a great change in the form of the I–H curve, yet the critical value of I, when κ had its maximum value, was but little affected by the mechanical treatment. Thus, taking as an example Ewing's Fig. 46, relating to an iron wire under the constant tensions due to loads of 0, 2, 4, 6 and 8 kilos, the points of maximum susceptibility were reached with the magnetic forces

* *Electrotechnische Zeitschrift*, vol. 22, p. 432.

† Ewing, *Magnetic Induction in Iron and other Metals*, 3rd edition, p. 378.

‡ *Phil Trans. Royal Soc.*, vol. 176, p. 609.

of 10·5, 8·6, 6·1 and 4·1 gausses, while the corresponding critical values of I were nearly constant, the values being 570, 610, 605, 590 and 580.

I examined in the same manner the I–H curves given by Ewing and Cowan in their paper on the "Magnetic Qualities of Nickel,"* but found that the critical value of I was greatly affected by the mechanical treatment. Nickel thus differs widely from iron in this respect.

In my experiments the differences among the strips of a given brand are probably largely due to variations in annealing.

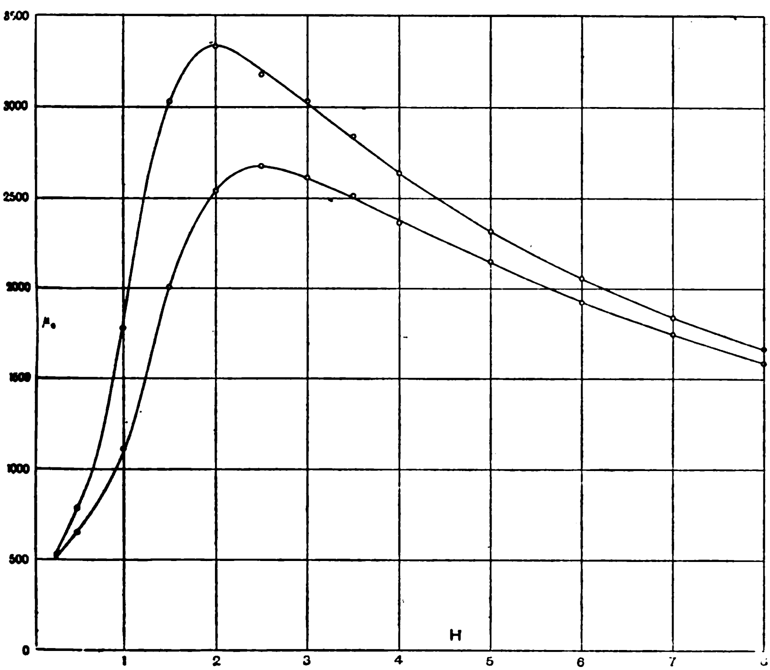


FIG. 25.—Sankey Iron.

The approximate constancy of the critical value of B_0 for a given brand suggests that this quantity may depend simply upon the chemical composition of the iron and not upon the manner in which the various constituents are grouped.

§ 64. The experiments of § 62 point to the conclusion that a permeability test of a single small specimen may often be of little value, when the magnetic quality of, perhaps, many tons of transformer sheet is in question. Probably the only satisfactory plan is to test all the iron used in a transformer, sheet by sheet, by the method described by Richter (§ 58) or by some modification of that method.

* *Phil. Trans. Royal Soc.*, vol. 179 A, pp. 325–332.

Conclusion.

§ 65. It now only remains to return thanks to those who have given me their assistance. My thanks are due to Professor J. J. Thomson, F.R.S., not only for the use of the resources of the Cavendish Laboratory, but also for his sympathetic interest in the work and for his advice. I am indebted to Mr. E. Hospitalier, of Paris, for his assistance in the matter of the magnetic units. The labour of taking the observations in the experiments made in 1903 was much lightened by the kind help of Mr. E. Smart of Perth Academy and of Mr. C. Chittock of Trinity College. I am indebted to Mr. A. Campbell of the National Physical Laboratory for verifying some of the results given in the paper as well as for reading and criticising the paper itself, and also to Dr. R. T. Glazebrook, F.R.S., Director of that Laboratory, for reading the paper. Messrs. Joseph Sankey & Sons, of Bilston, kindly gave me some transformer iron for my work and also read my paper. But my chief debt is due to my colleague, Mr. T. G. Bedford, M.A., of Sidney Sussex College. Not only did he give me most efficient help in many of the experiments, but he also devoted much labour to reading and criticising the paper in its various stages. The paper is much less imperfect than it would have been without his aid. I have also received help and advice from Mr. W. G. Pye and Mr. F. Lincoln, the former and the present instrument-makers at the Laboratory.

APPENDIX.

Mr. Albert Campbell's Experiments.

Mr. Albert Campbell has kindly furnished me with some results obtained by him at the National Physical Laboratory. The effect of a strong magnetic force in reducing the apparent induction subsequently found for a small force was noticed by him in May, 1902, while I did not begin the work described in the present paper until July, 1902. My own experiments on this point were, however, completed before I knew of Mr. Campbell's result. We compared notes in October, 1902, and since then he has verified some of my results. Mr. Campbell has also been so good as to read the present paper in manuscript and to give me the benefit of his criticism. With respect to his own work he writes as follows :—

"Whilst testing some ring-stampings of transformer sheet iron at the National Physical Laboratory during May, 1902, I noticed that the results were much affected by the previous magnetic history of the iron. I found, however, that perfectly consistent results could always be obtained by thorough demagnetisation, by reversals, starting from a sufficiently high value of H. An ordinary commutator, worked by hand, was employed in this process. The following instance will show the general nature of the effects observed. After the ring had been demagnetised, $H = 1{\cdot}240$ gave $B_0 = 2{,}500$. The ring was then cycled a number of times with $H = 27{\cdot}5$; after 10 cycles

at $H = 1{\cdot}240$, B' had the lowered value 1,950. After 100 cycles at $H = 1{\cdot}240$, B' had fallen to 1,910. The ring was again demagnetised, when (after 100 cycles) $H = 1{\cdot}240$ gave the original value $B_0 = 2{,}500$. It will be seen that these effects are similar to those described by Mr. Searle."

"In 1902 Mr. Searle told me he had found that virgin iron is magnetically improved by demagnetisation. I have since tested the point for a number of different specimens of iron sheet, which, so far as I know, had never been magnetised before. The specimens were in the form of rings and were tested by the ballistic method, the hysteresis loop being obtained. In all cases the demagnetisation increased the permeability for moderate values of B and decreased the hysteresis loss for loops in which both before and after the demagnetisation B had the fixed limits $\pm 2{,}500$ or $\pm 4{,}000$. In one case, with $\pm 2{,}500$ as the limits of B, demagnetisation increased the permeability from 2,020 to 2,130, and decreased the hysteresis loss by about 3 per cent."

"Since reading the manuscript of Mr. Searle's paper in September, 1903, I have made a few experiments upon a ring of steel piano-wire in its commercial state, the procedure being practically that mentioned by Mr. Searle in § 36. The ring was demagnetised, starting from 40 gausses, a value much exceeding 20 gausses, the critical magnetic force found for this specimen. The μ'–H curve found after the application of a large magnetic force of 60 gausses differed from the normal curve in a manner similar to that shown in Fig. 6 (§ 36) which refers to material of widely different magnetic quality. The following table shows the results; in it μ_0 denotes the 'normal' permeability and μ' the 'apparent' permeability after the application of the force of 60 gausses."

H	2	4	6	8	10	12	14	16	18	20	22
μ_0	73	77	86	99	117	152	233	457	597	630	625
μ'	55	57	63	71	84	114	192	424	596	633	

Dr. R. T. GLAZEBROOK: I am afraid, Mr. President, I did not realise that you expected me to open this discussion. I have, however, been much interested in the paper and in the previous work connected with it at Cambridge. Some of the difficulties which attach to the measurement of magnetic induction are known to every one, especially the difficulties which are found when one requires to make the measurement on a scale suitable for commercial practice; and I think in the magnetic square that Mr. Searle has brought before us he has devised a means of effecting this which in many cases will prove useful and simple. When once the magnetic square is constructed —and it need not, I think, be on exactly the same scale Mr. Dr. Glazebrook.

Dr. Glazebrook. Searle has made it, although no doubt it is desirable to keep it large—it is possible to work on simple rectangular strips instead of having to have the specimens cut in rings and having to wind each specimen or group of specimens separately. Here, as the members can see, the magnetising coils are permanent fixtures, and nothing requires to be done except to put the specimens in their place. Of course various questions arise as to the leakage effects at the corners, as to whether the pressure between the clamps is sufficient to reduce the reluctance at the joint to a negligible quantity, or within what limit it reduces it. I take it it will be generally felt that Mr. Searle has shown in his paper that, at any rate within the limits that are desirable in practice, the magnetic square forms an apparatus which is satisfactory and sufficient. I notice that quite at an early stage in what he had to say to us he spoke of the very grave uncertainties that were introduced in the ordinary method of testing by the ballistic method. I do not think he gave us anywhere an estimate of them, although I have had such an estimate from him—an estimate, I mean, of what these uncertainties were. I am sure it would interest the Institution if Mr. Searle could tell us in his reply quite briefly what kind of uncertainties he found introduced by the neglect of the precautions which he has been describing. With regard to the main portions of the paper, I believe it was known in a general kind of way that we had to take some care with regard to the previous magnetic history of the iron if accurate results were wanted. Mr. Searle himself refers to a paper by Professor Rowland in which that point was brought out, and to one by Professor Ewing; but I do not think there existed before so careful and laborious an investigation of the point as that which has been given to us this evening. Although it cannot be necessary for practical purposes always to go into all the refinements that have been here described, still it is most valuable and most important to have the need for these refinements put on record, and to have the results of careful investigations made by this method. In conclusion, I think I have only to express my own interest in the paper, and the hope that it will be found to be of real value and importance in the study of the magnetic properties of iron.

Mr. Campbell. Mr. ALBERT CAMPBELL: I am afraid I have not very much to say, as I think Mr. Searle has put in his appendix practically all I wanted to mention on the matter. I performed a few experiments in corroboration of the interesting discovery that Mr. Searle made with regard to virgin iron, and I do not think Mr. Searle emphasises sufficiently the interest of his discovery. I do not know that any one had noticed it before him. If you take a piece of iron that has never before been acted upon by magnetising forces, test its permeability curve, and then de-magnetise it, you will usually find on re-testing that it has improved considerably in magnetic quality. Its normal permeability is much better than the permeability before it had been strongly magnetised and then de-magnetised. It is very curious that the shaking up of the molecules by magnetisation and de-magnetisation should make the iron magnetically better than it was in its original state. As for my other experiment described here, it is merely a verification, for ordinary

Mr. Campbell.

piano steel wire made pretty hard, of what Mr. Searle showed for the softer iron, namely, that if you de-magnetise from a point above the critical point you wipe out the previous history, and that, if you do not do so, you may get very erroneous results indeed for the permeability curve.

Mr. Holden.

Mr. FRANK HOLDEN: I should like to ask Mr. Searle what would be the effect of the substitution of a make-and-break switch for the alternator giving approximately a sine-wave. My opinion is that it would change the value of the critical magnetising force very considerably.

Mr. Mordey.

Mr. W. M. MORDEY: This interesting paper seems more likely to appeal to physicists and instrument makers than to engineers or users of sheet iron for transformer and dynamo work. The author's experiments make it clear that under certain conditions, tests made by the ballistic galvanometer method may lead to wrong conclusions unless the magnetic history of the iron is known.

I am interested in this matter just now, as I have been going into it with my friend Mr. Hansard. In a paper lately read before the British Association, we pointed out some objections to the ballistic method for engineering purposes. For more than fourteen years I have specified iron by wattmeter tests only, for the fairly obvious reason that the wattmeter tells us what we want to know, namely, the total losses of energy from eddies and hysteresis, whereas the ballistic galvanometer gives us only the latter. The author's researches indicate that it may not always give us the latter correctly, and they at least show that great care and skill are required—even greater care and skill than we supposed. We know the method is slow and tedious. I venture to think these experiments afford further reason for avoiding the ballistic method.

Fortunately for practical purposes—except for measuring instruments—the previous magnetic history of the iron does not matter, as in a very few periods or revolutions the iron under any circumstances must settle down to working conditions.

To show the importance of having something more than can be got from a ballistic galvanometer, I may quote a few figures from the paper just mentioned: Total loss tests were made by wattmeter, and hysteresis by ballistic galvanometer. One of our objects was to find how the losses were affected by the thickness of the iron, and therefore some of the tests were made with iron varying from 0·0136 to 0·024 in. thick. We found even at such low densities as 2,500 B that there was a good deal of difference. The loss in eddies in the thinner iron was 36 per cent. of the total loss, and in the thicker it was 57 per cent. At 4,000 B, still a low density, the eddies were 39 and 60 per cent. respectively, while at 6,000 B they were respectively 43 and 64 per cent. of the total. These were at 100 ∿. At 50 ∿ with 10,000 B—a density often reached in transformers and always exceeded in generation even at 50 ∿—the losses from eddies came to 36 and 55 per cent. of the total loss for the 0·0136 and 0·024 sheets respectively.

I mention these results as showing that the ballistic galvanometer methods are better adapted to the study of physical conditions in iron than to the determination of the true losses that go on under ordinary

Mr. Mordey. working conditions with transformers and dynamos. It may be thought that if the hysteresis is determined, the rest can be calculated, but it appears that with the bases that have hitherto been considered sufficient, such calculations are not to be relied upon.

We have to thank the author for giving us this very useful and interesting account of his researches.

Mr. Searle. Mr. G. F. C. SEARLE, in reply (*communicated*): I desire first to thank the Institution for giving me the opportunity of reading this paper. I am very grateful to Dr. Glazebrook for his kind remarks. With reference to the uncertainties, to which he alluded, I may say that they were first detected in some preliminary measurements of hysteresis made by the ballistic electrodynamometer method,* which I described a few years ago, and that they were of the order of 10 per cent.

Throughout the experiments described in the paper an alternating current was used for demagnetising the iron, and thus I have no experience to guide me in giving an answer to Mr. Holden's question as to the effect of the substitution of a make-and-break switch for the alternator. I suppose, of course, that he refers to a reversing key. But Mr. Holden's question suggests that it might be interesting to study the effect of variations in the suddenness in the reversals of a given force upon the apparent induction for that force. I hope I may be able to make experiments upon this point.

Mr. Mordey remarked that for practical purposes the previous magnetic history of the iron does not matter, as in a very few periods or revolutions the iron under any circumstances must settle down to working conditions. I allow that after many periods under all circumstances the iron will reach a steady state, and also that when B exceeds about 5,000, the previous history will have very little effect upon the magnetic force required to produce a given value of the apparent induction. But, for lower values of the apparent induction, my experience is that no mere increase of the number of reversals of the magnetic force has any effect in restoring strongly magnetised iron to the highly permeable state which is attained by effective demagnetisation. I will add that I was formerly of the opinion that after many reversals of a given magnetic force, the change of induction on the reversal of that force is independent of the magnetic history.

Mr. Mordey has pointed out that the engineer needs more information than that which is obtained by the use of the ballistic galvanometer, for the reason that eddy currents are responsible for a large portion of the total iron loss in transformers. No doubt the best way is to measure the total iron losses in the transformers themselves after they have been made, though many persons will probably desire to make preliminary tests on comparatively small masses of iron.

In connexion with wattmeter tests of transformers, it is usual to express the results by means of curves or tables showing how the various losses, at a given frequency, depend upon [B], where [B] is the amplitude of B as deduced from the readings of a voltmeter on the assumption that the induction has the same value at all parts of the

* *Phil. Trans. Royal Soc.*, vol. 198 A, p. 33.

transformer at a given instant; it is usual to make the further assumption that the E.M.F. induced in the coil connected to the voltmeter is a pure sine function of the time. Mr. Searle.

But I wish to point out that, unless the lines of induction, which are linked with the magnetising circuit, are all of the same length, there will be considerable variations in the value of the induction in different parts of the transformer, at any given time. This is particularly the case when the greatest magnetic force is less than the critical force, for then the greater the magnetic force, the greater the permeability. To take a definite example, suppose we have a ring of "Tagger plate" with internal and external radii 6 and 12 cm. respectively, and that the magnetic force at the inner boundary is 0·6 gauss. Then the magnetic force at the inner boundary will be 1·2 gausses and, by the table in § 57, the induction will vary from $0{\cdot}6 \times 1{,}308$ or 785 to $1{\cdot}2 \times 3{\cdot}687$ or 4,424. This want of uniformity in the distribution of the induction gives rise to several complications. Even though $d[\mathrm{B}]/dt$ be a pure sine function of the time, the inequality will cause the value of $d\mathrm{B}/dt$ at any given point to involve the higher harmonics, because the B–H loops for small values of B are not geometrically similar to those for large values of B. The presence of these harmonics will affect the eddy-current losses. The effect would be difficult to estimate, because the eddy-current at any given point depends upon the distribution of $d\mathrm{B}/dt$ throughout each plate in the transformer, and not simply upon the value of $d\mathrm{B}/dt$ at the point. It may thus easily come about that the eddy-current losses at a given frequency are not proportional to $[\mathrm{B}]^2$. If there are joints in the magnetic circuit, these will tend to cause further disturbances in the distribution of the induction.

The inequality in the distribution of B will also lead to great inequalities in the distribution of W, the hysteresis loss. For the "Tagger plate" the number of ergs lost per cubic cm. per cycle may be represented approximately by $\mathrm{W} = 0{\cdot}001\,\mathrm{B}^{1\cdot6}$, and hence with the values of B just quoted, W ranges from 42·8 to 681·3, the latter number being nearly 16 times the former. The result of the inequality is that the mean value of $\mathrm{B}^{1\cdot6}$ may differ considerably from the 1·6th power of [B].

In taking the mean values we must remember that [B] is found by integrating B over the *section* of the ring, while [W], the mean value of W, is found by integrating W throughout the *volume* of the ring. Thus, if a and b be the radii of the ring, we have

$$[\mathrm{B}]\,(a-b) = \int_b^a \mathrm{B}\,dr,$$

$$[\mathrm{W}]\,(a^2-b^2) = \int_b^a 2\,\mathrm{W}\,r\,dr.$$

If $\mathrm{W} = 0{\cdot}001\,\mathrm{B}^{1\cdot6}$, we have

$$[\mathrm{W}]\,(a^2-b^2) = 0{\cdot}002\int_b^a \mathrm{B}^{1\cdot6}\,r\,dr.$$

Mr. Searle. A rough graphical calculation, using the data of §57, shows that in the example I have chosen, the value of [W] is about 179 while 0·001 $[B]^{1·6}$ is about 185.

If we take $H = 2$ at the inner boundary of the ring, B ranges from 2953 to 8226 and W ranges from 357 to 1838. The value of [W] is now about 892 and the value of 0·001 $[B]^{1·6}$ is about 940.

These examples do not, of course, explain the results obtained by Mr. Mordey, but they do perhaps render it desirable that the inequalities in the flux distribution should be kept in mind in experiments with transformers.

I was aware that the finite width of a ring might lead to errors in magnetic testing, but I am indebted to Mr. A. Press for drawing my attention to the fact that the inequalities in the flux distribution in transformers may lead to apparent discrepancies between theory and practice. I hope Mr. Press will consider the question further, so that we may be able to estimate how far the inequalities I have mentioned are responsible for effects such as Mr. Mordey describes in his paper.* The dimensions of the magnetic square were chosen with the view of diminishing as far as possible the effects due to inequalities in the flux distribution. If Mr. Mordey makes further experiments on the iron losses in transformers, the interest of his work would be greatly increased if he would give the dimensions of the transformers, so that some estimate might be made as to how far uniformity was attained in the distribution of the induction throughout the transformer.

The President. The PRESIDENT: I now ask you to give a hearty vote of thanks to Mr. Searle for bringing this very interesting paper before us and drawing our attention to some features which have not received the attention they deserve.

A hearty vote of thanks was accorded to the author, and the meeting adjourned at 9.35 p.m.

* *Report of The British Association*, 1904; *Electrician*, vol. 53, p. 790.

GLASGOW LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

ROBERT ROBERTSON, Member.

(*ABSTRACT.*)

November 8, 1904.

In placing me in the position I occupy to-night as your chairman, you have done me a great honour, an honour which, I assure you, I appreciate most highly. I do not claim any special qualification for the office, but as a strong advocate of education in its broadest sense in connection with all scientific professions and industries, I have a great belief in the value of such Institutions as ours as an important factor in such education, and it will be to me a very great pleasure to devote my energies to help on the progress of this branch of the Institution, and I trust I may be able in co-operation with the committee to add a little to its prosperity, and thus prove worthy of the trust you have placed upon me. I hope it will not be altogether unprofitable for us to spend a little time in a survey of what has been done in the use of electricity in collieries generally, and more particularly in the county of Lanark, where the coal industry is an important one.

Apart from the fact that the coal industry is of considerable importance in this district, and the question of the use of electricity in connection with it is not without interest to a number of our members, there is another reason why the consideration of it at this time is appropriate. As you are aware, a committee, appointed by the Home Department of the Government, has been investigating this question for the last two years, and only recently issued their report, and this has brought the subject very much in evidence. This report, with the minutes of evidence and the draft rules, has been published, and has called forth a considerable amount of adverse criticism, but it would be out of place for me to add anything at this time in the way of criticism particularly owing to the fact that the proposed rules are only in draft, and representations are being made by the Mining Association of Great Britain in favour of alterations, and these are at present under consideration. While this inquiry will, no doubt, be the means of calling attention to the importance of electricity in connection with this industry, it is of the utmost importance that no code of rules or legislative enactment should be passed, which in any way trammels or hinders the legitimate

development and extension of its use. The Mining Association should have the strongest support of this Institution, in so far as their efforts are directed to this end.

It has often been alleged, and not, I think, without reason, that legislation has already been the means of retarding the development of electricity in this country, and it would be unfortunate if anything new should be done with the same result. There is no doubt, however, that legislation on the subject—carefully drafted with the view of leaving all freedom compatible with reasonable safety—will do much to help the advancement of the industry, by clearing away much doubt and prejudice against the new power in the minds of many men connected with the industry, who are ignorant of the merits of the power, but have unfounded fears owing, in a large extent, to their anticipation of danger where no real danger exists.

The purposes for which power is required in a coal mine are probably more varied than in any other industry, and the points at which power is required are also very widely separated. On the surface the most important purposes for which power is required are the winding machinery for bringing the coal to the surface, and the fans for ventilating the workings below; but in addition to these, there are waggon tips, coal screens, washing and elevating machinery, conveying plant, workshops, and also the moving of the loaded waggons under the screens, besides many other minor purposes. Underground, the uses are almost equally varied, including haulage, pumping, drilling, coal-cutting, and lastly, an application not yet widely developed, the driving of conveying machinery at the working face. These uses are not only varied, but from time to time the power required has to be conveyed to other points as the field gets worked out, and this in itself forms a strong argument in favour of electricity. Owing to its efficiency, both in production and transmission, to its reliability, and, probably most of all, to its flexibility and the facility with which the plant can be changed from place to place, electricity is without doubt the most suitable form of power for the purpose.

After dealing with the general question of the application of electricity in collieries, the next important point is to decide which of the two classes of current, direct or alternating, is the most suitable. There is no doubt that in this country, up till the present time, direct current has been much more used than alternating current, but I do not think that this is any guide to the relative merits of the two. There has been a great deal of controversy on the subject, and much can be said in favour of each system, and each has its advocates, but while I cannot go into the merits of each in detail in the time at my disposal, my conviction is that just as electricity has in most cases a superiority over compressed air, so the alternating-current system has in many cases a decided superiority over the direct-current system, and that future development will confirm this opinion. This class of current largely predominates in the case of mines on the Continent, where we will see that, as regards magnitude and completeness of installations, more has been done than in this country. In Belgium, in Germany, and in Austria, there are many collieries which are entirely operated

by means of electricity, and in some cases I have seen single motors at work there larger than the whole installations in most of the collieries in this district.

The next point of importance, in connection with Continental practice, is the introduction, a few years ago, of the high-speed mining-pump, due, I think, originally to Professor A. Riedler. These pumps are run at speeds varying from 120 to 300 revolutions per minute. There are a great many examples of these, which have been in use for some time. In one case a pump running at 146 revolutions per minute delivers over 400 gallons per minute against a head of 2,500 feet, and in another example over 700 gallons a minute are pumped against a head of 500 feet, the speed being 180 revolutions per minute. As regards hauling, the Continental colliery plants have no special features, and so far as I have been able to ascertain, there are few examples of coal-cutters in use. This, I fancy, must be due to the fact that the seams are of such a nature that these are not required. The special feature of the application of electricity to collieries on the Continent is its use for winding coal and men in the main shafts. There are a considerable number of examples of these throughout Belgium and Germany. The smaller ones only involve motors of moderate size, and if the generating units are large as compared with them, no serious trouble or difficulty will arise in connection with the frequent stopping and starting and accelerating of the motors.

When we come to consider cases of winding, where the power required is nearly equal to or in excess of the generating units, these difficulties become very serious. In Germany there are two examples illustrating this—in the one case the winding motor develops over 1,000 H.P. at its maximum, which is about double the output of each generator. Although this is only required for very short periods, the effect is that the drop in pressure is very great. This difficulty has been overcome at another colliery, where the Ilgner system is in use. This system depends upon the use of a heavy flywheel acting as an energy accumulator to carry the motors over the peaks of the winding load. This system, though somewhat complicated, appears to have been successful in operation, but the installation must be expensive, and careful consideration would require to be given before deciding in any particular case whether it is a system which would give economical results. In other cases storage batteries are used for making the load on the generators more uniform, and this system has also been successfully employed in connection with several collieries on the Continent. On first consideration it would appear that, as a rule, the winding engines being near the boilers little advantage, if any, could be got from the employment of electricity for this purpose. The conditions under which winding engines are operated are, however, such that it is impossible to adopt steam-saving appliances, and the quantity of steam consumed per effective horse-power is in most cases very large. The maximum power required is very great, but this is only needed for a few seconds during the acceleration in each operation, and for the remainder of the time the engine is working most inefficiently. These conditions are sufficient to warrant the careful consideration of electricity as a

power for this purpose, especially in the case of the equipment of new collieries.

Turning now to the consideration of what has been done in collieries in England and Wales, we find a great number of plants at work, the most of which are much more extensive than those in Scotland. Out of a selection of some 40 or 50 collieries in England and Wales, taken at random, I find that the average horse-power of the electric installations is over 400, and cases of over 1,000 H.P. capacity are not unknown. Another interesting comparison from the same collieries is that while the direct-current system predominates, it only does so to the extent of 55 per cent. against 45 per cent. in the number of collieries, and this is very much reversed when the total horse-power is considered, the figures then being 40 per cent. and 60 per cent. respectively. These figures must not, however, be taken as giving the averages over the whole of the collieries, as they only apply to those about which I have been able to get information.

The importance of the coal industry in Lanarkshire may be gathered from a comparison of it with that of the rest of Scotland. If we make the comparison on the number of collieries, we find that about 45 per cent. of the whole collieries in Scotland are situated in this county. Taking the number of persons employed, the percentage rises to about 47½, and, again, if we make the comparison of the output, Lanarkshire gets credit for about 49½ per cent. These figures are sufficient to indicate that we are considering an industry of some importance, the number of persons employed at it in the county being over 52,000, and the output last year over 17,000,000 tons. Without attempting to distinguish to a month or two as to which was the first instance of the application of electricity to power purposes in collieries in Scotland, I may mention four plants, all of which were installed during the years 1892 and 1893.

In Lanarkshire there was a plant installed under the supervision of my firm of about 120 H.P. for haulage and pumping at Earnock, and another plant about the same size at Milnwood Colliery. During the same years there were other two plants installed in Scotland, one at Dumbreck Colliery and another at one of the Lothian Coal Company's pits. I believe also that a small pump was worked at another of that company's pits as early as 1889, but I have no definite information on the subject. It is thus only a short period of about twelve years since this application of electricity, which we are considering, was started, but even taking this into consideration, I do not think the progress that has been made is equal to what might have been expected. I have had opportunities of knowing what is being done in this way at many of the collieries, and I have recently supplemented this knowledge by getting information on the subject in respect of other collieries, and although the time at my disposal has not been sufficient to enable me to make my investigation quite exhaustive, the figures I now lay before you represent the facts in connection with over 80 per cent. of the collieries in Lanarkshire, and over 90 per cent. in respect of the output, and, with one or two exceptions, all the important collieries in the district are included. Of these, not more than 11 per cent. in number,

or 20 per cent. on the basis of output, are using electricity for power purposes, and in the majority of these cases it is only used in a very minor degree, by having small motors installed for driving small pumps, etc., the power for which is obtained from generators originally installed for lighting only. The number of plants of 100 H.P. and over is very small, and in the majority of these the power does not exceed 200 H.P., with a few isolated exceptions, where the power installed or being installed reaches 500 or 600 H.P. The use of electricity is confined mostly to pumping, underground haulage, and coal-cutting, and there are only a few cases where all subsidiary operations on the surface are also done by electricity, and there is only one case where electricity is applied to conveying plant at the working face. There is not, I believe, at the present time a single example of alternating current in use in a colliery in Lanarkshire, but a plant on this system is in process of construction. This plant will, I understand, include an example, on a small scale, of the Ilgner system, to which I have referred, and we may have an opportunity ere long of seeing this at work in our own neighbourhood. In other parts of Scotland there are some important installations and a few examples of alternating current.

The whole question, however interesting it may be from the point of view of the electrical engineer, must show a substantial balance in its favour in the ledger if it is to commend itself to the colliery owner. In looking at it from this point of view, no generalisation can be made, and each case must be considered on its merits. Looking at a few examples of what saving has been effected will, I think, convince any one that there is certainly sufficient justification to warrant every colliery owner giving the question consideration. In an example with which I am familiar, the substitution of a system of electric haulage in place of horse haulage underground effected a saving sufficient to repay the capital expenditure in between two and three years. Numerous examples could be given, and in some cases the saving claimed is sufficient to meet the cost in two years. This is sufficient to warrant, as I have said, careful consideration of the subject, but there is a more important factor in the Lanarkshire coalfield which makes the consideration of this subject a necessity, that is, the fact that the thicker and shallower seams are gradually becoming exhausted, and if the industry is to maintain its importance, measures must be adopted for working the lower and thinner seams, and in electric power, I am sure, will be found an agent which, if properly applied, will make it commercially possible to work seams which could not otherwise be worked at a profit.

The question of the source of supply of electricity is one of some importance, and in this respect the country generally is entering upon a new era. In many districts throughout these islands companies have been established for the purpose of supplying electricity, mainly for industrial purposes. Some of these companies have already begun to give a supply. In one of these districts, in which this industry is of great importance—South Wales—the starting of the supply has, I believe, given a great stimulus to the application of electricity to coal

mining, and a considerable number of collieries are putting in plant, which will be supplied from the power company's mains. A similar supply will shortly be available over a substantial part of the district we have been considering to-night, and I trust that it may become an important factor in the industrial progress of the district.

DUBLIN LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

MARK RUDDLE, Member.

(*ABSTRACT.*)

November 10, 1904.

As it is not usual for your chairman's address to be followed by a discussion, the omission of this feature entails upon him a great responsibility in the selection of the subject-matter upon which he should address you. I hope, therefore, that in the remarks which follow you will find ample material for your criticism, as in view of the rapid increase in our membership I propose to deal briefly with the method of training necessary for entrance into the profession we represent.

The right of electrical engineering to be termed a profession has sometimes been called in question, but I think electrical engineers now admittedly hold a clear title to rank as belonging to a distinct profession, since our development has been mainly due to the efforts of a long line of experimenters and philosophers. Science, as Huxley says, is organised common-sense, and the groundwork of our claim to be classed as a profession is the necessity for the exercise of all our past experience in the treatment of each problem as it arises—that is to say, for the application of those general principles the knowledge of which has been derived from all one has seen, or read, or thought about. The laboratory experiment of to-day is utilised in practical engineering to-morrow, and there is no other branch of engineering which so lends itself to exact mathematical treatment and demonstration.

It is unnecessary for me to enumerate the multifarious applications of electricity in every-day work; new uses, and improvements of existing ones, so crowd upon our notice that it becomes practically impossible for an engineer in full work to keep himself abreast of the times in this respect in all branches of experiment and work. Every day is making more manifest the absolute necessity for specialisation in our profession, for, owing to the highly scientific and complex nature of the problems which have to be solved before arriving at a satisfactory method of meeting the demands of new applications and conditions, no single individual can hope to have a thorough up-to-date knowledge of the whole cycle of professional work. This leads up to a most important point, viz., the training of electrical engineers with regard to their successful entrance into and practice of the profession.

A municipal engineer is often looked upon as an encyclopædia of information as well as a universal provider of employment, and parents frequently consult me as to the easiest way in which their sons can become trained electrical engineers, with, needless to say, a correspondingly satisfactory salary, and in the majority of instances leave me with a feeling in their minds that I am unduly magnifying the amount of study and training necessary before the first step can be taken upon the ladder of remunerative practical work.

Man carries with him the physical texture of his ancestry, as well as the inherited intellect bound up with it, but this extends no further in the making of an engineer than giving the possession of a quick comprehension, a retentive memory, and a power of grasping problems. All else in modern engineering has to be acquired by a more or less tedious process of strenuous education.

I shall not attempt to place before you to-night any cut-and-dried scheme or syllabus embracing the whole art of training an electrical engineer, for that at present seems to be a matter with regard to which the most diverse opinions are held by the leading experts in technical education. All of you will, no doubt, have in your minds the very exhaustive paper read by Dr. Walmsley before the Institution in London in February, 1904, and the prolonged discussion which ensued thereon, and it is a serious matter to find that even now no settled plan seems to meet with general approbation. This is greatly to be regretted, for upon the system of technical education to be pursued in the near future depends largely the position which the Empire will continue to hold in the commercial markets of the world. Notwithstanding our backwardness in this matter in comparison with many parts of the Continent, we have hitherto managed to hold our own, being largely assisted by the natural resources of the country, but competition is becoming more keenly felt every year, and we cannot afford to drift along in the same old careless way.

One predominant fact appears certain, namely, that one course of general training is necessary for those men who have the ability and funds for qualification into the higher branches of the profession, and that an entirely different training is required for those who, under ordinary circumstances, will have to begin their career with the routine work of the many applications of electrical engineering. The candidates for the higher branches will be those who intend to devote themselves more particularly to original research and to the theoretical development of the scientific side of the profession, but this section is outside the scope of my present remarks. I feel more concern for what may be termed the rank and file of the profession, each one of whom, nevertheless, carries a marshal's baton in his knapsack, if he has been properly trained and qualified to seize the opportunity when it occurs.

The question seems to hinge largely upon the nature of the primary and secondary education received up to the age of sixteen. The syllabus should have comprised, in addition to the ordinary subjects of general education, a good grounding in algebra, euclid, elementary physics, and, if possible, either French or German, or both. If in the secondary

school there is even a small workshop, a very valuable amount of knowledge could be imparted in the elementary principles of simple tools and machines. Whatever the course of training to be pursued after this age, full advantage cannot be taken of it if the secondary education has not been directed with a definite idea of the engineering object in the future, for inadequate preparation means that much valuable time must be lost in preparatory work after leaving school. After this period two general courses of training are open to the student, each having strong advocates and neither giving universal satisfaction. First, he may enter a suitable engineering works and spend two to three years in learning as much as possible of machine work, turning, fitting, etc., together with some experience of drawing office work. After this, having gained an insight of practical every-day work, he may then enter an engineering college for thorough training in the theoretical side of his work. On the other hand, he may, on leaving school, at once take up theoretical work in a well-equipped technical college, and after, say, a three years' course, he may enter an engineering works with an eye to one particular branch of professional work or to gain all-round experience. The latter course has much to recommend it, as there is then no break in the educational course such as must occur if the student goes into a workshop straight from school, and after leaving the latter, has to resume the habit of learning and of routine study, which is not always an easy task.

There is, however, a middle course which is often recommended for those students who are unable for various reasons to spare sufficient time to take full advantage of the general training I have mentioned—that is, to enter a works for practical training and to work up the theoretical side in evening classes. Except where no other course is possible, I do not recommend this plan. It is hard for a youth of sixteen or seventeen to keep ordinary shop hours at mainly physical work and to give up the majority of his evenings to private study or attending technical classes, and I am not sure that it is unattended with danger to his proper physical and mental development. A growing youth must have time for recreation and rest, and success is dearly bought at the expense of a debilitated constitution. Still, there are cases in which there is no option but to pursue this plan; but men so trained cannot expect to compete on equal terms with those who have had a more liberal course of study, except in cases of unusual ability and aptitude.

Much, however, depends upon the student himself. If he has an innate love of the science and art of electrical engineering, and of learning for the sake of finding out hidden things; if he has the physical and mental constitution to stand the strain of burning the candle at both ends, so to speak, he will, with average good-fortune, attain his end more surely than many others who start under more, apparently, favourable auspices, for, having the self-stimulus of an insatiable knowledge-hunger, he is in closer sympathy with his teachers, and does not wait to be spurred on in his work. And this is the true spirit which should animate those seeking to enter our profession. The answering of examination questions and obtaining passes and certificates alone are no test of a man's fitness. In saying this I do not

wish to imply that these examinations are entirely useless, for they undoubtedly do serve a useful purpose in separating the earnest student from the merely brilliant one. The failures in formal examinations often provide the most reliable raw material to be worked up afterwards, as only those who have grit in them continue to persevere in spite of their failure to pass, and this determination to persevere strengthens them for their after-career. There is no such thing as finality in the knowledge of electrical engineering—a week now is more pregnant with discovery than one hundred years was formerly, and those who desire to keep in the front ranks must keep pace with the times in their special spheres of work.

A serious danger is now developing owing to the great inrush of young men into anything electrical in name; many of them seem satisfied to acquire just enough technical knowledge to secure a post. Once secured, they settle down and perform their routine duties more or less thoroughly, and neglect the keeping of their training up to date. Sports and recreations claim their leisure hours entirely, and they rarely occupy such idle moments as occur between the intervals of actual work in anything bearing on their professional work. Time goes on, and opportunities occur for promotion or for entering fresh fields of work, and they find they stand no chance against competitors of more modern training from not having kept their knowledge up to date. It is not enough to have sufficient knowledge for present needs; one must look ahead, and be prepared for developments.

I have hitherto dealt principally with the technical side of a student's training, but before a student can expect to secure a position of any importance he has yet to acquire the commercial or business side of his education. No technical training institute can teach a student how to handle workmen, or how to turn out finished plant from a workshop at commercially remunerative prices, nor would it be reasonable to expect it to do so, for such knowledge can only be gained in the hard school of practical experience, and learnt little by little as opportunity offers. And yet such is the ultimate object of all profession training in the majority of cases, for the most perfectly and scientifically designed scheme is quite valueless unless it can be made a commercially financial success.

I think I have detained you long enough upon this subject for to-night, and I would now say a few words upon the work of our local centre. The standard of papers read before you has been well maintained during the past session, and, in fact, one or two have seemed almost too high class for a large number of the members, though not for the credit of our centre. I judge of this by the very limited amount of discussion which ensued thereon, and, with all due deference to my hearers, I cannot but feel it somewhat of a reproach that so few members can be got to join in the discussions, even upon subjects with which most of you are familiar in your every-day business. This should not be. It is not quite fair to expect the same men to speak upon every occasion, and it does not tend to elicit the most profitable discussion upon the particular subject. This, I venture to say, is a great loss to all of us, and takes away largely from the main object of

our meetings, which should be the advancement of our mutual knowledge, and, therefore, the interests of the profession at large. I would, therefore, appeal to our hitherto silent brethren to help us in the future; it is not necessary to make a long speech, a few words of practical experience straight to the point are worth infinitely more than a long dissertation couched in ornate phraseology but adding little or nothing to our information. A more general discussion would enable us to invite shorter papers from members, and this in turn would remove much of the present difficulty in getting members to contribute papers.

In conclusion, gentlemen, I have to thank you for your attendance here to-night, and to express the hope that at the close of the present session our proceedings will show that we have made substantial progress in advancing the interests of the profession to which we have the honour to belong and of the Institution which officially represents it.

LEEDS LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

WALTER EMMOTT, Member.

(*ABSTRACT.*)

November 17, 1904.

Practically all branches of Engineering have been at a somewhat low part of the commercial curve, during the year 1903. As to the cause of this, I think you will all agree that it is not due to any want of energy on the part of those responsible for the industries concerned, nor do I think it due to any strained relations between Capital and Labour during that period. We must, I think, take it, that this depression, which has extended not only to this country, but to nearly all the civilised world, so far as engineering is concerned, has arisen through circumstances practically beyond human control. Had the depression been confined to Great Britain alone, we might have concluded that foreign competition was turning us out of our own markets, as well as securing our trade at home, which we are so anxious to guard. I have carefully studied the various returns, and am, thereby, confirmed in my opinion that our safety lies largely in maintaining a firm grip on the various departments over which we have control, with a view to, at least, holding our own. Let us not forget, that times of depression and activity alternate, and as we are now at the bottom of the curve, we may, I believe, fairly look for an upward tendency in the immediate future.

The effect of a complete system of standardisation on the various engineering industries cannot be over-estimated. Not only will the manufacturer be able to turn out better work, but the prime costs will be reduced, and delivery expedited. All the leading manufacturers have for some time been making the various parts of their engines and machines to template, and interchangeable, to the benefit of both the manufacturer and the user. What we now require is some sort of uniformity as to sizes and output, and some reasonable range of E.M.F. and periodicity in central station working. This, if taken in time, is not such a serious matter as might at first sight appear, as the exceedingly low voltage stations have by this time been mostly changed over to the higher voltages common to-day.

Steps have been taken during the past year to amend our patent laws, and new regulations have been drawn up which come into force on the 1st of January next. These may be summed up as follows :—

In addition to inquiries which an examiner is directed to make by the Patents Act, 1883, he shall make further investigation for the purpose of ascertaining whether the invention has been wholly or in part described previously in any specification published and deposited in the United Kingdom within fifty years next before date of application. If the invention has been previously claimed or described, notice will be given to the applicant, who will have the opportunity of amending his specification, which will afterwards be subjected to a further examination. It will be noticed that this arrangement will place us somewhat on the same lines as the American and German patent laws in regard to examination, and much will depend on the examiners, and the value of the arrangement will be mainly to indicate to the honest inventor whether, in view of prior publication, it is worth his while to proceed with his patent.

The chief matter of interest to telephone engineers during the past year, has been the inauguration of a few Municipal Telephone Undertakings. Whether such undertakings will prove the great success which has been foreshadowed remains to be seen. Personally, I see no reason why they should not ultimately prove a success, and of one thing there is evidence already, viz., that the competition has had the effect of obtaining some valuable concessions, and a better service for the public than might otherwise have been enforced for some time to come, from the Company holding what may be termed the monopoly.

The question, however, arises as to whether the telephone, being a national institution, is likely to be that great success which one naturally desires, if the working is divided over three systems of control, *i.e.*, the various Municipalities, The National Telephone Co., and the Post Office. The latter already controls the trunk system throughout the country, and therefore deals with a large slice of the telephonic work of this country, but I venture to think that the Telephone Department of the Post Office is the proper authority to control the whole of public telephone work in the country, and until this has been done, I fail to see how we can ever attain to that high state of efficiency in telephonic matters which is so desirable.

The most notable commercial feature in the Telegraph Department, during the past year, is no doubt the underground connection of London with the North, which has been under discussion for the last fifteen years, and may now be considered an accomplished fact. In the early days, we considered we were doing well, if, with a well-adjusted Varley "pecker" (relay), we were able to get through thirty words per minute; now, the pace of signalling attained is only limited by the amount of work to be got through, and by the means at hand, outside the purely electrical, of dealing with that work.

In submarine telegraphy the commercial value of a cable, and the rates charged for the transmission of messages, both depend upon the speed of working, or the number of words per minute which can be transmitted. By duplicating and automatic working, the speed has been increased ten-fold. Yet the materials used, and the general construction of the deep sea cables of to-day, are practically the same as those in vogue nearly sixty years ago, and the "Minutes of evidence, taken

before the Submarine Telegraph Committee, at the office of the Board of Trade" in 1859, afford a striking illustration of this fact. It is worthy of note that two of the founders of the Institution of Electrical Engineers, both of whom are still with us, notably Lord Kelvin, and Sir W. H. Preece, gave evidence on that occasion.

In the central station there is no margin for any very extended increase in the efficiency of our dynamos, and we are therefore left with the two items, fuel, and its utilisation, as the only two directions in which reduction in generating costs can be effected, and even in these there is not a very great margin to work upon in an up-to-date station, with the best steam-raising, superheating and condensing arrangements.

From a close practical acquaintance with the various power gas generators, and the modern gas engine for large powers, I am led to believe that at no distant date a combination of power gas generator and gas engine will perform a very important function in our generating stations. As we all know, to our regret, the steam-engine and steam-raising plant, as now in use in even the most modern stations, is not by any means so efficient as could be desired. The various losses from flues, radiation, condensation, etc., are familiar to the youngest member of our Institution. The fact is also well known that the internal combustion engine, either as an oil or gas engine, is far less wasteful in this respect. But it is only during the past few years that gas engines, of nearly the power of the steam engines at our command, have been introduced with any degree of confidence. This has not been by reason of inability on the part of the gas engine manufacturers to make large engines, but more from the fact that the power gas plants had not reached the necessary requirements of a large supply with a capital outlay in first cost and efficiency to compete with a large steam-raising plant.

A type of power gas plant, which will probably play a most important part in the future of gas-driving in this country, is that known as the suction gas producer. From tests I have made with such a producer of 250 B.H.P., I have no hesitation in concluding that an average, over all, thermal efficiency of 25 per cent., or twice that of a good steam plant, can be obtained.

The question of economical power gas production being settled, the next factor is that of the engine, and from my own experience and observation, I consider that we need have no fear in installing large power gas engines for central station work. Engines of 1,000 and 1,500 H.P. are now quite common, especially on the Continent and in America, and in this country a confidence is being gradually produced, which will result in a wider application of these larger powers. I have had the installing of two 4-cylinder 300 B.H.P. 250 r.p.m. engines, and I find that running under varying loads, and with varying thermal efficiencies of gas, they are equal to the best steam engines as to steadiness. I grant that my experience so far has been, that, if anything, more attention is required with the gas engine than with a steam engine of the same size, the latter often being left to take care of itself, but I believe that a large amount of this extra attention is

due to a natural nervousness, and a fear that something may happen; but this will wear off soon enough.

I grant that as regards the relative size of the steam and gas engine the balance is in favour of the former so far as floor space is concerned. The question of speed is also in favour of steam, so far as the size and cost in the dynamo are concerned. The relative first cost of the two classes of engine is also in favour of steam; at the same time the cost of the power gas plant is less than boilers, etc., and the cost of banked-up fires for steam to meet any emergency (which is no inconsiderable item in the generating costs of a large station), does not exist. With a supply of gas at hand we have no difficulty in starting up and getting current on to the switchboard in slightly under a minute. For small stations, say to meet the requirements up to 15,000 inhabitants, a gas-driven plant will probably prove to be a most successful combination in the very near future.

There has been a marked increase in the number of combined destructor and electricity works of late, and all indications go to prove that the number of such works will increase even more rapidly in the near future; this even in view of the fact that some of the power companies are canvassing most of the small towns. This increase in combined stations may arise from the fact that the towns which have obtained provisional orders and have not yet put them into force are being pressed by the Board of Trade to do so, or they run the risk of losing their powers, which they are loth to do, and, further, the establishment of a refuse destructor in many places has become an absolute necessity on sanitary grounds alone, as it is practically the only means of disposing of the ever-increasing refuse of our towns. Some use must be made of the steam generated, and the production of electricity seems to be the most natural available. As there is now no difficulty in getting an output of 45 k.w. per ton of refuse of even only a fair average quality, it is evident that the return is worthy of consideration as representing a certain monetary value which would otherwise be expended in coal. By this means a municipality is helped with its electricity undertaking, and is also able to get rid of the refuse. As a rule the destructor comes under the control of the sanitary committee and is installed to meet its requirements, while the electrical department is controlled by the electricity committee. Although these two committees are often quite distinct as to their personnel, the joint result financially affects the whole body of ratepayers as if the whole were under one committee. This, no doubt, is the reason why there is no recognised system of dealing with the accounts of the two departments when thus combined, as it is regarded as being "all the same in the long run."

Unfortunately, this want of uniformity in the separate systems of debits and credits, prevents an exact comparison being made of the results of the working of the various stations of this type. Some, for instance, debit the steam raised based on the electrical output from sanitary to electricity account and provide the labour for stoking. Others debit a proportion of the standing charges of destructor to electricity, and in other cases an even more complex arrangement

is adopted. The result is to make some of these stations appear as being financially in a worse position than they really are. One thing is certain, that by combining the two departments, the municipality obtains the best possible arrangement for the efficient disposal of its refuse, and any electricity obtained is an asset which would otherwise be lost unless some means were found for utilising the steam, as for instance in sewage pumping, stone breaking, clinker crushing, etc.

MANCHESTER LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN

C. D. Taite, Member.

(*ABSTRACT.*)

November 18, 1904.

I propose to deal principally to-night with the branch of electrical work with which I am best acquainted—namely, electricity supply undertakings, and having recently made a tour in America, under the auspices of our Institution, and also of the American Institute of Electrical Engineers, I propose to discuss a few points in which American practice differs from our own. Both in this country and in America the electricity supply business started in a very modest way with small stations, the main point of difference in the practice of the two countries being that in America all wires and cables were run above ground, while almost from the earliest days, both feeders and distributors in this country were laid underground with few exceptions. A fundamental difference, due to legislative enactments in the methods of dealing with the problem of supplying electricity from central stations, was observed in the two countries from the commencement. In Britain, with the sole exceptions of London and Newcastle, competition in the supply business was prohibited, one provisional order being granted for each city, town, or district. In America, on the contrary, a town of the size of Chicago or Philadelphia would be divided up amongst quite a number of companies, totalling sometimes as many as twenty, each operating on a comparatively small scale, and frequently adopting entirely different systems as regards voltage and type of current. The development was more rapid over there, largely on account of the absence of vested interests, and also because the overhead system of supply enabled cables to be run more easily and much more cheaply than in this country. To the early success of supply undertaking in America is perhaps traceable a great divergence of practice in distributing electricity. I refer to the almost universal system of supplying current in the States at a pressure of 110 to 120 volts direct current for lighting and 230 volts for motors, as compared with our pressures of from 220 to 250 for lighting and 440 to 500 for power.

It seems not improbable that when the higher-pressure lamp came

into vogue it was found it would be so expensive to change the arc lamps and motors that the psychological moment was allowed to pass without any alteration being made. The Americans, with very few exceptions, did not make the change, and probably will never do so now. The result is that their distributing systems—so far as they are laid underground—are, at the present time, very much more expensive than ours, and where the supply necessitates sub-stations and transformation from high-tension three-phase to low-tension direct current, sub-stations have to be much more frequent than over here. When the American engineers found their original sites becoming too small to meet the increasing demand, they recognised the fact that centralisation of large units under one roof would tend to economy, and there began in the various cities and towns the process of consolidation, with the result that at the present time one generally finds in each city one company only operating, this company being a consolidation of the many pioneer companies.

By a series of developments, therefore, America has arrived at a point which our authorities foresaw from the first, and provided for by refusing to sanction more than one provisional order for a given area, and the evolution which has taken place there seems to emphasise the fact that a competition between electricity and gas and between private plants is so keen, that to create competition between rival undertakings in the same area is totally unnecessary, and not to the best interests of the community.

In England we are rapidly becoming converted to the advantages of the steam turbine for driving our large units, particularly for alternating-current work. The manufacture of direct-current dynamos suitable for the high speeds at which turbines have to be run has not yet become general practice, although there are a fair number of successful turbo-driven direct-current generators in operation, but, generally speaking, they are of comparatively small size. It is now clear, however, that turbo-alternators can be satisfactorily designed and built for almost any power, and we shall shortly have running the power-station of the Underground Railways in London, equipped with ten 5,500 k.w. turbo-alternators, these machines being capable of carrying a load of 50 per cent. above that at which they are rated. The advantages of turbines have been enumerated so frequently that there is no need to tabulate them here.

In America it did not appear that the turbine was making such rapid headway as in this country. One could not help being struck, for instance, by the very large number of slow-speed machines in course of construction going through the shops of the General Electric Co. and the Westinghouse Company of America. On the other hand, at Boston and Chicago turbine stations are in course of construction, the machines being of the vertical type and of 5,000 k.w. capacity. In both these stations, but more especially in the case of the Chicago station, the division of the plant into units complete in themselves has been very thoroughly developed; thus not only was each set supplied with its individual condensing plant and hydraulic pump for the step bearing of the turbine, which is worked at 1,200 lb. pressure per square

inch, but there was by the side of each machine a boiler feed pump; the firemen had, therefore, nothing to look after but their boilers and stokers. Each turbine, again, had its own range of boilers, which normally was kept quite distinct from the rest of the boilers, there being only a—comparatively speaking—small connecting pipe between two adjacent ranges for use in cases of great emergency; there was also a common chimney for two ranges of boilers. Babcock & Wilcox boilers, sometimes with mechanical stokers and sometimes without, appeared to be a universal feature of American power-houses.

With regard to the control of these large units, the Americans have adopted everywhere—in some cases in a most elaborate manner—a complete system of oil-break motor-driven switches with electrical remote control; in some cases duplicate control boards have been provided, so arranged that while one remains in use the other cannot be operated. Duplicate 'bus-bars appear to be universal, and they are so designed as to render it possible to subdivide the feeders and sub-stations which they supply into distinct groups, each of which can be fed by a separate machine.

Turning now from central station practice to the question of transmission and distribution of electricity, we find in America conditions which do not exist in this country. The longest transmission scheme which was visited by the Institution of Electrical Engineers on the occasion of the recent tour was that from Shawinigan on the St. Maurice River in the province of Quebec to Montreal, a distance of about 90 miles. Power is transmitted on this system at a pressure of 50,000 volts, the current being three-phase at 30 periods, and the conductors being of aluminium. This transmission scheme is typical of many other cases, with the result that three-phase working from the transmission point of view was developed much earlier than was the case in this country, where the necessity for high-tension transmission did not arise so soon. At the same time it is noteworthy that while the three-phase transmission schemes have been perfected, the distribution schemes are almost invariably on the original system of three-wire direct current at 110 to 120 volts between the neutral and each of the outers, with a certain amount of alternating current for the scattered suburbs.

Electricity is in much more general use in America for all purposes than is the case in this country, in spite of the fact that in the large cities, at any rate, the price charged is considerably higher than in England; thus in Boston, where they have a lamp connection of 500,000 16 c.p. lamps and 24,000 H.P. in arcs and motors, the normal flat rate for lighting is 10d. per unit, subject to discounts according to current consumption; a small power consumer pays 3d. per unit, and the lowest figure quoted to any party is 1d.

With regard to output, the New York Edison Co., with a population in its area of about 2,000,000, has a maximum load of 56,600 k.w. for lighting and power, with an annual output of 131,000,000 k.w.-hours, which is a greater output than all our London companies and municipalities combined can muster. Montreal is an exceptional case, but it is so interesting that its position is worth recording; it has a population

of 350,000 and 10,500 consumers, compared with the 5,171 consumers of Manchester, with a population nearly twice as large. The annual consumption, which includes power for the tramway service, is no less than 85,000,000 k.w.-hours, and the company which supplies the current is the fortunate possessor of a 75 per cent. load-factor. Amongst the consumers at Montreal is a 3,000 H.P. cotton-mill, which is driven throughout by electricity, paid for at the rate of ½d. per killowatt-hour; the mill has arranged its hours of work so that in certain winter months no current is taken between the hours of 4 p.m. and 7 p.m., hence the low rate of charge. This alteration has only necessitated a reduction of the average weekly working hours from 56 to 54.

Municipalities in America, at any rate in the larger cities, and to a considerable extent in the smaller ones, have not cared to take up the position of public suppliers of electricity, as has been done in this country; they have instead given franchises, usually for fifty years or thereabouts to companies. The lamp question has been solved in America by giving the consumer free lamps. All lamps prior to being sent out are tested for candle-power and consumption, the limit for a 16 c.p. lamp being in the region of 14·5 c.p. and the consumption 3·2 watts per candle-power; all lamps are marked with their voltage, candle-power, and with the name of the supply company. When lamps are brought in, should they give less than 14 c.p. at normal voltage, they are, if sufficiently good, put aside for colouring for shop signs and theatrical purposes; otherwise, if too much blackened, they are destroyed. The life of the lamp is only reckoned as about 500 hours; their price is 7½d.

It is a remarkable fact, and one much commented on in America, that while the American generating costs are as a whole no greater than similar costs in England, being, in fact, considerably lower than are obtained, generally speaking, by London stations, yet the selling price of current in America has to be considerably higher than the price in England in order to obtain the same return on the capital invested. It was a matter of great difficulty to obtain detail costs—including management charges—of American supply undertakings, but it seems pretty clear that the cost of the various consolidations that have taken place has been very appreciable. To begin with, the original companies which formed the consolidated company had frequently adopted different systems of generations and supply; thus some would be alternating with different periodicities, while there would also be considerable variation in the supply pressures. To bring these various systems on to a common basis has been an expensive matter, and it is well known also that consolidations of this description rarely take place without the creation of a large amount of paper capital.

The supply of electricity business is different to most others with which we are familiar owing to the preponderating effect of capital expenditure as compared with running costs upon the selling price, and it behoves us, therefore, to do our utmost to prevent the capital of the undertakings with which we are concerned being unnecessarily inflated; as plant becomes obsolete the capital it represents should be written down out of revenue, and although this may mean a temporary loss of

apparent profit, the undertaking will soon reap the benefit by the increased business which the reduced charges will bring in. It cannot be driven home too strongly that a heavy capital expenditure per kilowatt installed is bound under ordinary conditions to necessitate a high tariff with a correspondingly restricted business. To undertakings, whether worked by companies or municipalities, which intend to cultivate a power load, this is a matter of great importance, and I am of opinion, therefore, that where current has to be transmitted to scattered areas, overhead wires should as far as possible be adopted; where it is necessary, as in the case of the power companies, to convey electricity in many directions over huge areas, it would seem that overhead conductors are essential, if the companies are to attain financial success. Any development in the use of power in their areas cannot but stimulate the demand for electricity throughout the country generally. We shall all watch, therefore, with great interest—not unmixed with anxiety—the progress made by the pioneer stations which these companies are equipping; apart from the supply of power to mines and factories, they should have a large sphere of usefulness in supplying current, not only to those towns which have not generating stations of their own, but also to those which have. In many cases I think it will be found to be to the best interests of those concerned to purchase electric current from outside, in preference to carrying out expensive extensions to existing works, provided that the prices quoted are as reasonable as one has been led to suppose they will be; in any case, municipalities and others will do well before embarking on extensive schemes of expansion to inquire what the power companies are prepared to do, and to compare the terms quoted with their own costs, not forgetting the effect of the proposed capital expenditure on the selling price.

NEWCASTLE LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

E. EUGENE BROWN, Member.

(*ABSTRACT.*)

November 21, 1904.

In delivering my opening address, I propose briefly to review the progress of Electrical Machinery during the last twenty years. During the era before the invention of the dynamo, progress in many branches was impossible for the reason that the primary battery was the only source of current, and the failure of King in 1845 to perfect the incandescent lamp, and of Jacobi in 1834 to run an electric motor boat, were notable examples of the difficulties to be overcome. But the work of Faraday completely changed all this, and gave that vast stimulus to invention to which we owe all subsequent progress. So long as the design of electric machinery was in the hands of philosophical instrument makers little progress was made, and it was only when the mechanical engineer took up the design that success was attained. The progress in mechanical engineering has been very great, and a comparison of early machines with those of to-day shows that empiricism has given place to knowledge and calculation, as the result of the investigations into the properties of metals.

The model of Pacinotti contained all the essential principles which are found in the modern machine, and to the same inventor is due the discovery of the reversibility of the dynamos. The dynamo brought out by Gramme was, however, probably the first machine to be designed from an engineering point of view, which was capable of absorbing many horse-power; the earlier designers could not appreciate the large amount of energy that was transformed, and many of their machines flew to pieces. Gramme also failed to understand the value of a strong field, so that his machines were very limited in their output, owing to sparking caused by the large number of turns and high speed.

Other machines designed on the Gramme principle were those of Schuckert, Gulcher, and others. In these, the armatures were made narrower with embracing pole-pieces, in order to reduce the "dead" wire. The Burgin dynamo was another example, but the Gramme machine of Elwell and Parker constituted a considerable advance on all these, it having only a single layer of wire so as to reduce the air-gap

and render the field stronger. The researches of Kapp and Hopkinson subsequently put dynamo designing on a scientific basis, and the result was soon seen in the excellent machines afterwards made. (The improvements in the design of Gramme machines were described with lantern illustrations.) When the Gramme machine had reached its limitation, it was followed by the drum armature of Siemens and Worms de Romilly. After the early machine of Siemens, which suffered from too many turns on the armature and too weak a field, came Edison, who was the first to design dynamos for large output. Although constructed with a very vague idea of the principles of the magnetic circuit, these showed great progress, and with the modification by Dr. Hopkinson they became very excellent machines. (Some different forms of field magnets were then shown and criticised.)

The drum-bar armature in the two-pole field did most excellent service for many years. But the two-pole field was heavy and expensive to make, besides being limited in output. The 30 inches diameter, 50 inches long machines of Siemens were probably the largest made. These objections led to the designing of the multipolar machine which has reached no limit.

The six-pole dynamo of Lord Elphinstone and Mr. Vincent of 1879 was very advanced in principle, as it had the lap-wound armature and an attempt to get a strong field; but it failed owing to the fact that it was not built on mechanical engineering lines, consequently its armature, with formed coils supported on a papier-maché drum, always flew to pieces.

The toothed or slotted core was the invention of Pacinotti, but the germ of the idea was also contained in the "H" armature of Siemens. Slotted cores were not very successful in the Gramme or two-pole machines, but when the multipolar machine was introduced they were used for mechanical reasons, in spite of the greater tendency to sparking due to the increased self-induction of the coil embedded in iron; the sparking, however, was eliminated by careful design of the machine and the use of carbon brushes. Since then, considerable changes have been introduced in the design of the multipolar machine, resulting in the production of the modern short radial pole machine of to-day, and in the perfection of its proportions.

The steam turbine, with its high speed, has introduced difficulties with regard to sparklessness and fixed lead of the continuous-current dynamo which the ordinary design for slow speed cannot overcome, so new methods have been used which seem to point to quite a revolution in continuous-current dynamo design. The method of "compensating winding" in slots in the pole-pieces, as patented by 1890 by Déri, appears to have been used successfully.

In 1882, Messrs. Gaulard and Gibb brought out the transformer system which caused a great stir in the electrical world. The continuous-current dynamo was declared to be obsolete for the purpose of town lighting, as the distribution of electrical energy at 110 volts, the highest voltage of the lamp of that time, was of course restricted to small areas, the cost of the copper mains being so great. Then the three-wire system of Hopkinson and Edison put the continuous-current system upon

a new footing, and the 200 to 250-volt lamp again increased the area over which distribution could be economically made. It is difficult to say what the system of the future will be, as it depends upon so many things. The primary source of energy and the method of transformation will probably be quite different to the present methods.

It is interesting to note that the new single-phase repulsion motor can work on either alternating or direct-current circuits, the armature being the ordinary direct-current with series or wave winding, and the field like the stator of an induction motor; when running as a single-phase motor the brushes are short-circuited and give the necessary polarity for the so-called repulsion action. The more this motor is studied, the more is one impressed with its importance.

It has been acknowledged that the three-phase system has not been a success on electrical railways, as, when used direct on the motor, it introduces great complication in the regulating arrangements, and complication means increased risk of breakdown. The direct current, with its simpler regulation, is better. The electrification of the entire system of railways is, beyond doubt, the great problem coming upon us in the near future. With regard to short lines the methods used have been fairly successful, as the system with short runs, requiring large acceleration after the numerous stops, has been well provided for; but with the long runs on the trunk lines the conditions are quite different, and require very careful consideration before a successful universal system is evolved which can be carried out all over the kingdom.

To sum up the progress to the present day. The greatest optimism should prevail, seeing how much has been accomplished. Much still remains, however, to be done, and every day brings news of some fresh discovery which, often trifling in itself, may be the starting-point to some important line of development. Let the imagination go where it will, nothing can be imagined that is more wonderful than the discoveries which every now and then startle the electrical world. It is years since it was said, that seeing the dynamo machine had attained over 90 per cent. efficiency, there was but little room for improvement, but nevertheless improvements have gone on, and the efforts to improve still further are unflagging. Telegraphy without wires was at one time deemed impossible, and now Tesla has proposed the transmission of energy without wires. No electrical engineer of the present day would say that it was impossible. The efforts to overcome distance will in the near future be crowned with success, and this seems to be the most important point of all. Now that distribution is comparatively easy, it is important that research should be concentrated on methods for rendering available supplies of energy in the form of water-power; or what is more important in the United Kingdom, bringing the energy of coal, or even peat, into the large towns. If this could be done at a low cost, then the era spoken of by Mr. Ferranti, of the universal use of electrical energy for light, heat, and cooking, as well as for all motive power, would be near accomplishment; but the prime motor, if coal is used, must be improved. The losses in transmitting the energy contained in the coal to the dynamo must be lessened, if cheap electrical energy is to be delivered to the masses.

But improvement in the utilisation of the current for lighting must yet be made. The incandescent lamp of about 5 per cent. lighting efficiency is but a crude method of using the current. The Nernst lamp is much better; the Cooper-Hewitt mercury vapour lamp is going in the right direction in efficiency, as the percentage of heat rays given out is smaller, but the colour of the light is not suitable for ordinary use. Possibly some method based upon the vacuum-tube method of high frequency will some day be utilised. If we could find out how the light is produced in the glow-worm and Cuban firefly, which give out radiance nearly all light rays, then some progress will be made, and it is certain to come as it is possible. The wave lengths of the electrical waves used in wireless telegraphy are known, and if these could be shortened so as to coincide with light waves, which as far as is known are of the same kind, then they could be transmitted through space and be used much in the same way as wireless telegraphy, by acting upon some susceptible medium. The discovery of radium, and the different kinds of rays which are supposed to emanate therefrom, have set the whole scientific world experimenting, and it is certain that something will be discovered which will lead to electrical energy taking up a position of even greater importance than at present. The American system of experimenting has been said to be too extravagant, the percentage of successes being small. In spite of this the Americans accomplish great things, and most of the important undertakings have been pioneered there. Experiments upon a great scale must be made if great things are to be done, and why should England be content at mere copying? In the progress of the world, commercialism is not to be considered. There is plenty of capital in England, let some of it be spent in the carrying out of great experiments for the common good, then the universal use of electrical energy by everybody for their daily wants may be the result. In conclusion, thanks are due to Dr. Silvanus P. Thompson, to Mr. John Holmes, and to Mr. Haggarty, for their kind help with regard to the lantern slides.

BIRMINGHAM LOCAL SECTION.

THE USE OF IRON IN ALTERNATE-CURRENT INSTRUMENTS

By W. E. SUMPNER, D.Sc., Member.

(*Delivered as an Inaugural Address, November* 23, 1904.)

It is probably no exaggeration to state that the amount of time, ingenuity, and skill, devoted during the last ten years to the improvement of alternate-current instruments, has exceeded that spent in perfecting those for direct-current measurements during a period more than twice as long. Yet no one will maintain that the result is anything like so satisfactory in the former case as in the latter. It will, I hope, be interesting to spend some time this evening in considering the reasons for this, and what prospect there may be of a better result in the future.

The first idea that will no doubt occur is that alternate-current measurements are more complicated than those with steady currents, and that in consequence the tests are not likely to be so accurate. Now I believe this view to be entirely wrong. Direct-current measurements, as the result of a development spreading over many years, are really much more numerous and varied, and are certainly quite as intricate in character as those practised in alternate-current working. It is not so much in the nature of the measurements to be made, as in the character of the instruments available for the tests, that the difference really lies. The result, I believe, is almost entirely due to the fact that for alternate currents there is no instrument at all comparable in sensitiveness with the galvanometers and voltmeters available for steady current work.

Now perhaps this idea, though familiar to some, will take others by surprise. But if we proceed to consider how much all good direct-current tests depend upon sensitive galvanometers or voltmeters, we shall, I think, most of us be surprised. Take the three fundamental cases of the Wheatstone bridge, the potentiometer, and the voltameter. In each of these instances the accuracy of the test is directly dependent on the use of a delicate voltmeter to adjust to equality two voltages. In the first case the adjustment is made in order to measure a resistance in terms of a standard. In the second, by means of known resistances, a current, or voltage, is measured in terms of these resistances and the E.M.F. of a standard cell. In the third case, in which,

say, a current balance is being standardised, although the voltameter measurement does not need a voltmeter, the adjustment of the current to a constant value is directly dependent on the use of such an instrument, and this adjustment is just as essential to accuracy as the correct measurement of the amount of chemical action. Measurements of insulation, magnetic induction, and capacity would hardly be possible without delicate galvanometers. Indeed the sensitive voltmeter seems the one common essential to all accurate electrical testing.

It is of little practical use to speculate as to what would have been the progress of pure and applied electrical science if such instruments had not been available for direct-current testing. But it is hardly possible to overstate the important part they have had in bringing about this progress, and every one will admit that the precision possible in the design of electrical apparatus, whether for direct or alternate currents, is almost entirely due to accurate measurements made with them. Most of the tests needed in connection with alternate-current machinery are still carried out by direct-current methods.

It is, of course, to the use of permanently magnetised steel that direct-current instruments owe their great sensitiveness. Whether of the fixed coil or moving coil types, galvanometers may be described as single-current instruments in which the deflecting force is proportional directly to the current or voltage applied. Unfortunately all alternate-current instruments have the characteristics of the double-current type, in which the deflecting force is proportional to the square of the current or voltage. This is obviously true of dynamometers, current-balances, wattmeters, phasemeters, and electrostatic instruments. It is also true of induction instruments, whether the current induces magnetism, as in moving-iron instruments, or eddy currents, as in the more recent induction meters. Hot-wire instruments have the same characteristic, since the heating effect varies as the product of volts and amperes. Instruments in which the deflecting force varies as the square of the applied voltage cannot be made sensitive for minute voltages, owing to the rapid diminution of the force as the voltage decreases, and this constitutes the main reason why alternate-current instruments cannot be made to indicate low voltages.

Hot-wire instruments are as yet the most suitable for measuring small alternating voltages. Commercial instruments can be obtained reading from one volt down to about one-tenth of a volt. Professor Threlfall has described an instrument before this Institution capable of measuring one-hundredth of a volt, and Professor Fleming has shown a somewhat similar instrument to the Physical Society of London. Mr. Duddell has recently made an instrument which may be described as a combination of a hot-wire voltmeter with the radiomicrometer of Professor Boys, the heat from the wire causing a thermoelectric current in a delicately suspended electric circuit placed in the field of a strong permanent magnet. With such an instrument, using as pointer a reflected ray of light, with the scale at one metre distance, it is possible to get a deflection of more than one centimetre per *microwatt* spent in the hot wire. But such instruments, and especially the last, are more fitted for laboratory and research purposes than for ordinary testing.

An instrument may be made sensitive either by weakening the control, or by increasing the forces causing the deflection. The strength of the control is a fair measure of the suitability of the meter for ordinary commercial work, since the weaker the control the more tenderly must the instrument be used, and the more liable are its indications to be affected by extraneous influences. The true measure of the sensitiveness (except for research purposes) is therefore the turning moment exerted per unit of current or voltage applied. Now it is generally stated that a galvanometer of the fixed-coil, or Thomson, type is more sensitive than the moving-coil form of instrument; but this is merely due to the fact that the control used can be made, and is made, much weaker in the former than in the latter case. With equally strong control the permanent magnet form of instrument is much the more sensitive of the two, owing to the strength of the field applied to the coil conveying the current to be measured. In order to make alternate-current instruments as sensitive as the corresponding direct-current ones, strong magnetic fields must be produced. The permanent magnet is useless for alternate-current work unless satisfactory instruments of the vibration type can be devised; but the electromagnet, the most powerful form of magnet, still remains a possibility. Electromagnets are not used in galvanometers because existing instruments are sufficiently responsive, and thus only when extreme sensitiveness is needed—as, for instance, in the oscillograph—is any one willing to put up with the inconvenience of supplying a subsidiary magnetising current, and maintaining it of constant strength. The problem of producing sensitive alternate-current instruments resolves itself therefore into an enquiry into the conditions under which iron-cored electromagnets may be introduced into such instruments, without involving errors due to differences of phase, frequency, wave-form, and other disturbing factors.

As an example of the need for more sensitive alternate-current instruments, I may instance the important problem of directly measuring an alternate current, or voltage, in terms of direct-current standards, such as a known resistance, and the electromotive force of a standard cell. As yet the only standards available for alternate-current work are of a secondary character, and consist of expensively constructed instruments which have been standardised by direct-current measurements. Some years ago I tried to find a means of using the Crompton potentiometer for the direct measurement of an alternate-current voltage in terms of a standard cell. I soon saw how this could theoretically be done, but I have not succeeded in making or in finding an instrument sensitive enough to satisfactorily carry out the test. It may, however, be interesting, in connection with our subject to-night, to state the method. The problem consists in accurately balancing two voltages each of the order of a volt, the one produced by a steady current, and the other by an alternating one. The current or voltage to be measured, whether direct or alternating, can be easily made to produce such voltages with the aid of shunts, or subdivided resistances, in the way commonly used with potentiometers. The direct voltage D is produced by the potentiometer, is adjustable by means of the

sliding contact, and is accurately known. The alternating voltage should consist of two portions A_1 and A_2 in series. The ratios of A_1 and A_2 to each other, and to the current or voltage to be tested, are supposed to be accurately known from the shunts or resistances employed. If now one of the terminals of the voltage D be connected to the junction of A_1 and A_2; if a voltmeter of the dynamometer type have its terminals connected to the extremities of the voltages A_1 and A_2; and if the free extremity of D be connected with the junction between the fixed and moving coils of the dynamometer, it will be seen that the voltages on the dynamometer coils are respectively $D + A_1$ and $D - A_2$.

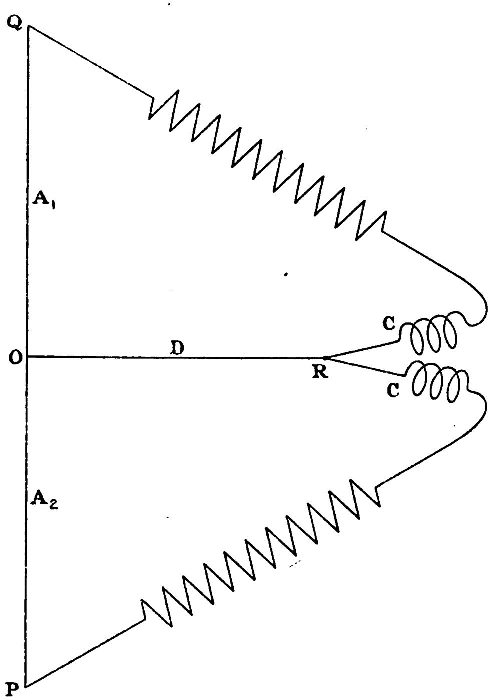


FIG. 1.

Thus in Fig. 1, O is the middle terminal, and P, Q the extreme terminals, of the alternating voltage $A_1 + A_2$. The direct voltage D is connected with the points O and R. The dynamometer coils C C are joined through suitable non-inductive resistances to the points P R and Q R.

Under these circumstances the turning moment exerted on the moving coil is proportional to the product $(D + A_1)(D - A_2)$. This is true for any instant, and thus must be true for the mean. Since D is constant, A_1 and A_2 are alternating quantities, and $A_2 = k\,A_1$, where k is some known constant, it will be seen that the mean product is—

$$D^2 - k A^2,$$
where A^2 is the mean value of A_1^2.

The instrument, if sufficiently sensitive, will only give zero deflection when $D^2 = k A^2$, or, supposing $k = 1$, when $A = D$. This means that the alternating voltage A is measured directly in terms of the standard cell, in a manner quite independent of frequency or wave-form. But the method, though perfect in theory, is impracticable, as is also the analogous method represented by interchanging the letters A and D in Fig. 1, in which case the direct-current voltage D is subdivided instead of the alternating one. An instrument of sufficient sensitiveness could only be made by weakening the control to an extent rendering it unsuitable for common use, and the method only appears to be applicable by establishing a subsidiary voltage D much greater than a volt, balancing it against the unknown alternating voltage, and measuring it immediately afterwards by the potentiometer in the ordinary way. Nevertheless, the mere invention of a sensitive dynamometer voltmeter would at once render the Crompton potentiometer, as now made, as suitable for the measurement of alternating currents and voltages as of the corresponding direct-current quantities for which it is so useful.

Now before discussing how to use iron in alternating-current instruments, it must first be stated that this is already done in many cases, as in moving-iron indicators, and in instruments of the eddy-current type. The latter now form an important class, and are either of the shielded pole type, the outcome of some pioneer experiments of Elihu Thomson, or of the rotary field type due originally to Ferraris. The rotating field instruments are particularly useful. They have the great advantage that the geometrical relations of the fixed and moving parts are not changed as the disc or cylinder turns, so that the indicating instruments possess regularly divided scales, and a range of deflection extending to nearly 360 degrees. Such instruments have many undoubted merits, but their calibration varies with frequency, and to a slight extent with wave-form. The wattmeters of this class are indeed not much more than good indicators. They are not the less useful on that account. They fulfil all the requirements of central station use, but for accurate measurement something more is needed.

The first instrument to which I wish to direct your attention is the Phasemeter, or Power-factor Indicator. These instruments may be made for either single-phase or multi-phase circuits. In the former case the calibration depends somewhat on the frequency, but only very slightly so in the latter. All such instruments consist of a fixed and moving set of coils, having a common diameter about which the moving system turns. The moving system is quite free to revolve. There is no control except that due to the interaction of the currents. Each system may comprise more than one coil, but as a rule the fixed system consists of a single coil through which the current passes, while the movable system consists of two coils inclined at some fixed angle to each other, and traversed by currents in different phases, obtained usually by tapping the multiphase mains, but, in the case of single-phase

circuits, by providing two paths of different inductive properties for the pressure currents needed for the coils.

Now, in the theory of these instruments as hitherto given, the action of the current in the fixed coil on that of each of the moving coils is separately considered. Two moments are exerted, one on each moving coil, and the position of balance indicated by the pointer is such that these moments are equal and opposite. This position is independent of the amperes, or volts, since alteration in either of these equally affects the two moments, so that if these moments are balanced for, say, 20 amperes, they are equally balanced for 30 amperes. The position of balance can therefore only depend upon the phase differences between volts and amperes, and hence the deflection of the pointer can be calibrated to indicate phase.

But there is another way of looking at the action which is useful to consider, and which I think has not been previously noticed. The two-coil system traversed by currents in different phase produces a rotating field, and the interaction between this and the alternating (or rotating) field due to the current (or currents) in the other system of coils must be such that there is only one stable position of the moving system of coils for which the torque vanishes, and this position can only depend upon the phase differences involved in the two systems of currents. This is illustrated in rotary synchronisers such as that of Everett Edgcumbe, the rotor and stator of which are each wound so as to produce a rotating field by splitting a single-phase current into an inductive and a non-inductive branch. If two generators are joined up, one to the rotor and the other to the stator, the rotor revolves rapidly if the frequencies are different, but as these approach equality the rotation is slow, clockwise if one machine leads, counter clockwise if it lags; the position of the pointer indicates at each moment the phase difference of the two currents. If the two machines are coupled together so as to generate voltages in fixed phase relation, the pointer attached to the synchroniser takes up a fixed position in correspondence with this phase. The instrument thus becomes a phasemeter and can be calibrated as such, but in its present form it is not suitable as a power-factor indicator.

Now imagine a coil traversed by an alternating current placed in a rotating magnetic field of the same frequency, and free to turn about the axis of rotation of this field. The position taken up by the coil depends merely upon the phase of the current through the coil. If we assume uniformly revolving fields, and currents varying according to the sine law, the position will be such that at the instant the rotating field is parallel to the coil, the current in the latter is passing through its zero value. The angular deflection of the pointer from some fixed point on the scale measured in degrees, will be an exact measure of the phase of the current. If the currents are of irregular wave-form, and the rotating field is not uniform, this will not be precisely the case (see Appendix), but there will in all cases be a fixed position of the coil for each phase, independent of the magnitudes of the currents in the different coils, and quite independent of the frequency since this will affect currents and fields alike. The most efficient way of producing

strong rotating fields is of course well known. It is exemplified in every induction motor. But it does not appear to have been noticed hitherto that phasemeters really act by means of rotating fields, and hence the means adopted in practice for producing such fields in commercial instruments are anything but the best available.

To illustrate this and other points I have had some instruments constructed in the Technical School workshops, all designed for strong alternating magnetic fields. The first is the phasemeter illustrated in Fig. 2. The fixed coils are wound through circular ring stampings just as in the stator of a three-phase motor. There are three coils, each of 8 turns, wound with wire suitable for 5 or 6 amperes, with each coil brought to a separate pair of terminals. The portion corresponding with the rotor consists of a cylindrical block of circular stampings held in a fixed central position, and unwound. In the air-gap between the two is placed a pivoted coil similar to those used on moving coil galvanometers or voltmeters, but without control. It is capable of free motion about the axis of the instrument and its position is indicated on the scale by an attached pointer. I have arranged some experiments which I think will convince you that this instrument is a very satisfactory indicator of phase or power-factor. The position taken up by the pointer is quite definite under any given circumstances, and if the coil is moved out of its equilibrium position the forces tending to make it return are considerable, in spite of the fact that a very large non-inductive resistance is used in series with the pressure coil. This is only what is to be expected, since the strength of the field due to the fixed coils is about equal to that usual in permanent magnet direct-current instruments. Here at all events is one alternating-current instrument in which strong fields are employed without any of the disadvantages and errors supposed to be associated with the use of iron under such circumstances. Of course it would be possible to wind the fixed coils for pressure instead of current, either for direct connection to the mains or, if necessary, for the secondary of a step-down transformer. In many cases the moving coil would be put in series with the secondary of a current transformer. It is usual now with high-tension mains to have two current transformers, each primary being placed in one of the mains, and each secondary circuit, after passing through an ammeter, returning through a path common to both secondary circuits, and in which an ammeter and power-factor indicator are placed. The connections are shown diagrammatically in Fig. 3, in which A_1, A_2, A_3 represent the ammeters, (1), (2), and (3) indicate the mains, in the first two of which the primaries of the current transformers $C\,T_1$ and $C\,T_2$ are placed, and the power-factor indicator is represented by P.F.I. Owing to the vector properties of alternating currents, if the current transformers are alike, the current measured by the ammeter A_3 will indicate the current in the third main, as shown by the vector diagram in Fig. 4. With such an arrangement it would be advantageous to use a reversing switch (short-circuiting during reversal) on the secondary of one of the transformers, say CT_1, as indicated in Fig. 3. By reversing this switch A_2 is unaltered, A_1 becomes A'_1, equal but opposite in phase to A_1, and A_3 becomes A'_3, about 70 per cent. greater than A_3 in magnitude, and strictly in quad-

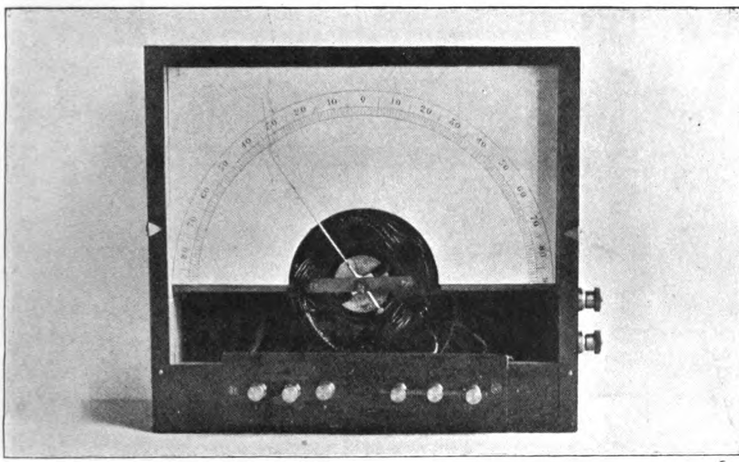


FIG. 2.—Phasemeter, or Power-factor Indicator.

rature with it, whatever the wave-form may be, provided only A_1 and A_2 are equal in magnitude. These relations are shown in the vector diagram Fig. 4, in which, owing to the properties of a right-angled triangle, A'_3 is perpendicular to A_3 if A_1, A'_1, and A_2 are equal to each other, whatever may be the phase difference between A_1 and A_2. The advantage of this device arises from the fact that the power-factor indicator can be made to read on circuits of low power-factor as well as

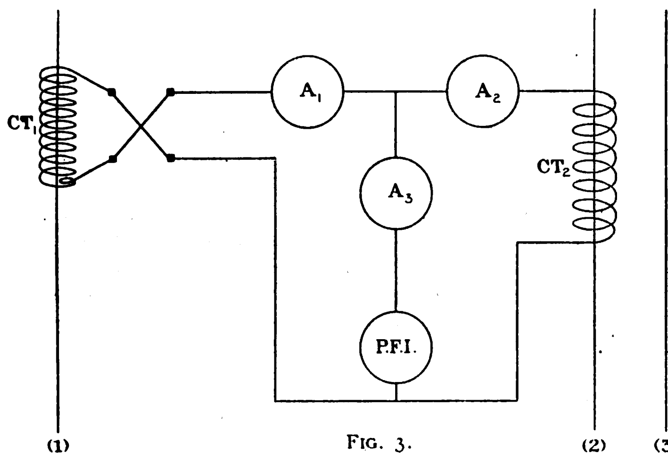


FIG. 3.

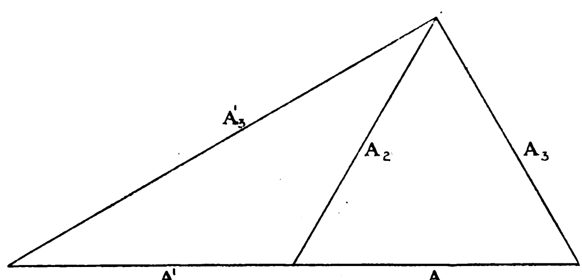


FIG. 4.

on those of high power-factor. As a rule these instruments are calibrated between power-factors 0·5 and 1·0, and are not available for low power-factor circuits. By using the reversing switch the phase of the current passing through the instrument can be altered by 90 electrical degrees, so that the position corresponding with unity power-factor is made to denote zero power-factor, and by means of an additional calibration of the scale low power-factors can be measured. If satisfactory power-factor indicators can be constructed, they will prove more serviceable than wattmeters on highly inductive circuits, since the latter instru-

ments under such conditions are liable to large percentage errors, and their indications are low down on the scale.

I now pass to the consideration of Wattmeters. The introduction of electromagnets into these instruments is naturally a more difficult problem. With the exception of electrostatic instruments, all accurate wattmeters are of the dynamometer type. Iron, and indeed all metal parts, are severely excluded, with the result that these instruments are insensitive, lacking in range, and subject to disturbances caused by external fields. It has nevertheless become necessary to use transformers in conjunction with them, owing to the high potentials and heavy currents now common. These accessories of course contain iron, and introduce errors, due to small differences in phase, which are as serious, I believe, as those which would arise from the use of well-designed electromagnets in the wattmeter itself.

The error in wattmeters due to the lag of current in the pressure coil has frequently been theorised about, but I think a somewhat exaggerated idea exists as to its nature and extent. A commercial wattmeter is intended to work on circuits of fixed voltage, and for a maximum load current. I have calculated in the accompanying table the wattmeter error caused by one degree lag in the pressure coil, for a fixed load current corresponding, say, with 1,000 volt-amperes, and for loads of different power-factor.

TABLE I.

WATTMETER ERROR FOR A LOAD OF 1,000 VOLT-AMPERES (FOR A LAG OF ONE DEGREE IN THE PRESSURE COIL).

Power Factor.	True Watts.	Error.	Error per Cent.
1·0	1000	0·3	0·03
0·9	900	7·6	0·85
0·8	800	10·5	1·31
0·7	700	12·5	1·78
0·6	600	13·9	2·32
0·5	500	15·1	3·02
0·4	400	15·9	3·98
0·3	300	16·6	5·54
0·2	200	17·1	8·55
0·1	100	17·3	17·3
0·02	20	17·4	87·2

The error increases the reading if the load current lags, and diminishes it if it leads. If the power-factor of the load is cos ϕ, the error in the reading is proportional to sin ϕ, while tan ϕ represents the error per cent. of the true reading. It will be seen that the error in the reading, instead of indefinitely increasing as the power-factor diminishes, rapidly attains a maximum value, which is less than 2 per cent. of the reading corresponding with the same current if non-inductive. The error per cent. may be enormous for low power-factors, but the error as a deflection may all the same be unreadable unless the instrument has a wide

scale. (The theory of this error, and the application to wattmeters used on three-phase circuits, are given in the Appendix.)

The lag in the pressure coil can be easily reduced to one-tenth of a degree, and the error caused by it is proportionately diminished. It is not so well known that the phase differences introduced by current and voltage transformers can, if these are properly designed, be reduced to similar small values. Of course the errors due to such phase differences are of the same nature and importance as if they were all due to lag in the pressure coil.

Electromagnets may be introduced into wattmeters either by putting the magnetising coil in series with the current, or by connecting it in shunt across the mains. In the former case, owing to hysteresis, eddy currents, and varying permeability, the induced magnetism will not properly follow the variations of the current, even when the magnetic circuit contains an air-gap, and only low magnetic densities are needed. On the other hand, if a shunt coil is used to produce the magnetism, not only is there an error due to the resistance of the coil, but the flux of magnetism is practically in quadrature with the voltage producing it. The resulting errors can perhaps be made negligible in either case, but it is the latter method I have tried experimentally lately, in order to bring the question before you.

If N is the magnetic flux in the core of a transformer or electromagnet produced by a current C through a winding of n_1 turns of resistance R, the voltage V_1 necessary to produce this current is given by—

$$V_1 = RC + n_1 \frac{dN}{dt}.$$

We also have—

$$V_2 = \qquad n_2 \frac{dN}{dt},$$

where V_2 is the voltage on open circuit of a secondary winding of n_2 turns (assuming no magnetic leakage). For large transformers on open circuit the term RC may be less than one ten-thousandth part of V_1. For magnetic circuits of small dimensions, suitable for instruments, especially if an air-gap is introduced to allow a moving coil to cut the flux, the term RC is much larger compared with V_1, but may still be a small proportion of it. As a rule the copper drop CR is by no means negligible compared with V_1. This is only too noticeable when testing small masses of iron for hysteresis losses by the wattmeter method, since the losses in the magnetising coil are often an appreciable fraction of the whole. In such cases I have often found it convenient to use two windings (as shown in Fig. 5), one of a few turns n_1, suitable for a considerable magnetising current C passed through the current coil of the wattmeter W; the other of a large number of turns n_2, connected with the pressure coil of the instrument. The reading of the wattmeter must then be divided by the ratio n_2/n_1 in order to get the watts wasted in the iron circuit. This method has several advantages. Only the core losses are measured, the losses in the magnetising coil becoming a matter of indifference. A wattmeter suitable for heavy currents, and

the measurement of large amounts of power, may be utilised for the estimation of only a few watts; and the flux can be calculated independent of the copper drop in the primary coil, by attaching a voltmeter V to the secondary coil, the resistance of this being allowed for, if need be, by the ordinary fall of potential method.

Unfortunately there does not appear to be any similar method of eliminating the copper drop when the electromagnet is to be used as part of the wattmeter itself. The effect of hysteresis, eddy currents, and variable permeability is to alter the R C term; but if we assume that by suitable design this term R C can be made negligible compared with V_1, we have—

$$V_1 = n_1 \frac{dN}{dt},$$

and hence, whatever the nature of the iron, the strength of the flux N,

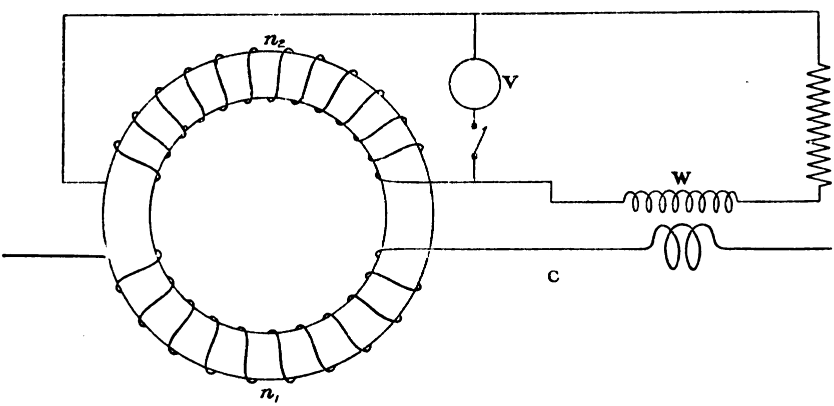


FIG. 5.

and therefore the magnetic density in each part of the air-gap, is strictly proportional to V_1, and in perfect quadrature with it. If now the load current A be passed through the primary of an air-core transformer (shielded if necessary from external fields by a shell of suitably laminated iron), the voltage induced in the secondary coil will be proportional to A in magnitude, and in quadrature with it as regards phase. It may thus be used to produce a minute current *a* through a large non-inductive resistance, and the moving coil of the instrument. Theory then shows (see Appendix) that if *a* is in quadrature with A, and if N is in quadrature with V_1, the torque on the moving coil is proportional to the average value of the product V_1A, and therefore measures watts, independently of frequency and wave-form, and of the nature of the iron used in the electromagnet.

The first instrument using this principle which I had made was of the reflecting type, having a metallically suspended coil as in ordinary moving coil galvanometers. Its action was promising. I have since

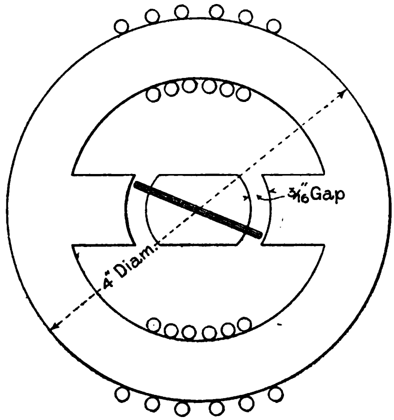


FIG. 6.

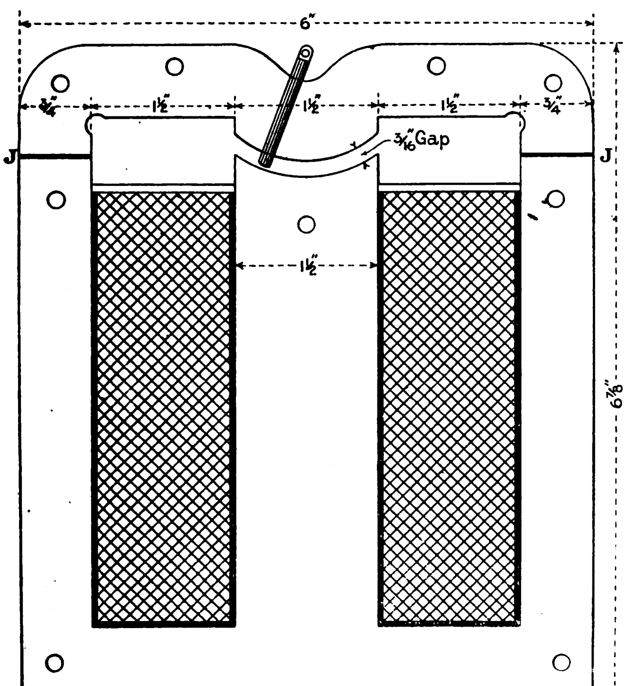
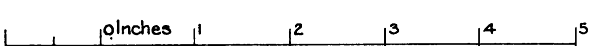


FIG. 7.



constructed two non-reflecting instruments (illustrated in Figs. 6, 7 and 8) which I now show you. In each of these instruments the moving coil is exactly similar to those used in commercial moving coil instruments for direct currents. They are pivoted and controlled by helical springs of the usual kind. The magnetic circuits are indicated in Figs. 6 and 7. That of the earlier instrument is of the Manchester dynamo type, and contains two air-gaps. The later instrument has a magnet of the Lahmeyer type, but arranged to have only one air-gap, the moving coil turning about an axis coincident with one of the long sides of the coil, and placed outside the magnetic field, the opposite side moving through the air-gap. The magnetic circuit is jointed along the line J J to allow of the insertion of the coils. The complete instrument with its current transformer and resistance is shown in Fig. 8. By winding the magnetising coil on the centre limb as in Fig. 7, instead of on the two side limbs as in Fig. 6, the effect of an ampere-turn is doubled, and by reducing the number of air-gaps from two to one the effect is again doubled. To further reduce the copper drop C R as compared with V_1, the voltage on the magnetising coil, this coil should be liberally supplied with copper, and should fill as much winding space as possible. Of course the air-gap should be a minimum. In the instruments shown, each air-gap is $\frac{3}{16}$ths of an inch across. It is quite possible to halve this, and still allow plenty of room for the free movement of the coil.

The calibration curve of the instrument outlined in Fig. 6 was tested by passing direct currents through the fixed and moving coils. For magnetising currents producing air-gap densities similar to those found in direct-current moving-coil instruments, the calibration curve obtained by varying the current in the moving coil was always of the same shape. When the deflection was interpreted by means of this calibration curve the reading was found strictly proportional to the product of the currents in the two coils. The instrument was then tested with alternate currents against a standard wattmeter on a non-inductive load of lamps. A voltage transformer was used to reduce the voltage of the circuit to one suitable for the instrument. The minute current through the moving coil (having 50 turns and a resistance 6 ohms) used in series with a non-inductive resistance of 300 ohms, was taken from a few turns of thin wire forming the secondary of a small iron-cored transformer, the primary of which consisted of two turns carrying the load current. The experiments showed that the calibration curve, or the relation between deflection in degrees, and watts, was exactly the same as that obtained with direct currents, for a load non-inductive throughout the tests. When the power-factor of the load was altered from 1·0 to about 0·1 it was, however, found that the constant of the instrument was considerably changed. This result was due partly to the fact that the copper drop in the magnetising coil was not negligible compared with the applied voltage, but it was also largely due to the phase differences introduced by the two transformers, each of which had closed iron magnetic circuits. These transformers were indeed quite unsuited for the purpose, and the copper drop in the magnetising coil of the instrument was (at 40 cycles) about 20 per cent. of the total

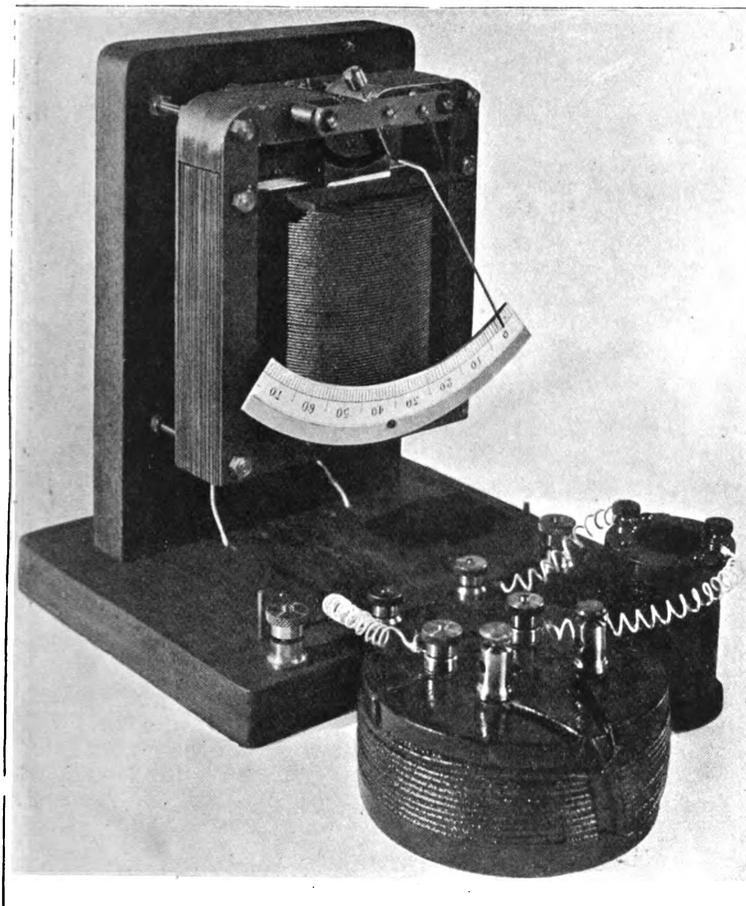


Fig. 8.—Wattmeter, with Current Transformer.

voltage applied. But even under these circumstances the deflection of the moving coil, as interpreted by the calibration curve obtained with direct currents, was strictly proportional to the watts for a load constant in power-factor and frequency.

No further experiments were made on this instrument, which was constructed simply out of such stampings as were to hand, while waiting the arrival of special stampings ordered for the instrument shown in Fig. 8. In this instrument much more space was allowed for the magnetising coil, and in other ways already mentioned the construction was modified with the view of reducing the copper drop. The magnetising coil consists of 1051 turns of No. 18 wire, having a resistance of 3·68 ohms. The section of the middle limb of the magnetic circuit is a trifle over 2 square inches, the length of the winding bobbin is about 4 inches, the air-gap is $\frac{1}{16}$ths of an inch. The moving coil consists of about 60 turns, having a resistance of 5·4 ohms. The structure is such that the moving coil can turn through an angle of 80 degrees without leaving the field. When tested with currents of 80 cycles per second, it was found that 54 volts produced a current of 0·13 ampere through the magnetising coil. It follows that the copper drop is only 0·88 per cent. of the applied voltage when the frequency of the current is 80. This means that even supposing there are no hysteresis or other losses in the iron circuit, the phase of the applied voltage cannot differ from that of the induced voltage due to the flux in the coil by more than half a degree. If there are hysteresis losses, this phase difference will not be increased but diminished, since the vectors representing the copper drop, and the voltage due to the magnetic flux, will no longer be at right angles to each other. In fact, hysteresis is an advantage from this point of view, although of course it increases the losses involved in producing the electromagnet. The losses in the magnetising coil are quite trifling. The copper losses for 0·13 ampere only amount to 0·062 watt. The iron losses I have not measured,* but they must be small owing to low induction density and the small volume of iron. For the current transformer for this instrument I have simply wound a coil of 10 turns (resistance 0·0163 ohm) so as to surround a secondary coil of about 600 turns having a resistance of 8·5 ohms. No iron was used with this transformer. The copper losses for 20 amperes through the primary are 6·4 watts. These could easily be made negligible by using thicker wire.

The results of several tests on the instrument with a resistance of 500 ohms in series with the moving coil and the current transformer are shown in Fig. 9. The calibration curve was first found by taking two sets of tests with direct currents. In one of these the magnetising current used was kept constant at 0·10 ampere, and in the other set it was made 0·15 ampere. Various currents were passed through the moving coil, and the corresponding deflection in degrees shown by the pointer was noted. When the product of the two currents, each measured in milliamperes, was 1,006, a deflection of 50 degrees was

* The hysteresis losses have been since tested. They are found to be 3·85 watts at 100 volts, with a current alternating at 100 cycles per second.

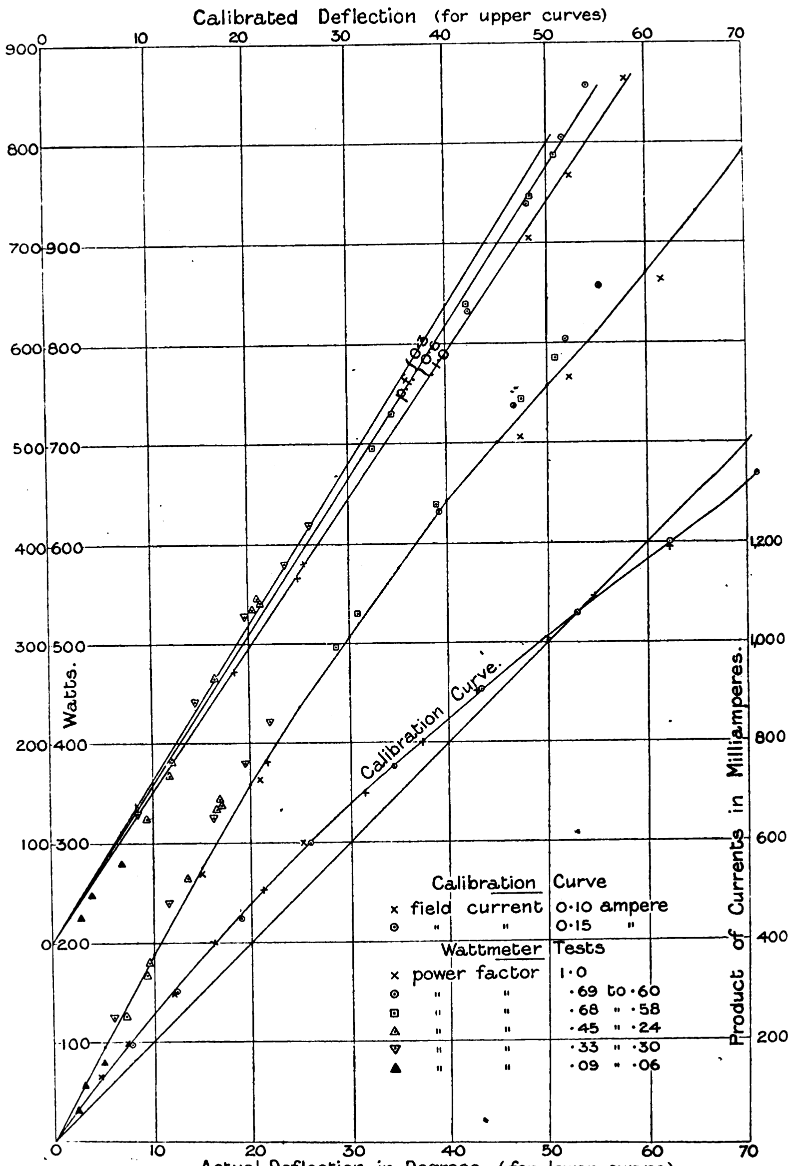

Calibrated Deflection (for upper curves)
0
10
20
30
40
50
60
70
900
800
700
600
500
400
300
200
100
0
900
800
700
600
500
400
300
200
100
Watts.
p.f. ·03
·06
1·0
Calibration Curve.
1,200
1,000
800
600
400
200
Product of Currents in Milliamperes.
Calibration Curve
× field current 0·10 ampere
⊙ " " 0·15 "
Wattmeter Tests
× power factor 1·0
⊙ " " ·69 to ·60
⊡ " " ·68 " ·58
△ " " ·45 " ·24
▽ " " ·33 " ·30
▲ " " ·09 " ·06
0
10
20
30
40
50
60
70
Actual Deflection in Degrees (for lower curves)

FIG. 9.

obtained. The observations are plotted to form the calibration curve, the two sets of points being distinguished by different signs, as shown. It will be noticed that both sets of points lie well on the same curve. A straight line is drawn from the zero through the point corresponding with 50 degrees, in order to show the deviation of the calibration curve from strict proportionality. Six sets of tests with alternating currents were then made on loads of different power-factor, but with the current frequency kept constant at about 80 cycles, and the voltage of the circuit in all cases being about 60. In each set of tests, except for the non-inductive load, the power-factor of the load diminished as the current and deflection increased, the limiting values being indicated, and the points corresponding with different sets of tests being separately distinguished, as shown in Fig. 9. The line drawn among the points is a reproduction of the calibration curve, the ordinates of the latter being all increased by 50 per cent., in order to show how far this curve indicates the relation between the deflection and the watts. It appears from these tests, and from numerous others which I have made, that the same calibration curve holds for the instrument under all circumstances, or, in other words, that for a specially prepared scale determined from this calibration curve the reading of the pointer will be proportional to watts for a varying load of any fixed power-factor and frequency. The constant to be used with such a scale is the same for all currents and voltages if the power-factor and frequency are unaltered, assuming, of course, no change in the transformer circuit. The constant of the instrument does change to some extent for alterations of frequency and power-factor. For the tests shown in Fig. 9 the frequency was kept at 80 cycles, but the power-factor of the loads used in the experiments ranged between 1·0 and 0·06. The divergences of the plotted observations from the calibration curve drawn among the points indicate the alteration of constant with the power-factor of the load. To show this more clearly the observations have been replotted as shown in the upper curves of Fig. 9. In these curves the observed power in watts is plotted, not with the actual deflection of the instrument, but with the calibrated deflection as determined from the calibration curve for a scale properly graduated, and such that the reading is 50 for a deflection of 50 degrees. Under these circumstances all the observations for loads of the same power-factor should lie on a straight line drawn through the zero, and this proves to be the case. It appears from these tests that the constant for unity power-factor loads differs by about 4 per cent. from that for loads of power-factor 0·6, and by about 8 per cent. from that for loads of 0·3 power-factor. These changes are much greater than correspond with the phase difference of less than 0·5 degree due to the copper drop in the magnetising coil, since reference to Table I. will show that for the power-factors mentioned, the changes in the constant should be less than 1·16 per cent. and 2·77 per cent. respectively. This shows that the main part of the alteration of constant is caused by the current transformer,*

* More recent tests have shown that the view here stated is quite correct. The self-induction of the secondary of this transformer is sufficient entirely to account for the errors found.

which was not specially designed for the purpose, and the errors caused by which I have not yet had time to investigate. This view is confirmed by tests showing the effect of alteration of frequency. With the same resistance, of 500 ohms, in series with the transformer secondary circuit, as used in the tests shown in Fig. 9, I found that, for non-inductive loads traversed by currents the frequency of which varied from 93 cycles to 43 cyles, the alteration of the constant was 1·4 per cent. This change is very much greater than corresponds with the phase difference of half a degree at 80 cycles caused by the copper drop. The error due to the transformer can be reduced by diminishing the number of secondary turns, by increasing the number of primary turns, or by increasing the secondary resistance. Similar tests made, with a resistance of 750 ohms in the transformer circuit, showed a change of constant of less than 1·0 per cent. for frequencies varying between 100 cycles and 45 cycles. A set of tests on the effect of power-factor made with this resistance in series with the moving coil, and at a frequency of 80 cycles, showed no measurable alteration of constant for power-factors ranging between 1·0 and 0·1. Of course there must have been some error, but in such tests as I have as yet had time to make I was unable to detect it, and there seems no doubt that the error was much less than in the previous tests.

Now, in my opinion, no wattmeter should be considered a satisfactory instrument if it has errors like some of those mentioned, though it will hardly be denied that many commercial instruments present greater errors in practice. I am not concerned to-night with any claim that the instrument shown you is accurate under all conditions. Still less am I concerned to show that the use of transformers with wattmeters must necessarily introduce grave errors. On the contrary, my belief is that, however faulty some wattmeters may be when used with transformers, a little attention given to the design of the latter will reduce the resulting errors to a negligible amount. This will, I believe, also prove true for transformers of the particular type needed for the instrument I have described. The chief importance attaching to the tests referred to is due to the light they throw upon the question of the possibility of constructing good wattmeters with iron-cored electromagnets.

The greatest errors arising in practice in wattmeter readings are neither due to transformers, nor to intrinsic faults in the instruments. They arise from the presence of heavy currents in neighbouring conductors. Some wattmeters are protected from such influences by surrounding the coils with a shield of suitably laminated iron. Iron is used for the transformers, and is used for these surrounding shields. Iron is used anywhere and everywhere, except in the place in which it is most efficient, namely, within the coils of the wattmeter itself. In the instrument I have shown you iron has been put in the wattmeter, but has not been placed in the transformer coils. The shield may still be needed to protect these coils, but not for the instrument itself, since this will be even less affected by external fields than ordinary permanent magnet voltmeters and ammeters.

I foresee no difficulty in rendering negligible the error due to the

transformer, and the instrument itself can be much improved upon. The length of the air-gap can be reduced, and other arrangements made to diminish further the copper drop as compared with the voltage applied to the magnetising coil. There is a discrepancy between the flux density in the air-gap as calculated (1) from the number of turns, the applied voltage, the frequency, and the section of the circuit, and (2) from the number of ampere-turns on the coil, the length of the air-gap, and the other particulars of the magnetic circuit. This is possibly caused by undue eddy currents, which reduce the effectiveness of the ampere-turns and increase the copper drop. If so, the use of thinner stampings will improve matters. But as the instrument stands it is as good a wattmeter as many using current and voltage transformers, while it possesses greater range and is more sensitive. My main object is to show that the errors arising from using iron magnetic circuits in wattmeters are not necessarily greater than those involved in the use of transformers with such instruments. I consider that the action of the instrument shown justifies this view.

If it be once granted that the errors can be rendered sufficiently small, no one will doubt the great advantage of an iron cored instrument, or dispute its supremacy in regard to sensitive qualities. It must necessarily be as delicate as a permanent magnet moving coil instrument, since the magnetic field can easily be made as strong or even stronger. An instrument such as illustrated in Figs. 7 and 8 can be utilised for the detection of very small alternating currents or voltages. Of course phase difficulties arise, but these can be overcome by making use of phase changing devices for applying a voltage of the right phase to the magnetising coil.

The influence of small differences of phase on the action of alternate-current instruments can generally be calculated if the phase differences are known, but these are difficult to determine experimentally. In order to investigate the action of transformers for instrument purposes, I have devised voltmeter methods for measuring small phase differences. It would take too long to explain these now, but I have set forth in Table II. the results of a number of recent measurements.* In carrying out these tests I have had the benefit of the able assistance of my colleague, Mr. David Owen. The phase differences measured are given in the column headed θ. The last column shows for the smaller values of θ the number of parts in *one million* by which $\cos \theta$ differs from unity. The first number given relates to a common water resistance, and implies that its power-factor may be taken as 0·999978. This should bring comfort to any one who has doubted the non-inductiveness of such resistances. The values of θ given for the wire spiral and for the direct-current arc are the minimum values I could detect with certainty with the voltmeters I had at disposal when the tests were made; consequently all that these numbers prove is that the power-factor in these cases does not differ from unity by more than corresponds with the figures given. Then follow a number of tests on voltage transformers of various

* For an explanation of the methods used, see "The Measurement of Small Differences of Phase," *Phil. Mag.*, 1905, p. 155.

TABLE II.

Apparatus Tested.	Load. Amps.	Load. P.F.	Phase Tested of	θ (degrees).	$10^6 \times (1 - \cos\theta)$.
Lead plates in weak acid ...	8	...	V, A	0·38	22
Platinoid wire spiral ...	7	...	,,	0·27	11
Direct-current arc	8	...	,,	0·17	4·5
0·1 unit Tr. at full voltage ...	0	...	V_1, V_2	0·34	17
1·0 unit Tr. for $50V_1$ to $50V_2$	0	...	,,	0·021	·068
,, ,, ,,	21	1·0	,,	0·34	17
(All tests at 80 cycles and 70 volts)... ...	7	0·09	,,	0·115	2
,, ,, ,,	23	0·09	,,	0·39	23
2·0 unit Tr. fully loaded ...	10	1·0	,,	1·04	165
3·0 unit Tr. for $100V_1$ to $1,000V_2$	0	1·0	,,	0·054	0·4
Two identical Tr. used to	10	1·0	,,	1·85	...
step up volts from V_1 to	20	,,	,,	3·4	...
V_2 and down from V_2 to	30	,,	,,	4·7	...
V_3; the value of θ found	40	,,	,,	5·3	...
between V_1 and V_3 being	50	,,	,,	5·8	...
halved. Normal full load	24·8	0·09	,,	1·08	177
30 A	25·4	,,	,,	0·18	5
,, ,, ,,	27·0	,,	,,	1·00	154
,, ,, ,,	40	low	,,	4·3	...
,, ,, ,,	50	,,	,,	5·2	...
10 A current Tr. for oscillograph	10	...	A_1, A_2	0·088	1·2
,, ,, ,,	7	...	,,	0·144	3·1
5 A current Tr. as supplied commercially for wattmeter	5·7	...	,,	2·32	...

capacities, with the secondaries, in some cases on open circuit, and in others supplying currents of various magnitudes and power-factors. The last three tests were on current transformers with secondaries closed on the circuits for which they were designed. It will be noticed how exceedingly small the value of θ is for some of the transformers on open circuit. This is of special interest in regard to electromagnets for wattmeters, since such electromagnets are essentially transformers on open circuit, and the phase difference measured is that caused by the copper drop in the magnetising coil. For the one unit, equal ratio, transformer this value of θ is only 0·02 degree, and cos θ differs from unity by less than 7 parts in *one hundred million*. On full load θ is only one-third of a degree, and is much the same for a load of power-factor 0·09 as for unity power-factor. The 3-unit transformer shows a lower phase difference for inductive loads than for non-inductive, and it is noteworthy that a very sharply defined minimum value of θ of 0·18 degree occurs at about 80 per cent. of the full load current, with a secondary circuit of power-factor 0·09. The small 10-ampere transformer was supplied by the British Thomson Houston Company, to reduce current in

the ratio of 20 to 1, for any secondary resistance not absorbing more than 2 volts, or 0·5 ampere. The phase difference between the primary and secondary currents at full load is less than one-tenth of a degree. The 5-ampere current transformer was supplied by a firm of the highest standing for use with one of their own wattmeters. The large phase difference found of 2·3 degrees would seem to imply great error in use, yet on fully considering all the conditions of the instrument I find that the error in reading works out to be only 1·7 millimetres on the scale, even when the full currents (*i.e.*, such as to produce full deflection on a non-inductive circuit) are flowing in the coils, and the power-factor of the load tested is the lowest for which the instrument is guaranteed, viz., 0·6. For smaller load currents this error is proportionately diminished, and it disappears altogether on high power-factor circuits.

My object in bringing these numbers before you is to afford evidence for my belief, that while transformers as supplied commercially for wattmeters may in many cases be such as to cause phase differences of more than one or two degrees, it nevertheless seems true that a little care spent in designing them would result in reducing these angles in practice to about a tenth of a degree. Even as matters stand, the errors caused by them are less than is generally supposed. They are, however, greater than those which need result from the introduction of iron cored electromagnets into wattmeters and other alternating-current instruments. Under these circumstances my contention is that such instruments should be designed on the lines of the permanent magnet moving coil instrument, and that well-considered efforts in this direction will prove successful.

The construction of sensitive alternate-current instruments appears no small object if we consider how much electrical advance, which has all been based on exact measurement, really owes to the introduction of sensitive galvanometers and voltmeters for direct currents; and that, in all probability, similar instruments for alternating currents will lead to important developments also. At all events, when we think of electrical progress in the past, it is only fitting for us to remember how greatly this has been dependent upon the work of one man. When most of us commenced to study electricity, the name of Sir William Thomson was already historic. When *he* first started the subject, the instruments available for testing were of a kind such as are only now used for class teaching of the most elementary description. But at a time before most men now living were born, and at an age younger than most of us now present, Sir W. Thomson applied for the first time to the structure of instruments skilful engineering design. He altogether revolutionised them, and brought them to such a state of perfection that, except in comparatively small details, they have not been improved upon since. The Thomson reflecting galvanometer, the syphon recorder, and the quadrant electrometer, represent the best instruments of to-day, as they did when first introduced as long ago as the middle of the last century. The sudden increase of sensitiveness then secured constituted a step in advance quite unexampled

either before or since. It is as difficult now to properly realise its magnitude, as to truly estimate its influence on electrical development. But we are all glad that Lord Kelvin has been able, not only to take a great leading part in, but also to watch the *whole* of, this progress, and that we can still congratulate him on possessing the enthusiasm and mental activity of younger men.

APPENDIX.

NOTE I.—THE ACTION OF A ROTATING MAGNETIC FIELD ON A COIL CONVEYING AN ALTERNATING CURRENT, AND FREE TO ROTATE ABOUT THE SAME AXIS AS THE FIELD.

Assume the axis of rotation perpendicular to the plane of the paper and represented by O in Fig. 10. Let the rotating field be of constant strength F, and of uniform angular velocity p. Let its position be $O\,F_0$ initially, and be O F at time t, where $\theta = pt$ is the angle $F\,O\,F_0$. Let the plane of the coil be represented by O C inclined at an angle D to $O\,F_0$. Suppose the current in the coil follows the sine law, and that its value at any moment is

$$A_0 \sin (pt - \phi),$$

where ϕ is the phase angle to be measured, then the torque tending to move the coil is at every instant proportional to

$$A_0 F \sin (pt - \phi) \cos (pt - D).$$

The mean value of this will be zero when

$$D = \phi,$$

or the angular deflection D of a pointer attached to the coil will directly measure the phase angle ϕ whatever the frequency of the current. If ϕ is greater than D the coil will move so as to increase D, and *vice versâ*, the turning moment reversing sign with $D - \phi$. It will be readily seen that the turning moment is greatest when D and ϕ differ by 90 degrees, and that there is an unstable position of equilibrium when these quantities differ by 180 degrees.

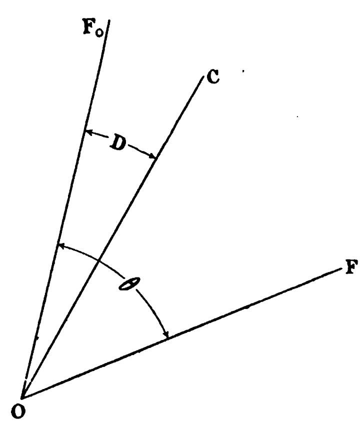


FIG. 10.

The rotating field will tend to induce a current in the coil as in an induction motor, and there will in consequence be a tendency for it to rotate with the field. This can be made quite negligible by the use of resistances in series with the coil. The more inductive these resistances are the less will be the effect. If the current in the coil is produced, as it would be in practice, by a current transformer, a tendency of this kind would

have no effect on the current through the coil, but only on the voltage absorbed by the primary.

If the current does not follow the sine law there will be no direct proportionality between the deflection D and the effective angle of lag of the current. If, however, the rotating field be uniform in strength and angular velocity, D will depend upon, and be a measure of, the time elapsing between the instant at which the field is directed along $O F_0$, and the instant at which the current passes through its zero value, but D must be measured from some zero position not necessarily coinciding with $O F_0$. The calibration of the instrument as a power-factor indicator will to a slight extent be dependent on the wave-form of the current.

For if F is constant in magnitude and angular velocity, while the current A through the coil has some special wave-form, the torque on the coil is proportional to

$$A \cos (pt - D),$$

and its average value will vanish for a particular relative position of the waves representing the current A and the sine function $\cos (pt - D)$, supposing these waves to be plotted to a time base. If the wave-form of A remains constant, but its phase (as represented by the instant of reversal) varies, the displacement of the wave A will be proportional to the time representing the phase, while the displacement of the wave $\cos (pt - D)$ will be proportional to D; hence for different phases of A the equilibrium value of D will be a direct measure of this time or phase.

If F is neither uniform in angular velocity nor constant in magnitude, the effect becomes more complicated. The torque is then measured by the average value of

$$A F \cos (\theta - D),$$

$$\text{where} \quad F = F_0 + f$$

$$\text{and} \quad \theta = pt + \delta.$$

F_0 is the average strength of the rotating field, and p the average value of its angular velocity. A, f, and δ are alternating periodic functions of the time. D is then no longer proportional to the time representing the phase of A, but is still solely determined by this time for given wave-forms of A, f, and δ.

NOTE II.—WATTMETER ERRORS.

If owing to self-induction in the pressure coil the current in it lags by an angle θ behind the voltage causing it, the wattmeter will read

$$V A \cos \theta \cos (\phi - \theta), \text{ instead of } V A \cos \phi,$$

where $\cos \phi$ is the power-factor of the load, and ϕ is positive for lagging currents, and negative for leading ones. The factor $\cos \theta$ can be considered unity for the small angles of θ at all likely to occur. Even if θ is one degree, $\cos \theta = 0{\cdot}99985$. This factor is, moreover, independent

of the phase of the load, and is therefore allowed for in the calibration of the instrument. Its variation with frequency is utterly negligible.

We can regard the small angle θ in the factor $\cos(\phi - \theta)$ as a small decrement of ϕ, so that if the true watts W are given by

$$W = V A \cos \phi,$$

we have for the error δW,

$$\delta W = -\theta \frac{dW}{d\phi} = \theta V A \sin \phi,$$

or

$$\frac{\delta W}{W} = \theta \tan \phi,$$

and

$$\frac{\delta W}{V A} = \theta \sin \phi.$$

In these equations, of course, θ must be in radians ($1° = 0{\cdot}01745$), so that $\theta \tan \phi$ represents the error in reading as compared with the true power in watts, while $\theta \sin \phi$ is the ratio the error in reading bears to the volt-amperes. These errors are worked out in Table I. for an angle of 1°. For small angles the errors are proportional to θ. Whether these errors produce an appreciable effect on the instrument reading depends very much on the meaning in watts of the full deflection of the instrument. If this denotes V A numerically, the error as a reading will not be easily detected for low power-factors, but if it denotes V A cos ϕ, or W, the error may be large.

The error increases with the phase-difference ϕ between volts and amperes. In the measurement of power on three-phase mains by the two-wattmeter method there is always a phase difference of 30 degrees or more between the currents in the two coils of one of the instruments, and it seems natural to expect appreciable errors in the measurement when the mains carry lagging currents. But on examination it will be found that the error is not greater than would be the case on single-phase mains carrying currents of the same power-factor.

For if the instruments are alike, the effect of a small lag θ in the pressure coils is the same as slightly diminishing ϕ. The two watt-meters read respectively—

$$W_1 = A V \cos(30 + \overline{\phi - \theta})$$

and

$$W_2 = A V \cos(30 - \overline{\phi - \theta}),$$

where V is the voltage between the mains and A is the line current assuming balanced loads.

So that the watts read are given by

$$W_1 + W_2 = 2 A V \cos 30 \cos(\phi - \theta),$$

and the true watts W are obtained from this by putting $\theta = 0$. Hence as before, regarding θ as a small decrement of ϕ, we have, if δW is the error in the measurement,

$$\frac{\delta W}{W} = \theta \tan \phi.$$

Note III.—On Wattmeters having Iron-cored Electromagnets.

Suppose an instrument such as is illustrated in Figs. 6 to 8 have its electromagnet excited by a coil of n_1 turns and resistance r by means of a current A_m produced by a voltage V_1. Let N be the total flux through the coil, the induction density in every part of the air-gap being assumed proportional to N. We have

$$V_1 = r A_m + n_1 \frac{dN}{dt} \quad \ldots\ldots\ldots \quad (1)$$

Let the load-current A be passed through the primary of an air-core transformer, the secondary of which is connected, through a considerable non-inductive resistance R, with the moving coil of the instrument. Assume the current c produced in the secondary winding is insufficient to alter the magnetic effect of A. We then have

$$c = -\frac{k}{R}\frac{dA}{dt} \quad \ldots\ldots\ldots\ldots \quad (2)$$

where k is some constant.

The torque exerted on the moving coil for any given deflection will be proportional to

$$\int_0^T c\,N\,dt,$$

where T is the periodic time. This quantity is proportional to

$$\int_0^T N\,\frac{dA}{dt}\,dt.$$

But since N and A are periodic functions having the same period T, this integral is easily shown to be the same numerically as

$$\int_0^T A\,\frac{dN}{dt}\,dt,$$

whence by (1) it is proportional to

$$\int_0^T A\,(V_1 - r A_m)\,dt.$$

Assuming that the vector representing the copper drop $r A_m$ is negligible in comparison with that representing V_1, it will be seen that the deflection of the instrument is a measure of

$$\int_0^T A\,V_1\,dt,$$

or of the watts, if V_1 is the voltage across the mains. This result will be true whatever the wave-form or frequency, whatever the nature of the iron forming the core of the electromagnet, and whatever the phase difference between V_1 and A, provided the copper drop is negligible, and the transformer current c varies as the rate of change of A.

Suppose the self-induced magnetic voltage is denoted by V_m, where

$$V_m = n_1 \frac{d\,N}{dt}, \quad \text{and} \quad V_1 = r\,A_m + V_m,$$

the deflection of the instrument measures the mean value of the product V_m A in all cases.

Suppose the coil of the electromagnet, instead of being shunted directly to the mains, forms the load of the secondary of an instrument transformer, the primary of which is shunted across the mains at voltage V.

The average product of V A then represents the watts to be measured, while the reading of the instrument is a measure of the mean value of V_m A.

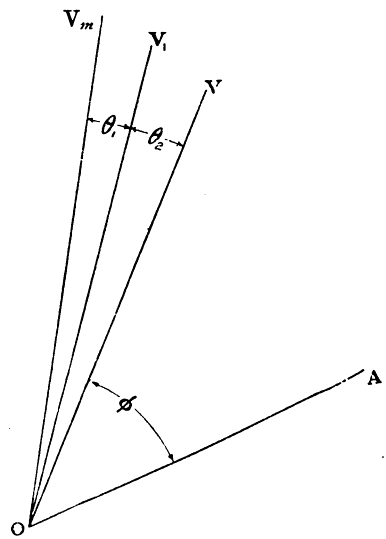


FIG. 11.

If Fig. 11 represents the vector diagram of the voltages and currents considered, and if θ_1 and θ_2 represent respectively the phase differences between V_1 and V, and between V_1 and V_m, the error in the wattmeter mainly depends upon the values of these phase differences θ_1 and θ_2. For the ratio of the lengths of the vectors V_m and V is independent of ϕ, the phase of the load-current A in reference to V, and varies only inappreciably with frequency, or for different values of V. If θ is the difference between the angle separating V from A, and the angle separating V_m from A, the error of the wattmeter follows the same law as that given in Note II., and we have for small values of θ,

$$\frac{\delta\,W}{W} = \theta \tan \phi.$$

The value of θ must be well within 1° if the wattmeter is to work satisfactorily on circuits of different power-factor. θ is the resultant phase error due to (1) the phase difference θ_1 between V_1 and V_m, (2) the phase difference θ_2 between the primary and secondary voltage of a pressure transformer if used with the magnetising coil, and (3)

any difference θ_3 between a right angle and the actual phase difference of the currents c and A.

The error is such that with lagging currents the deflection caused by a given number of watts is smaller than it should be. The error is thus in the opposite direction to that which an ordinary dynamometer type wattmeter shows. The complete vector figure is indicated in Fig. 12, in which arrow heads are used to show the sense in which the vectors are drawn. The magnetic voltage V_m, being proportional to the rate of change of N, must be drawn as a vector leading by 90

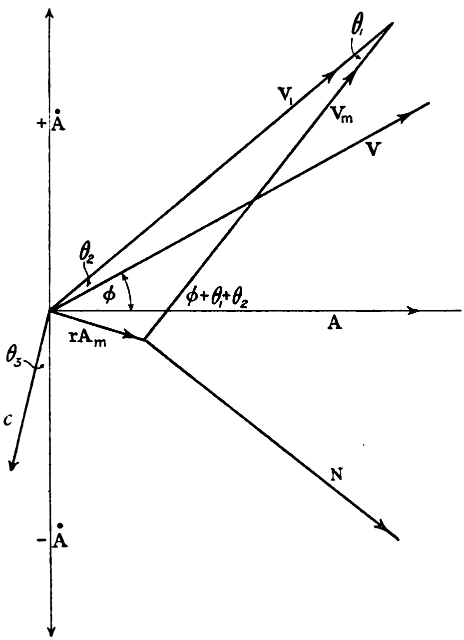


FIG. 12.

degrees in respect to the vector N. Similarly, the vector representing $\frac{dA}{dt}$ or $\dot{A}$ must lead the vector A by 90 degrees, while the vector $-\dot{A}$, to which by equation (2) c is proportional, must lag behind A by 90 degrees. If there is self-induction in the moving coil circuit, c will lag behind this last vector by a small angle θ_3, as shown. Consideration of the figure will show that the angle between the vectors representing the field N and the current c in the moving coil is

$$\phi + \theta_1 + \theta_2 + \theta_3,$$

or the error is such as to increase ϕ, where ϕ is an angle of lag.

It is to be noted that the inductive effect of the alternating field does not tend to turn the moving coil. The field flux N_1, enclosed by

the latter, varies with the position of the coil, but, for a given position, is always proportional to the total flux N produced by the alternating electromagnet. The induced electromotive force is proportional to the rate of change of N, so that with sufficient non-inductive resistance in series with the coil the current produced by this electromotive force is proportional to, and in phase with, the rate of change of N. The field in which the coil is placed is measured by N, so that the torque on the coil due to this induced current is proportional to

$$N \frac{dN}{dt},$$

the average value of which is zero for all periodic functions.

To ensure that the moving coil circuit is sufficiently non-inductive, it is necessary to use a resistance large enough to prevent a deflection being caused when the magnet is excited, with the moving coil circuit closed, but with no load current passing through the primary of the transformer.

REPORT TO COUNCIL ON THE INTERNATIONAL ELECTRICAL CONGRESS AT ST. LOUIS.

By W. DUDDELL, Member;

Hon. Secretary to the Delegation to the Congress.

(*To be discussed during April*, 1905.)

At the annual convention of the American Institute of Electrical Engineers, 1903, two papers were read on subjects which it was suggested should occupy the attention of the International Congress at St. Louis.

The first paper, by Professor Carhart, on "The Legalised Standard of Electromotive Force," after pointing out that the present legalised value of the E.M.F. of the Clark cell is probably 0·001 volt too high, proposed that the saturated Cadmium cell should be substituted for the Clark cell, and that steps should be taken to secure the legal adoption by the United States Congress of the Cadmium cell, provided it be dedicated to the public by the inventor.

The second paper, by Dr. Kennelly, on "Magnetic Units and other subjects that might occupy attention at the next International Electrical Congress," proposed that the Congress should complete the units of the magnetic circuit by bestowing names on the C.G.S. units of magneto-motive force and reluctance; and he proposed that the names, already provisionally recommended by the American Institute of Electrical Engineers, should be adopted. These names are for—

The C.G.S. unit of magneto-motive force, "Gilbert."
The C.G.S. unit of magnetic reluctance, "Oersted."

It is to be remembered that the Paris Electrical Congress, 1900, has already named the C.G.S. unit of magnetic flux (one line) a "Maxwell," and the unit of induction (one line per cm^2) a "Gauss," and these names were accepted by the "Committee for the Construction of Practical Standards for use in Electrical Measurements" of the British Association in the same year.

We have between the above quantities the following relations:—

$$\frac{\text{Gilberts}}{\text{Oersteds}} = \text{Maxwells.}$$

$$\frac{\text{Maxwells}}{\text{sq. centimetres}} = \text{Gausses.}$$

The second proposition of Dr. Kennelly was that names should be given to each of the C.G.S. units in both the magnetic and the static

systems, and he suggested that the prefix *ab* or *abs* should be used with the names of the practical units (Volt, Ampere, Ohm, etc.), to form names for the corresponding C.G.S. electro*magnetic* units, and the prefix *abstat* to form names for the C.G.S. electro*static* units: thus the C.G.S. unit of resistance, magnetic system, would be one absohm; the C.G.S. unit of current, static system, would be one abstatampere.

Dr. Kennelly's third proposition was that the Heffner-Alteneck Reichsanstalt standard amyl-acetate lamp be sanctioned as a secondary standard of light.

His fourth suggestion was that it is desirable that steps should be taken by an International Electrical Congress to establish a uniform International basis for the standardisation of electrodynamic machinery.

Copies of these papers were sent by the American Institute to this Institution, and were referred by Council to two committees for consideration and report.

The Committee on the Standard Cell, composed of Prof. Ayrton (Chairman), Dr. Glazebrook, Mr. R. Kaye Gray (President), Dr. Muirhead, Mr. J. Swinburne, Mr. H. Taylor, and Mr. A. P. Trotter, resolved: "We are not prepared at present to displace the Clark cell; but will wait for the National Physical Laboratory experiments before recommending a determination of the value of the Cadmium cell."

The Symbols Committee, composed of Prof. Ayrton, Mr. W. Duddell, Dr. Fleming, Dr. Glazebrook, Mr. R. Kaye Gray (President), Mr. H. E. Harrison, Mr. W. H. Patchell, Mr. J. Swinburne (Chairman), and Dr. Thompson, reported as follows:—

"With regard to the choice of Magnetic Units, the Committee is of opinion that the only two systems which need be considered are the C.G.S. System and the Volt-Ampere-Ohm System, and that the quantities to be named, if any, are—

1. Magnetic Potential.
2. Magnetic Flux.
3. Magnetic Reluctance.

Of these two alternatives, the Committee is in favour of the adoption of the C.G.S. System as that on which to base any nomenclature of Magnetic Units, but it is of opinion that a system of nomenclature is not called for.

"If, however, the Congress decides on naming Magnetic Units, the Committee thinks it is important that the names chosen should be simple and euphonious. The Committee disagrees with Dr. Kennelly's prefixes for the absolute electrostatic and electromagnetic systems of units, and expresses the opinion that no system of prefixes should be employed in which each prefix does not bear some definite numerical signification."

The Council appointed delegates to represent the Institution officially at the Congress. The delegates were: Mr. R. Kaye Gray (President), Col. R. E. Crompton, C.B. (Past President), Prof. J. Perry, F.R.S. (Past President), Dr. R. T. Glazebrook, F.R.S. (Member of Council), Mr. H. E. Harrison, and Mr. Duddell as Hon. Sec.

Most of the delegates travelled out to Boston with the Institution

party, and they and the other members of the Institution were most warmly and hospitably received and entertained by their American confrères the whole way along the journey.

On Monday, September 12th, the Congress opened in the Coliseum Music Hall, and unanimously adopted the following permanent organisation :--

GENERAL OFFICERS OF THE CONGRESS.

President: Prof. Elihu Thomson.

Vice-Presidents: Mr. Bion J. Arnold (Chairman of Executive Committee), Prof. H. S. Carhart, Prof. W. E. Goldsborough, Mr. C. F. Scott, Dr. S. W. Stratton.

General Secretary: Dr. A. E. Kennelly.

Treasurer: Mr. W. D. Weaver.

Honorary Vice-Presidents: Prof. Moise Ascoli, Dr. R. T. Glazebrook, F.R.S., Señor Antonio Gonzales, Col. R. E. B. Crompton, C.B., Mr. R. Kaye Gray, Prof. L. Lombardi, Prof. John Perry, F.R.S., M. Henri Poincaré.

The Congress was divided into eight sections for the reading and discussion of papers, which were attended by the members of the Congress and by the delegates from the different scientific and technical societies.

Besides these sections there was constituted, in response to invitations by the Government of the United States, a Chamber of Delegates of the Congress to which foreign governments sent official representatives as follows :—

Argentine Republic: Dr. Jorge Newbery.

Austria-Hungary: Prof. Charles Zipernowsky.

Australian Colonies: John Hesketh, Esq.

Canada: Ormond Higman, Esq.

Denmark and Sweden: Prof. Svante Arrhenius.

France: M. Poincaré, M. Guillebot de Nerville, M. Paul Janet, M. Ferrie, M. Dennery.

Germany: Kaiserlich Postrat Litzrodt.

Great Britain: Col. R. E. Crompton, Dr. R. T. Glazebrook, F.R.S., Prof. John Perry, F.R.S.

Hungary: Joseph Vater, Bela Gati.

Italy: Prof. Moise Ascoli, Prof. L. Lombardi, Ing. A. Maffezzini, Marquis Luigi Solari.

India: J. C. Shields, Esq.

Mexico: Señor Rafael P. Arizpe.

Spain: Señor Miguel Otamenti, Señor Antonio Gonzalez.

Switzerland: Prof. Ferdinand Weber.

United States: Prof. H. S. Carhart, Dr. A. E. Kennelly, Prof. H. J. Ryman, Prof. S. W. Stratton, and Prof. Elihu Thomson.

On Tuesday, September 13th, a joint discussion was held in Section A between this Institution and the American Institute of Electrical

Engineers on "Standards and Systems of Electromagnetic Units," of which a verbatim report is given at the end.

The Chamber of Government delegates considered this question of Electromagnetic Units and the further question of International Standardisation, and after discussion in the Chamber, at the meeting on September 10th, two sub-committees were appointed to deal with the questions of International Electromagnetic Units and of International Standardisation respectively.

At the meeting on September 15th, the following report of the Committee on International Electromagnetic Units was accepted and unanimously adopted :—

Committee on International Electromagnetic Units.

The sub-committee appointed September 13, 1904, beg leave to suggest that the Chamber of Delegates should adopt the following report :—

"It appears from papers laid before the International Electrical Congress, and from the discussion, that there are considerable discrepancies between the laws relating to electric units, or their interpretations, in the various countries represented, which, in the opinion of the Chamber, require consideration with a view to securing practical uniformity.

"Other questions bearing on nomenclature and the determination of units and standards have also been raised, on which, in the opinion of the Chamber, it is desirable to have international agreement.

"The Chamber of Delegates considers that these and similar questions could best be dealt with by an International Commission representing the Governments concerned. Such a Commission might in the first instance be appointed by those countries in which legislation on electric units has been adopted, and consist of (say) two members from each country.

"Provision should be made for securing the adhesion of other countries prepared to adopt the conclusions of the Commission.

"The Chamber of Delegates approves such a plan, and requests its members to bring this report before their respective Governments.

"It is hoped that if the recommendation of the Chamber of Delegates be adopted by the Governments represented, the Commission may eventually become a permanent one."

The following report was also received, and unanimously adopted, from the Committee on International Standardisation :—

Committee of the Chamber of Delegates on International Standardisation.

The Committee of the Chamber of Delegates on the Standardisation of Machinery reports as follows :—

"That steps should be taken to secure the co-operation of the technical societies of the world by the appointment of a representative commission to consider the question of the standardisation of the Nomenclature and Ratings of Electrical Apparatus and Machinery.

"If the above recommendation meets the approval of the Chamber of Delegates, it is suggested by your Committtee that much of the work could be accomplished by correspondence in the first instance, and by the appointment of a General Secretary to preserve the records and crystallise the points of disagreement, if any, which may arise between the methods in vogue in the different countries interested.

"It is hoped that if the recommendation of the Chamber of Delegates be adopted, the Commission may eventually become a permanent one."

At the meeting on September 16th, the following resolutions were unanimously adopted :—

"That the Delegates report the resolution of the Chamber as to Electrical units to their respective Governments, and that they be invited to communicate with Dr. S. W. Stratton (Bureau of Standards, Washington, D.C.) and Dr. R. T. Glazebrook (National Physical Laboratory, Bushey House, Teddington, Middlesex, England) as to the results of their report, or as to other questions arising out of the resolution."

"That the Delegates report the resolution of the Chamber as to the International Standardisation, to their respective technical societies, with the request that the societies take such action as they may deem best to give effect to the resolution, and that the Delegates be requested to communicate the result of such action to Col. R. E. B. Crompton, Chelmsford, England, and to the President of the American Institute of Electrical Engineers, New York City."

In accordance with the report of the Committee of the Chamber of Delegates on the Standardisation of Machinery, Col. Crompton communicated the desire of the Congress to the Institution of Civil Engineers, under whose direction the various sections of the Engineering Standards Committee hold their sittings. The matter was referred to the Main Committee, and by them to the Electrical Plant Committee, who will in due course report to the Institution of Civil Engineers, with the request that they confer with the Institution of Electrical Engineers.

On September 14th, a joint meeting of the American Institute and the Institution of Electrical Engineers was held at the Festival Hall in the World's Fair Grounds, Mr. R. Kaye Gray, our President, being in the chair, when Mr. B. J. Arnold, President of the A.I.E.E., gave an address dealing with the application of alternating-current motors to traction purposes, which, together with the discussion, is reprinted below *in extenso.*

The final meeting of the Congress was held in the Library Hall in the Exhibition Grounds on Saturday, 17th, when the recommendations of the Chamber of Delegates were announced and the closing speeches made. In the evening, the members of this Institution and of the American and Italian Institutions resumed their tour, homeward bound.

Mention has only been made in this report of those meetings of the Congress in which this Institution took an official part. As all the papers and discussions will be published in full by the Congress, nothing need be said of the 150 to 160 papers considered by the

Congress, some of which were contributed by members of this Institution, and most of which were actively discussed by our members.

The success of the Congress was in a large measure due to the hearty co-operation between the members of the American, Italian and British Institutions of Electrical Engineers, and to the indefatigable energy of its organisers, and especially to Prof. Elihu Thomson, the President, and Dr. Kennelly and Mr. Weaver.

In conclusion, the Institution Delegates wish to put on record their entire agreement with the resolutions, given above, of the Chamber of International Delegates. So strongly are the delegates convinced that the appointment by the different Governments of an International Commission to deal with International Electromagnetic Units and similar subjects is in the best interest of the electrical industry, that they wish to urge the Council to use all its influence and to take every necessary step to obtain the adoption of the recommendation by the Government.

The delegates also recommend that the Council should formally give its adhesion to the proposed formation of an International Commission to consider the question of the standardisation of the Nomenclature and Ratings of Electrical Apparatus and Machinery, and should take any steps it may deem necessary to give effect to the proposal, by appointing a committee to take action in the matter, or otherwise.

The following three papers on Standards and Electro-magnetic Units were read and discussed jointly with this Institution at the meetings of the International Electrical Congress at St. Louis, Sept. 12 to 17, 1904. They are reprinted here by the kind permission of the Committee of Organisation :—

ON THE SYSTEMS OF ELECTRIC UNITS.

By Professor M. Ascoli, President and Delegate of the Associazione Elettrotecnica Italiana.

1. I think that the International Electrical Congress should take cognisance of what has been done on the subject of electrical units since the last Congress. Therefore, I believe that a short report on the work done, especially in Italy, on this important question may not be without interest.

What I intend to say has nothing to do with any change which may be proposed of the value of the practical standard used at present. This paper is intended to deal with the fundamental theory of units.

2. In the equations of electromagnetism on which the definition of units is founded, we have several coefficients, to some of which we give particular values so as to establish particular systems of units. But I think it will be more useful in order to prevent any misunderstanding to keep at first all the coefficients in the formulæ, leaving to these their generality.

It is preferable, I think, in deducing the units to start from the old expressions of the laws of different kinds of mutual action between electric and magnetic quantities; that is to say, electrostatic, magnetic, electromagnetic, and electrodynamic actions.

We have thus four equations, one side of which is a force, expressing the laws of Coulomb, Laplace, and Ampere:

$$f=\frac{1}{a}\frac{e^2}{r^2} \qquad f=\frac{1}{\beta}\frac{m^2}{r^2} \qquad f=\frac{1}{\gamma}\frac{mids}{r^2}\sin\omega$$

$$f=\frac{1}{\delta}\frac{ii'\,ds\,ds'}{r^2}\left(\cos\varepsilon-\frac{3}{2}\cos\theta\cos\theta'\right) \quad . \quad . \quad . \quad (1)$$

where the symbols have a well-known signification, and a, β, γ, δ, are special coefficients.

A fifth equation is

$$i=\frac{e}{t}$$

which can be said to express the equivalence between the current defined by electromagnetic action and the current defined by convection.

Many other equations frequently used may be considered as definitions of so many other magnitudes. For example—

$$\begin{aligned}\text{Energy}&=eV \text{ defines the potential } V\\ V&=iR \text{ defines the resistance } R\\ e&=CV \text{ defines the capacity } C\\ \text{Energy}&=\tfrac{1}{2}Li^2 \text{ defines the self-induction } L. \quad . \quad . \quad (2)\end{aligned}$$

In the preceding five equations we have seven magnitudes not having a geometrical or mechanical character, or, in other words, not depending only on the three fundamental magnitudes: length, mass, and time. These seven magnitudes are e, m, i, a, β, γ, δ. We have five equations between seven quantities; two of them must, therefore, be chosen in order to have the others determined; and the choice must be an arbitrary one, until some new physical laws are discovered.

It has been, therefore, a misconception to suppose that length, mass, and time were sufficient to define the electric units. On the contrary, the fundamental magnitudes must be five in number. Three of them can be l, m, t; the other two can be chosen in any way among the seven above-mentioned quantities, or others connected with them, by some known equations. It is not at all necessary to choose these two quantities among the coefficients a, β, γ, δ.

It has been suggested that it would be possible for new laws to be discovered in the future, so that even the coefficients above-mentioned might be expressed in terms of l, m, and t. But in the present state of the science, this suggestion is entirely arbitrary. Mr. Fessenden, for instance, following Maxwell's conceptions, tried to assume that of the two coefficients a and β, the first could be a density, the second the reciprocal of a pressure; but Fessenden's arguments are at the utmost

valuable only in suggesting the hypothesis of the proportionality between the said quantities, but not the hypothesis of their equality. In this way a new constant of unknown nature is introduced.

3. It is easy to deduce from the five equations above quoted* that the four coefficients, α, β, γ, δ, are connected by the two relations in

$$\frac{\gamma^2}{\alpha\beta} = v^2 \quad \frac{\delta}{\alpha} = v^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which v represents a velocity that experiment proves to be dependent upon the nature of the matter occupying the region considered, and equal to the velocity of light in the same. Only two of the four coefficients can be chosen at will; that is to say, two quantities are sufficient to define the electromagnetic properties of the surrounding matter, of the ether, for instance.

In the electrostatic system, if we assume $\alpha = 1$, $\gamma = 1$, it follows that $\frac{1}{\beta} = v^2$, $\delta = v^2$; in the electromagnetic system, if $\beta = 1$, $\gamma = 1$, it follows that $\frac{1}{\alpha} = v^2$, $\delta = 1$. If we suppose $\alpha = 1$, $\beta = 1$, as in a Hertzian system, we have $\gamma = v$, $\delta = v^2$.

Any system, provided that it satisfies the above conditions, is a rational one. It would be, therefore, preferable to choose a different word to indicate the system which gets rid of 4π from the formula of electromagnetism, suggested by Mr. Heaviside.

To obtain the rationalisation, in the Heaviside sense, we must put $\gamma = 4\pi$ in the expression of the m.m.f. ($g = \frac{4\pi}{\gamma} i$) which follows from the third of the above equations. We have, in this case, from

$$\frac{(4\pi)^2}{\alpha\beta} = v^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If together with this rationalisation we wish also to keep unaltered the unit of electric quantity as chosen in the electromagnetic system, as was proposed by Fessenden, we must put $\frac{1}{\alpha} = v^2$, as it is in this system; it follows that $\beta = (4\pi)^2$, as the value of the permeability of the standard medium, as Mr. Fessenden stated, notwithstanding the remedy of Professor Fleming. For this example we have a proof of the utility of the above method.

4. But the so-called rationalisation, notwithstanding the proposals of Fleming and Fessenden, introduces some units that are not used in practice. So we will find it necessary to retain five different systems, unless we accept the proposal of Mr. Fessenden and assume the c.g.s. units also for practical purposes; but this change would itself be undoubtedly so objectionable, that I can hardly see how the proposal could be accepted.

* See Professor Somigliana, "Sulle unita elettriche e magnetiche." *Rendimenti* dell' Institute Lombardo. January, 1900.

Professor Giorgi, of Rome, took up the question more than three years ago in order to construct a system, rational in the Heaviside meaning, but at the same time an absolute one; that is to say, depending upon a minimum number of fundamental units and having the advantage, essential for practical use, of keeping the units of the actual practical system for the measurement of the most important magnitudes.

The proposals of Mr. Giorgi were presented and discussed before the Associazione Elettrotecnica Italiana at the general meeting of October, 1901 (Rome). After this discussion a committee was appointed (Professors Ascoli, Donati, Grassi, Lombardi, Roiti), who presented a report also on behalf of the Italian Physical Society at the general meeting of the Associazione in October, 1902 (Turin). The committee in this report expressed the opinion that the Giorgi system had the necessary characters which would entitle it to be substituted for the present systems, and recommended that the new system be brought before an international electrical congress.

In the meantime Mr. Giorgi has published some explanatory notes; the details of the system were given in the technical periodicals of Europe and America. It was also presented and discussed before the London Physical Society, and a report of this discussion can be found in the London *Electrician*, in 1902. Professor Robertson subsequently returned in the same journal to the Giorgi proposals, and several authors, who took part in the discussion which ensued, seemed to have entirely forgotten the remarkable preceding work. Mr. Emde in Germany, in an interesting paper on electric units, read before the Elektrotechnisher-Verein in Berlin some months ago (see E. T. Z.), discussed at length, and favourably, the Giorgi system.

The Giorgi system is a rational one (in the Heaviside sense); that is to say, it assumes the relation $\gamma = \pi$, and the coefficient γ is, therefore, one of the two magnitudes chosen in an arbitrary way. As regards the second outstanding magnitude, Mr. Giorgi proposed, not one of the other coefficients, but one of the practical electrical units used at present; for instance, the Chicago international ampere or ohm. In this case the ampere (or ohm) will no longer remain a unit theoretically defined, but it would become a fundamental arbitrary unit. In the same way the metre is no more considered the ten-millionth of the terrestrial quadrant, but it is the length of the platinum bar existing in Paris which very approximately corresponds to the primitive definition.

The measurements of the earth, which at first were intended for the definition of the metre, now express in metres the length of the quadrant. What would be now the result of measurements made for the determination of the absolute electric units, which required such long and hard labour? As has already been stated, among the quantities which are no longer arbitrary, we have the two coefficients, α and β; that is, the electric and magnetic constants of the standard medium, of the ether, for instance. It would perhaps be better to change, in the work of the Chicago Congress, the value of these constants instead of changing the value of the ohm previously employed.

5. It is easy now to see that, independently of the fundamental

units of length, mass, and time, the units of E.M.F., resistance, capacity, inductance—that is, the most important ones in practical use—are not at all changed if we keep the same unit of time, energy (or power) and of electrical current (or resistance). In fact, this is plainly shown by the equations (3). For this reason we are free to choose any units of length and mass, provided that the unit of energy resulting from them remains the present joule ; that is, 10,000,000 ergs of the c.g.s. system. We can, therefore, choose as unit of length 10^n cm., and as unit of mass, 10^m grams, provided that $2n+m=7$. We can take, for example, $n=2$, $m=3$, that is, take the metre and kilogram as units of length and mass. These units seem to be very appropriate because the Paris standards are precisely the metre and kilogram.

We have, in this way, the advantage of establishing a new system of rational units, keeping the practical system now used, and of reducing this system to an absolute one with very convenient fundamental units (metre, kilogram, and second).

Some may regret to give up the c.g.s. system, but it can be observed that the c.g.s. system is already partially abandoned in practice, and that it will always retain its historical value because the arbitrary unit of current (or resistance) that we would choose will have its origin in the old system.

If we suppose that the international ampere (or ohm) corresponds exactly to the theoretical definition depending upon the c.g.s. system, the electric and magnetic constant of the ether would be

$$\alpha = \frac{1}{36\pi} 10^{-9} \quad \beta = 4\pi \; 10^{-7}.$$

But in fact these constants are affected by the errors of observation made in the absolute measurements ; if the 4π enters in the expression of α and β, it occurs only from a historical reason, because the arbitrary unit is chosen very near to the value of a non-rational unit.

In conclusion, I believe that the Giorgi system must be preferred to the other rational systems which have been hitherto proposed. No difficulty of a legal kind exists against it, because none of the units accepted by the Government are charged ; on the contrary, it prevents any change in the future. I do not think that it is necessary at present to introduce officially the new system, the more so as any one can use it without trouble of any sort ; but I believe that the Congress should take cognizance of it, and put it on an official plane with other systems which have been proposed and may be proposed in the future. Especially I would like to call to it the attention of professors teaching electrical science.

APPENDIX.

PROPOSALS CONCERNING ELECTRICAL AND PHYSICAL UNITS.

By Professor G. GIORGI.

It is suggested that the existing system of practical units may be completed as follows, thus making an absolute system of practical units:

1. *Concrete Electrical and Magnetic Units.*

Besides the existing units, ohm, coulomb, volt, farad, henry, and ampere, the following ones are proposed:

For *m.m.f.*, the ampere (already practically used under the name of ampere-turn).

For *magnetic flux*, the product of one volt by one second, which may be called the *weber* (as proposed by the British Association).

For *magnetic inductance* (permeance, that is $\frac{\text{flux}}{\text{m.m.f.}}$), the henry (already existing as the unit of self-induction).

These, together with their reciprocals, make a complete and self-consistent system of electrical and magnetic concrete units. They may be combined with the following:

2. *Mechanical Units.*

For *length*, the metre.
For *mass*, the kilogram.
For *time*, the second.

Thence—

For *power*, the watt.
For *work*, the joule, etc., etc.

3. *Electrical and Magnetic Specific Units.*

No name for any specific unit is proposed. Instead of having specific units ready made, it is preferable to make them by referring the concrete units to any unit of length, area, volume, which may be preferable, according to the circumstances of a case; thus, *volt/m*, or *volt/mm*, or *volt/inch* as it may be desirable.

When the metre, kilogram, and second are taken as fundamental, specific units of the absolute practical system result as follow:

Amp./m for magnetic force (magnetic field intensity, gradient of magnetic potential).

Volt/m for electric force (electric field intensity, gradient of electric potential).

Weber/m^2 for magnetic induction (magnetic displacement, magnetic influx per unit area).

Coulomb/m^2 for electric induction (electric displacement, electric flux per unit area).

Henry/m for magnetic inductivity (permeability, magnetic constant of a medium).

Remark.—The magnetic constant of free ether becomes $\mu_0 =$ 0·000,001,256 henry/m.

Farad/m for electric inductivity (dialectric power, electrostatic constant, ratio of electric displacement to electric force).

Remark.—The electric constant of free ether becomes $k_0 =$ 0·000,000,000,008,842 farad/m.

Results.

In this manner we obtain an *absolute system of practical units*, which is independent of both the c.g.s. electrostatic system and the c.g.s. electromagnetic system, and does not interfere with either.

As fundamental units of this system there may be taken, the *metre*, the *kilogram*, the *second*, and the *ohm* (the latter to be defined by the practical standard adopted by the Congress of 1903, or by the standard kept at the Board of Trade in London).

This system is "rationalised" (in Mr. O. Heaviside's signification); that is, is free from any unnecessary 4π. In this system, electric current is identified with m.m.f.

This system is neither electrostatic nor electromagnetic, because neither the electric nor the magnetic constant of free ether is assumed as a fundamental unit.

This system is completely dualistic, all units having a magnetic and an electric signification at the same time, which halves the number of units needed; all electric and magnetic formulæ are identical.

All units, fundamental and derived, are of convenient size.

The system may be called the *absolute practical system.* Its units may be called *absolute practical units.*

Concerning Practical Use.

The system consists entirely of units already in practical use.

Practicians are not required to make any change, nor to learn anything new. They are simply to be instructed that their present units may also be used as absolute ones, thereby making the c.g.s. systems unnecessary in their calculations.

The necessity of making conversion of units is thus avoided (see Note C).

Concerning Scientific Use.

Neither the c.g.s. electrostatic nor the c.g.s. electromagnetic system is touched. Scientists will be free to use any one of these systems, without modification, or to substitute for them the absolute practical system, with the advantage of simplified and rationalised formulæ; agreement with practical use; units of convenient size; dimensions simple, without fractional exponents; fundamental units independent of absolute measurement; no distinction to be made between electrostatic and electromagnetic calculations,

Theoretical Grounds.

The theoretical grounds on which the absolute practical system is founded are fully set forth and discussed in the papers mentioned in Note A.

The point of fundamental importance to be kept in view is the following:

In order to derive electric and magnetic units from mechanical units, a fourth fundamental or independent unit is necessary. In the c.g.s. electrostatic and in the c.g.s. electromagnetic systems, the fourth unit assumed is respectively the electrostatic or the magnetic constant of free ether; but this has many disadvantages. In the absolute practical system the fourth unit is the *ohm.*

Of course, when any electric or magnetic unit is arbitrarily chosen, all others are deduced from it.

Note. A.

History.

1. G. Giorgi.—"Unita Razionali di Elettromagnetismo," read before the general meeting of the Italian Association of Electrical Engineers, October, 1901, in Rome. See *Atti* dell' Associaz. Elettr. Italiana, 1901, p. 402; *L'Elettricista,* 1901, December; *L'Elettricita,* 1901; *L'Industria,* 1901 (+); *Il Nuovo Cimento,* 1902. See also abstracts in *Science Abstracts,* in *L'Eclairage Electrique,* etc.

2. Discussion of Same.—See report of said meeting, *Atti* dell' A. E. T., 1901.

3. G. Giorgi.—"Rational units of electromagnetism," read before the Physical Society of London, on May 27, 1902.

4. Discussion of Same.

5. Prof. Donati.—Report on G. Giorgi's proposals. See *Nuovo Cimento,* 1902.

6. Prof. Ascoli.—Sul Sistema di Unita Proposto dall' Ing. Giorgi; read before the Congress of the Societá Italiana di Fisica, held at Brescia in September, 1902.

7. Discussion of Same.—See *Nuovo Cimento,* 1902.

8. G. Giorgi.—"Il Sistema Assoluto M. Kg. S." Read before the A. E. T., May 2, 1902. See *Atti* dell' A. E. T., October, 1902; *L'Elettricista,* 1902, etc.

9. Report of Committee, appointed by the Associazione Elettrotecnica Italiana, and by the Societá Italiana di Fisica, consisting of Prof. Grassi, Prof. Ascoli, Prof. Roiti, Prof. Lombardi, Prof. Donati; read by Prof. M. Ascoli at the general meeting of the Italian Electrical Association, held in Turin, November, 1902; also discussion of the same, See report of the meeting in *Atti* dell' A. E. T., 1902.

10. G. Giorgi.—"I Fondamenti della Teoria delle Grandezze Elettriche," read before the said Congress. See *Atti* dell' A. E. T., 1903. See also abstracts in *Science Abstracts* and elsewhere.

11. G. Giorgi.—"Le Formole Teoriche di Elettricita," read before the A. E. T., Dec. 15, 1902. See *Atti* dell' A. E. T., 1903.

12. G. Giorgi.—Notazioni e simboli Elettrici. See *Atti* dell' A. E. T., 1903.

Note B.

List of Units of the Absolute Practical System.

1. *Mechanical.*

Magnitudes.	Absolute practical units.
Length	m
Area	m^2
Volume	m^3
Time	sec
Frequency	$\sec^{-1}$
Velocity	m/sec
Acceleration	$m/\sec^2$
Mass	kg
Density	kg/m^3
Force...	(...... no name exists)
Torque	joule
Energy	joule
Power...	watt

2. *Electrical.*

Magnitudes.	Absolute practical units.
Quantity of electricity...	coulomb
Electric displacement	coulomb/m^2
Electric current...	amp.
E.M.F.	volt
El. force	volt/m
El. conductance	mho
El. conductivity...	mho/m
El. resistance	ohm
Capacity	farad
El. inductivity (= specific capacity, or electric constant of a medium)	farad/m
Coefficient of self-induction	henry

3. *Magnetic.*

Magnitudes.	Absolute practical units.
Quantity of magnetism (flux)	weber
Magnetic induction	weber/m^2
Magnetic current $\left(=\frac{d\phi}{dt}\right)$	volt
M.M.F.	ampere
Magnetic force...	amp./m
Magnetic inductance $\left(= \text{permeance} = \frac{flux}{M.M.F.}\right)$...	henry
Magnetic inductivity (= magnetic constant of a medium, permeability)	henry/m
Magnetic reluctance	henry^{-1}

Note C.—Method of Application.

To Calculate the Capacity of the Earth in Farads.

(*a*). Following the methods hitherto used.	(*b*). Using the absolute practical system.
Radius of the earth $r = 6 \times 10^8$cm. Dielectric constant of free ether $k = 1$ (*electrostatic system*) Capacity of the earth, in c.g.s. electrostatic units $(K) = \frac{r}{k} = 6 \times 10^8$ Coefficient for converting electrostatic into electromagnetic value $v^2 = 9 \times 10^{20}$ Capacity of the earth, in c.g.s. electromagnetic units $[K] = \frac{(K)}{v^2} = \frac{6}{9} 10^{-12}$ Coefficient for converting c.g.s. value into practical value $e = 10^9$ Capacity of the earth in farads $K = e[K] = 67 \times 10^{-5}$ farad	Radius of the earth $r = 6 \times 10^6 m$ Dielectric constant of free ether $k = 88 \times 10^{-13}$ farad$/m$ Capacity of the earth $K = 4\pi kr = 67 \times 10^{-5}$ farad

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THE ABSOLUTE VALUE OF THE E.M.F. OF THE CLARK AND THE WESTON CELLS.

By Professors Henry S. Carhart and George W. Patterson, University of Michigan.

(*A research made under a grant from the Carnegie Institution.*)

Introduction.

The method used by us in determining E.M.F.'s relies on the measurement by an absolute electrodynamometer of a current through a known resistance. The resulting potential difference is compared by the potentiometer method with the E.M.F. of the cell under investigation. In our work we assume certain coils, marked 1 ohm, to have values given in the certificates of the Reichsanstalt which refer to them. The other experimental data are lengths, referred to a Rogers bar with Rogers certificate, masses, weighed with weights compared with weights with certificates from the United States Bureau of Weights and Measures, and time, obtained from a Rieffler clock in our laboratory, whose error is of too small an order to affect our results. The acceleration due to the gravity does not enter our problem.

The Electrodynamometer.

The electrodynamometer is a two-coil instrument, each coil of which consists of a single layer of conductors wound on a cylinder. The same arrangement was used by Patterson and Guthe,* and Carhart and Guthe.† Our present instrument is of the same general design, but has its coils wound on plaster of Paris cylinders instead of wood and vulcanite, as in our older instrument. The diameter and length of each coil are in the ratio of 2 to $\sqrt{3}$. This ratio simplifies the computation for the torque between the coils.‡ The following table shows the data for the two coils :

	Number of turns.	Mean diameter.	Mean length.	Conductor.
Fixed coil...	593	47·372 cm.	41·006 cm.	0·062 cm. diam. 0·069 cm. diam. over all.
Movable coil	36	10·044 cm.	8·698 cm.	0·0375 cm. thick. 0·128 cm. wide.

The conductor of the suspended coil is copper ribbon, whose width is intended to be equal to the width of the space between turns. The effective length of this coil could not be determined with as great accuracy as the other dimensions, as it was not practicable to keep the distance between turns absolutely uniform. This lack of uniformity is very slight, and leads to no appreciable error, as the length of the coil appears only as a correction when the ratio of length to diameter is $\sqrt{3}$ to 2.

The smaller coil is suspended by a wire whose torque balances the torque between the coils when the current to be measured is passing. It is our invariable rule to twist the wire one complete turn. Mirrors at both ends of the suspension, in conjunction with telescopes and scales at a distance of 2 metres, enable us to determine when the twist of the wire is as desired. At the distance chosen, 1 mm. on the scale, as viewed through the telescope, corresponds to 1/25,133 of a turn. An error of 1/250,000 of a turn would be easy to detect. The real difficulty in our measurements is in the wire, however, for elastic fatigue and subpermanent set of the wire have caused us much trouble, and are still interfering with our obtaining satisfactory results. The suspending wire is permanently soldered into a small brass rod at one end and a larger brass cylinder at the other end. The brass rod may be coupled to the suspended coil, in which case the larger brass cylinder is held in the torsion head. To obtain the tortional constant of the wire, we turn the wire end for end, load the wire with a total mass equal to that of the suspended coil, by adding a hollow brass cylinder

* Patterson and Guthe, *Proc.* A. A. A. S., 1898, p. 154, and *Phys. Rev.* December, 1898, vol. vii., p. 257.

† Carhart and Guthe, *Proc.* A. A. A. S., 1899, p. 103, and *Phys. Rev.*, November-December, 1899, vol. ix., p. 288.

‡ A. Gray, "Theory and Pract. Absol. Meas. in Elect. and Mag.," vol. ii. part I, p. 275. Also Patterson, *Physical Review*, 1905.

which closely fits the cylinder soldered to the wire, and clamp the brass rod in a support. We then determine the period of torsional vibration of the system. The moment of inertia of the system used is 2251·11 gm. — cm.^{2}, made up as follows :—

Hollow cylinder	2241·65 gm. — cm.^{2}
Inner cylinder	9·39
Mirror	0·07
	2251·11

The ratio of length to diameter of the combined outer and inner cylinders is $\sqrt{3}$ to 2, an arrangement which gives the same moment of inertia about all axes passing through the centre of gravity of the cylinder. This insures freedom from error if the axis of the cylinder differs from the axis of suspension. In actual fact no appreciable difference between these axes occurs. The same cylinder was used by Patterson and Guthe, who also used a second cylinder of as nearly as practicable the same dimensions, and which gave concordant results, thus making it probable that the cylinders were free from blow-holes, which would hardly have had equal effects in both cylinders. We have used both phosphor bronze and steel wires for the suspension. The phosphor bronze wires had diameters about $\frac{1}{3}$ of a mm. (0·30, 0·33, and 0·35). The steel wire was 0·28 mm. in diameter. Our instrument is arranged to hold a suspension wire from 90 cm. to 115 cm. long. The length of wire should preferably be chosen so as to make the torque with one complete turn approximately that of the coils when the current through the instrument causes over the standard resistance a potential difference equal to the E.M.F. of the cell under test. This tends to eliminate the effect of any errors in the calibration of the potentiometer. We have concluded to lengthen the suspension wire to about 2 metres, using a wire of somewhat larger diameter. For one complete turn the torque is inversely proportional to the length and directly proportional to the fourth power of the diameter ; and consequently a twist of one turn for the longer wire will cause much reduced shear in the wire, and we believe that by this means the effects of elastic fatigue and subpermanent set will be materially reduced. The effect of the set of the wire is to reduce the effective twist. As the square of the current is proportional to the twist of the wire necessary to hold the coil in its initial position, we see that an error in the effective twist of 1 per cent. corresponds to an error in the current of $\frac{1}{2}$ per cent.; or, with the telescope and scale at 2 m. distance, 1 cm. error is equivalent to an error in the current of about 1 in 5,000 (more exactly, 1 in 5,026·6). The usual effect of the elastic fatigue has been to make the zero change by about 3 to 4 cm. With repeated twists in the same direction the uncertainty reduces to about 1 cm. ; but the question arises, "Is the rigidity of the wire the same as when undergoing torsional vibrations ?" We hope soon to reduce the elastic fatigue to such a degree that we may feel safe in assuming the rigidity to be the same under both conditions.

We chose plaster of Paris cylinders to hold the coils after experimenting with wood, vulcanite, and porcelain. Dr. Guthe was still with us when we chose plaster of Paris for the support of the fixed coil, and he and one of us made a series of tests as to the magnetic neutrality of the plaster of Paris. It appeared to be almost perfectly inert. Since making the suspended cylinder of plaster of Paris, two other tests have been made. In one the cylinders were placed with axes at 45 deg. and the full current sent through the fixed coil. The movable cylinder did not turn appreciably, and it would have been easy to detect 1/250,000 of a turn. Later the periods of torsional vibration of the suspended coil were determined with and without the full current in the fixed coil. The results reduced to the same temperature are 35·606 ± 0·001 sec. for the former and 35·607 ± 0·001 sec. for the latter. These results agree within the errors of observation, and we conclude that plaster of Paris has unit permeability.

For one complete turn of a wire on which a mass of moment of inertia K executes torsional vibrations with a complete period T the torque is

$$T_1 = \frac{8\ \pi^3\ K}{T^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The action of a current I (c.g.s. units) through two cylindrical coils for which L, D, N, and l, d, n are length, diameter, and number of turns of conductor for each coil respectively, and where $L : D :: l : d :: \sqrt{3} : 2$, produces a torque,

$$T^2 = \frac{\pi^2\ d^2\ N\ n\ I^2}{\sqrt{L^2 + D^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equating these torques and solving for I we obtain

$$I = \frac{1}{T\,d}\sqrt{\frac{8\ \pi\ K}{N\,n}\ \sqrt{L^2 + D^2}} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In deriving* equation (2), it has been assumed that the coils are equivalent to current sheets, and it is well to inquire whether this assumption may be allowed. The fixed cylinder is wound with wire about 0·069 cm. diameter, including a silk insulation, the bare wire being 0·062 cm. in diameter. The suspended cylinder is wound with a ribbon 0·0375 cm. thick and 0·128 cm. wide, and the spaces between turns are approximately the same width as the ribbon. It follows that one-half of the winding has spaces corresponding to the ribbon on the other half, so that the average effect is that of a current sheet. We have assumed that it is proper to take as the effective diameter the arithmetical mean between the outer and the inner diameter; for,

* For the derivation of this expression see Patterson, *Physical Review*, 1905.

although the torque depends on the square of the radius for turns at various distances, we must recognise that the layer of the ribbon next to the cylinder is relatively shortened, and that this produces a tendency to larger current density near the surface of the cylinder. It appears probable that one item offsets the other, and that the mean radius is fairly taken as the arithmetical mean. The lead wires from the suspended coil to the mercury cups are in the plane normal to the axis of the fixed coil, and in the vertical plane through the axis of the suspended coil. It follows that they can exert no torque on the fixed coil. One mercury cup is over the other, and both are in the line of the suspending wire. The lead wires to the mercury cups from outside are twisted together except for the short space near the cups, where of necessity they are separated. Want of symmetry here may be eliminated by reversing the connection between the coils, and in our work it is always so eliminated. The winding of the fixed coil is wire of so small radius that the ripples in the magnetic field cannot be appreciable. The arithmetical mean between the outer and inner radii of the coil is taken for reasons similar to those mentioned in connection with the suspended coil. The lead wires to the fixed coil go to the ends of an element of the cylinder level with the axis, and are twisted together except for a piece parallel and near to this element. This piece can produce no torque about a vertical axis, and besides, its effect is always equal and opposite in amount in symmetrically-placed elements of the suspended coil.

The effect of the earth's magnetic field is eliminated by the reversal of the current through the whole instrument. We, therefore, obtain balances with all possible permutations of the current in the two coils—four balances in all. The differences among these four are appreciable, but of very small magnitude.

When we have succeeded in reducing the effect of elastic fatigue in the suspension to smaller values, we shall hope to reach results accurate to at least one part in 5,000. For the present we are only prepared to say that the legalised value of the E.M.F. of the Clark cell (1·434 volts under standard conditions) is too high.

THE SO-CALLED INTERNATIONAL ELECTRICAL UNITS.

By Dr. FRANK A. WOLFF, National Bureau of Standards, Washington.

As one of the most important questions likely to be considered by the St. Louis International Electrical Congress will be that of redefining the fundamental electrical units, it may not be out of place at this time to review briefly the efforts which have thus far been made to bring about international uniformity in this respect.

The need of a definite and universal system of electrical units was early recognised, and became a necessity as soon as industrial applications of electricity were made. At first the principal measurements were those of resistance (line resistance, insulation resistance, measurements for the location of faults, etc.). These were expressed in terms of some entirely arbitrary standard, such as the resistance of a given length of an iron or copper wire of given cross-section. This naturally led to a great multiplicity of units, none of which ever gained general acceptance.

In 1848 Jacobi pointed out that it would be more satisfactory to adopt as a universal standard the resistance of a certain piece of wire, copies having the same resistance being easily constructed. Jacobi carried this suggestion into practice by sending copies of his standard, since known as "Jacobi's Etalon," to the leading physicists of that period.

In 1860 Werner von Siemens proposed as a standard of resistance the resistance, at 0 deg. C., of a column of mercury of a uniform cross-section of 1 sq. mm. and 1 m. in length.

In 1861 a committee composed of the most eminent English physicists was appointed by the British Association to consider the question of standards of electrical resistance. The leading foreign physicists were invited to offer suggestions, and various special investigations of the problems with which the committee was confronted were undertaken by its members.

It was decided that the unit of resistance should be defined in terms of the Gauss-Weber absolute system of electromagnetic units, which had already received such well-merited recognition; but since this unit was inconveniently small, it was decided to define the practical unit as an integral decimal multiple of the same.

The value of the unit depends upon the units of length, mass, and time adopted as the basis of the system. Those chosen by Gauss and Weber were the millimetre, milligram, and second; while in England efforts were being made to establish an absolute system for the definition of all physical units, for which the fundamental units of Weber were of inconvenient magnitude, and for which the centimetre, gramme, and second were finally adopted (the c.g.s. system).

The practical unit of resistance in this system was defined as 10^9 c.g.s. electromagnetic units; and while this definition fixes the unit theoretically, it can only be applied in practice by the measurement of

some particular resistance in absolute measure. This requires the construction of especially designed apparatus, with which measurements lying within a very limited range may be made; the determination of its instrumental constants most frequently involving tedious mathematical approximations, and the elimination of errors of observation. With all possible precautions the errors of such methods exceed, even to-day, a hundredfold the relative errors in resistance comparisons.

Investigations were, therefore, made to determine whether the absolute unit of resistance could be accurately defined in terms of the resistance of a definite portion of a definite substance. The electrical properties of alloys and pure metals in the solid and liquid states were studied with this end in view. On account of the excessive influence, on the resistance, of even small quantities of impurities in metals of the highest obtainable purity, and of small variations in the composition of alloys, the choice was greatly limited. It was found, in addition, that solid metals had to be rejected on account of the marked influence of physical changes produced by annealing, hardening, drawing, bending, etc.

Mercury, already recommended by Siemens, was, therefore, the only material to be further considered, but was also rejected for two reasons, viz., the large differences found to exist between coils supposedly adjusted to different German mercurial standards, and differences between a number of mercurial standards constructed by members of the committee.

The committee, therefore, recommended the alternative method of constructing material standards adjusted with reference to the absolute unit. In this connection a special form of resistance standard, known as the British Association type, was designed, and after an investigation of the constancy of a number of new alloys in addition to many already in use, one containing two parts by weight of silver to one part by weight of platinum, was finally selected as best meeting all requirements.

In 1863 and 1864 the values of certain coils were determined in absolute units by one of the methods proposed by Weber, and from these measurements the "B. A." unit was derived. A number of copies were issued gratis by the Association, and, in addition, arrangements were made for supplying others at a moderate price. The B. A. unit soon gained general acceptance in the English-speaking countries, while the Siemens unit still retained its supremacy on the Continent.

No action was at that time taken by the British Association committee to define the units of current and electromotive force further than in terms of the c.g.s. system. The currents to be measured were all relatively small, and were usually measured by means of a tangent galvanometer with a sufficient accuracy. Electromotive forces were seldom measured, and then usually in terms of the Daniell cell. In 1872 Latimer Clark brought to the attention of the committee the superiority of the cell which now bears his name, recommending it as a suitable standard of electromotive force, but no definite action was taken by the committee.

In 1878 it was shown by Professor H. A. Rowland that the B. A. unit was in error by more than 1 per cent., and soon after the existence of a discrepancy of this magnitude was verified by a number of other investigators.

In 1881 a call was issued by the French Government for an International Electrical Congress, to be held in connection with the first International Electrical Exposition at Paris, for the purpose of adopting definitions of the electrical units which might serve as a basis for legislative enactments. In the meantime, a number of mercurial standards had been constructed and had been found to be in satisfactory agreement; moreover, the results of most of the absolute determinations had been referred either directly or indirectly to the Siemens unit.

The Paris Congress, therefore, recommended that the practical electrical units be defined in terms of the units of the c.g.s. system of electromagnetic units, and that the unit of resistance be represented by a column of mercury 1 sq. mm. in cross-section, at the temperature of 0 deg. C., of a length to be determined by an International Commission appointed for this purpose, as appears in the following resolutions:—

RESOLUTIONS OF THE INTERNATIONAL CONGRESS OF ELECTRICIANS, PARIS, 1881.

1. That the c.g.s. system of electromagnetic units be adopted as the fundamental units.
2. That the practical units, the ohm and the volt, preserve their previous definitions, 10^9 and 10^8 c.g.s. units respectively.
3. That the unit of resistance, the ohm, be represented by a column of mercury 1 sq. mm. in cross-section at the temperature of 0 deg. C.
4. That an International Commission be charged with the determination, by new experiments, of the length of the mercury column 1 sq. mm. in cross-section, at a temperature of 0 deg. C., representing the ohm.
5. That the current produced by a volt in the ohm be called an ampere.
6. That the quantity of electricity produced by a current of 1 ampere in one second be called a coulomb.
7. That the unit of capacity be called a farad, which is defined by the condition that a coulomb in a farad raises the potential 1 volt.

The Congress * also recommended the employment of the carcel as the standard for photometric comparisons.

The International Commission appointed in accordance with paragraph 4 of the resolutions of the Paris Congress of 1881 met at Paris in 1882, but definite action was deferred until two years later, when the following definitions were unanimously recommended:—

The legal ohm is the resistance of a column of mercury 1 sq. mm. in cross-section, and 106 cm. in length, at a temperature of melting ice.

The ampere is equal to one-tenth of a c.g.s. unit of the electromagnetic system.

The volt is the electromotive force which will maintain a current of 1 ampere in a conductor of which the resistance is a legal ohm.

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* For the sake of completeness, the recommendations of the various International Electrical Congresses on photometric standards are included in the summary.

The value adopted for the length of the mercurial column was taken as 106 cm., notwithstanding that most of the best results were very close to 106·3, and it was thought advisable to adopt a value known to be true to the nearest centimetre for a period of ten years. On account of this uncertainty, no steps were actually taken by the various Governments represented.

The Conference also adopted as the unit of light of any colour the quantity of such light emitted in a perpendicular direction by 1 sq. cm. of molten platinum at the temperature of solidification; and as the practical unit of white light the total quantity of light emitted perpendicularly by the same source.

In 1889 a second International Congress of Electricians was held at Paris, by which the following definitions were adopted:—

The joule, the practical unit of energy, is equal to 10^7 c.g.s. units. It is equal to the energy disengaged as heat in one second by a current of 1 ampere flowing through a resistance of 1 ohm.

The practical unit of power is the watt. The watt is equal to 10 c.g.s. units, and is the power of 1 joule per second.

The practical unit of self-inductance is the quadrant, which is equal to 10^9 cms.

The Congress recommended that the power of machines be expressed in killowatts instead of in horse-power.

It adopted also, as the photometric standard, the "bougie decimal," defined as one-twentieth of the Violle platinum standard adopted by the Conference of 1884.

The following definitions were also adopted:—

The period of an alternating current is the duration of a complete oscillation.

The frequency is the number of periods per second.

The mean intensity is defined as the mean value of the current during a complete period. without reference to its sign.

The effective intensity is the square root of its mean-squared value.

The effective electromotive force is the square root of its mean-squared value.

The apparent resistance is the factor by which the effective current must be multiplied to obtain the effective electromotive force.

The positive pole of a storage cell is that which is connected to the positive pole of a dynamo in charging, and which is the positive pole during its discharge.

In addition, the question of defining and naming practical magnetic units was discussed. The definition proposed for the unit of field intensity was the intensity of a uniform field which would produce an electromotive force of 1 volt in a conductor 1 cm. in length normally cutting the lines of force with a velocity of 1 cm. per second. The name proposed for this unit was the "Gauss"; and as the unit, which is equal to 10^8 c.g.s. units, does not correspond to field intensities ordinarily dealt with, the micro-Gauss was suggested for ordinary use.

The Weber, defined as 10^8 c.g.s. units, was proposed as the unit of magnetic flux.

No definite action was, however, taken by the Congress on either of these units.

The increased accuracy obtainable by the use of apparatus of improved construction, and by refinements in the methods employed, led to a much closer agreement of the various redeterminations of the absolute electrical units, and their relation to the Siemens unit, the Clark cell, and the electrochemical equivalent of silver in terms of which many measurements were made. The rapid development of the electrical industries also called for a redefinition of the units, and the legalisation of such definitions.

In December, 1890, a committee was appointed by the English Board of Trade to consider what action should be taken by the Board with a view to causing new denominations of standards for the measurements of electricity for use for trade to be made and duly verified. The members of this committee consisted of two representatives each of the Board of Trade, the General Post-Office, the Royal Society, the British Association, and the Institution of Electrical Engineers.

A set of resolutions embodying the proposals which appeared to be desirable were drafted, and copies of the same were submitted to the various interests for criticism. These resolutions also embodied proposals for standards of resistance, current, and electromotive force.

In 1891 a committee was appointed by the American Institute of Electrical Engineers to report on units and standards. The report of the committee, made in June, 1891, which deals mainly with magnetic units, is as follows :—

Your committee, considering that authorised and recognised names for four practical electromagnetic units, at present unentitled, are needed by electrical engineers in this as well as in other countries, for dealing conveniently with magnetic circuits in analysis, discussion, and design, recommends to the Institute the four units as appended in detail, of magnetomotive force, reluctance, flux, and flux-density, in the hope that, if favourably considered, the Institute may further the endeavours of the next International Electrical Congress toward securing for them universally recognised titles.

. . . .

1st. *Magnetomotive Force;* or difference of magnetic potential.

Simple definition.—The analogue in a magnetic circuit of voltage in an electric circuit.

Strict definition.—The magnetomotive force in a magnetic circuit is four π multiplied by the flow of current linked with that circuit.

The magnetomotive force between two points connected by a line is the line integral of magnetic force along that line. Difference of magnetic potential constitutes magnetomotive force.

Electromagnetic dimensional formula, $L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$.

The absolute unit of M.M.F. is $\frac{1}{4\pi}$ x unit current of one turn.

The practical unit is $\frac{1}{4\pi}$ x ampere of one turn, or one-tenth of the absolute unit, *i.e.*, 0·0796 ampere-turn gives the unit. The prefix kilo- would perhaps be occasionally used for practical applications.

2nd. *Magnetic Flux.*

Simple definition.—Total number of lines of force or total field.

Strict definition.—The magnetic flux through a surface bounded by a closed curve is the surface integral of magnetic induction taken over the

bounded surface, and when produced by a current is also equal to the line integral of the vector potential of the current taken around the boundary.

The uniform and unit time rate of change in flux through a closed magnetic circuit establishes unit electromotive force in the circuit.

Electromagnetic dimensional formula, $L^{\frac{3}{2}}M^{\frac{1}{2}}T$.

The absolute unit is one c.g.s. line of induction.

The practical unit is 10^8 c.g.s. lines.

Fluxes range in present practical work from 100 to 100,000,000 c.g.s. lines, and the working units would perhaps prefix milli- and micro-.

3rd. *Magnetic Intensity*, or induction density.

Simple definition.—Flux per sq. cm.

Strict definition.—The induction density at a point within an element of surface is the surface differential of the flux at that point.

Electromagnetic dimensional formula, $L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$.

Absolute unit, one c.g.s. line per sq. cm.

Practical unit, 10^8 c.g.s. lines per sq. cm.

In practice, excluding the earth's field, intensities range from 100 to 20,000 lines per sq. cm., and the working unit would perhaps have the prefix milli- or micro-.

4th. *Magnetic Reluctance.*

Definition.—Unit reluctance in a magnetic circuit permits unit magnetic flux to traverse it under the action of unit magnetomotive force.

Dimensional formula, $L^{-1}M^{0}T_{0}$.

The practical unit is 10^{-9} the absolute unit.

Reluctances vary in present practical work from 100,000 to 100,000,000 of these practical units; so that the working unit would perhaps employ the prefix mega-.

There were considerable differences of opinion manifested in the discussion following the presentation of the report, and definite action thereon was postponed.

At the Frankfort International Electrical Congress, in September, 1891, the question of naming and defining the magnetic units was brought up. The names Gauss and Weber, for field intensity and flux respectively appeared to meet with general approval, but there was considerable disagreement as to what their numerical values should be, 10^8 being apparently preferred for both.

Owing to the limited time allowed for consideration, no definite action was taken.

In connection with the British Association meeting in Edinburgh in 1892, a conference was held, attended by Helmholtz, Guillaume, and others, to discuss the Board of Trade Report, which was submitted at the meeting. It was resolved to adopt for the length of the mercurial column 106·3 cms., and to express the mass of the column of constant cross-section instead of the cross-sectional area of 1 sq. mm. Final action was deferred to await the decision of the Chicago International Electrical Congress, arrangements for which had then been made.

This Congress, to which the various Governments were invited to send delegates, met in 1893. The Governments represented were: United States, Great Britain, France, Italy, Germany, Mexico, Austria, Switzerland, Sweden, and British North America. Prof. von Helmholtz was made Honorary President of the Congress, and Prof. H. A. Rowland President of the Chamber of Delegates. A Chamber of Delegates was organised, composed of the official delegates of the various Governments represented, by which the following resolutions, were adopted after six days' deliberation:—

Resolutions of the International Electrical Congress, Chicago, 1893.

Resolved, That the several Governments represented by the delegates of this International Congress of Electricians be, and they are hereby, recommended formally to adopt as legal units of electrical measure the following:—

Ohm. As a unit of resistance, the *international ohm*, which is based upon the ohm equal to 10^9 units of resistance of the c.g.s. system of electromagnetic units, and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice 14·4521 grammes in mass, of a constant cross-sectional area and of the length of 106·3 cms.

Ampere. As a unit of current, the *international ampere*, which is one-tenth of the unit of current of the c.g.s. system of electromagnetic units, and which is represented sufficiently well for practical use by the unvarying current which, when passed through a solution of nitrate of silver in water, and in accordance with accompanying specifications, deposits silver at the rate of 0·001118 of a gramme per second.

Volt. As a unit of electromotive force, the *international volt*, which is the electromotive force that, steadily applied to a conductor whose resistance is 1 international ohm, will prodnce a current of 1 international ampere, and which is represented sufficiently well for practical use by $\frac{1000}{1434}$ of the electromotive force between the poles or electrodes of the voltaic cell known as Clark's cell, at a temperature of 15 deg. C., and prepared in the manner described in the accompanying specifications.

Coulomb. As a unit of quantity, the *international coulomb*, which is the quantity of electricity transferred by a current of 1 international ampere in one second.

Farad. As a unit of capacity, the *international farad*, which is the capacity of a condenser charged to a potential of 1 international volt by 1 international coulomb of electricity.

Joule. As a unit of work, the *joule*, which is equal to 10^7 units of work in the c.g.s. system, and which is represented sufficiently well for practical use by the energy expended in one second by an international ampere in an international ohm.

Watt. As a unit of power, the *watt*, which is equal to 10^7 units of power in the c.g.s. system, and which is represented sufficiently well for practical use by work done at the rate of 1 joule per second.

Henry. As the unit of induction, the *henry*, which is the induction in a circuit when the electromotive force induced in this circuit is 1 international volt, while the inducing current varies at the rate of 1 ampere per second.

Specifications.

In the following specifications the term silver voltameter means the arrangement of apparatus by means of which an electric current is passed through a solution of nitrate of silver in water. The silver voltameter measures the total electrical quantity which has passed during the time of the experiment, and by noting this time the time average of the current, or if the current has been kept constant, the current itself can be deduced.

In employing the silver voltameter to measure currents of about 1 ampere, the following arrangements should be adopted:

The kathode on which the silver is to be deposited should take the form of a platinum bowl not less than 10 cms. in diameter and from 4 to 5 cms. in depth.

The anode should be a plate of pure silver some 30 sq. cms. in area and 2 or 3 mms. in thickness.

This is supported horizontally in the liquid near the top of the solution by a platinum wire passed through holes in the plate at opposite corners. To prevent the disintegrated silver which is formed on the anode from falling on to the kathode, the anode should be wrapped round with pure filter paper, secured at the back with sealing-wax.

The liquid should consist of a neutral solution of pure silver nitrate, containing about 15 parts by weight of the nitrate to 85 parts of water.

The resistance of the voltameter changes somewhat as the current passes. To prevent these changes having too great an effect on the current, some resistance besides that of the voltameter should be inserted in the circuit. The total metallic resistance of the circuit should not be less than 10 ohms.

Specifications for the Clark Cell.

A committee, consisting of Messrs. Helmholtz, Ayrton, and Carhart, was appointed to prepare specifications for the Clark's cell. Owing to the death of von Helmholtz no report was ever made by this committee.

Magnetic Units.

A motion was made and carried that for magnetic units the c.g.s. system be commended, and that for the present no names be given to these units.

Photometric Standards.

A resolution was adopted as follows :—

Resolved, that this committee, while recognising the great progress realised in the standard lamp of von Heffner-Alteneck, and the very important researches made at the Reichsanstalt, also recognises that other standards have been proposed and are now being tried, and that there are serious objections to every kind of standard in which an open flame is employed. It is, therefore, unable to recommend the adoption at the present time of either the von Heffner lamp or the pentane lamp, but recommends that all nations be invited to make researches in common on well-defined practical standards, and on the convenient realisation of the absolute unit.

In March, 1900, the following resolution was adopted by the American Institute of Electrical Engineers :—

Moved, That the committee on units and standards be requested to investigate and report at the ensuing meeting in regard to the advisability of the following :—

1. The giving of names to the absolute units of the electrostatic and electromagnetic systems.
2. The denotations, by means of prefixes, of multiples of such units.
3. The rationalisation of the present system by means of taking the absolute unit of magnetism as equal to the present magnetic line, and the absolute unit of difference of magnetic potential as equal to the present absolute unit of current-turn.
4. The advisability of taking up any or all of the above matters at the Congress to be held in Paris this year.

In May, 1900, the following report of the committee was adopted by the Institute :—

1. We consider that there is need for names for the absolute c.g.s. units in the electrostatic and the electromagnetic systems ; also for suitable prefixes to denote decimal multiples and sub-multiples of these units, in supplement and addition to those already in common use.
2. That the International Electrical Congress convening this year at Paris should be urged to bestow the above-mentioned names and create said decimal prefixes.
3. That much advantage would accrue to a universal "rationalisation"

of electric and magnetic units, and that the Congress be requested to consider the means and advisability of such "rationalisation."

4. That we recommend that the whole subject should be brought up as a topic for general discussion at the approaching general meeting of the Institute in Philadelphia.

(*Signed*) F. B. CROCKER
W. E. GEYER
G. A. HAMILTON
W. D. WEAVER
A. E. KENNELLY, *Chairman.*

PARIS CONGRESS, 1900.

The last official Congress was held at Paris in August, 1900.

A committee of Section 1 to consider questions in reference to the units reported as follows:—

The committee will only take into consideration propositions not involving modifications of the decisions of previous Congresses.

The committee believes that there is no actual need of giving names to all the electromagnetic units.

However, owing to the employment, in practice, of apparatus giving directly field intensities to c.g.s. units, the committee recommends giving the name "Gauss" to this c.g.s. unit.

The committee recommends giving to the unit of magnetic flux, the value of which is subsequently to be fixed, the name "Maxwell."

The report adopted by the Section, after a spirited discussion, was as follows:—

1. The Section recommends giving the name "Gauss" to the c.g.s. unit of magnetic field intensity.
2. The Section recommends giving the name "Maxwell" to the c.g.s. unit of magnetic flux.

These units were given an international character and standing by their adoption at the general meeting of the official delegates of the various Governments, after a stormy debate.

PART II.

THE LEGALISATION OF THE ELECTRICAL UNITS BY THE VARIOUS GOVERNMENTS.*

Notwithstanding that the resolutions of the Chicago Congress were adopted with practical unanimity, and might, therefore, have been considered as in a sense binding on the various Governments, up to this date only six Governments, United States, Great Britain, Canada, Germany, Austria, and France, have legislated on this subject, and only a few of these have acted strictly in accordance with the resolution of the Chicago Congress.

DISCUSSION OF LEGALISATION.

Strictly speaking, no two countries have defined the electrical units in the same way. This naturally suggests that there must be good and sufficient reasons, which may in part be traced to the insufficiency of the Chicago definitions.

* For copies of the laws, see Bulletin No. 1, Bureau of Standards.

1. It is evident that all three of the units should not be defined in terms of concrete standards, connected as they are by Ohm's law so that only two of the three are independent, and hence the third should be defined in terms of the other two.

2. The two units adopted as fundamental should be defined only in terms of concrete standards, and not in terms of the absolute units.

3. The specifications for the silver voltameter were shown to be inadequate.

4. Redeterminations of the electromotive force of the Clark cell at 15° C. in absolute measure indicated that this value was nearer 1·433 volts than 1·434 volts.

However, the variations introduced in the definitions by some of the Governments lead to confusion, and are in violation of the principles laid down at the Chicago Congress.

The Unit of Resistance.

Taking the fundamental units up in turn, it will be found that the unit of resistance legalised by the United States, Germany, France, and Canada, and the definitions in the proposed Belgian and Swiss laws, are essentially the same as those adopted at Chicago, differing only in that no reference is made to the unit of resistance being based on 10^9 c.g.s. units in case of the German and French laws, and in the proposed Swiss and Belgian laws. In fact, it must be admitted that this statement may be regarded as superfluous.

Austria. In Austria, on the other hand, the unit of resistance is defined as 10^9 c.g.s. units of the electromagnetic system, which "*for practical purposes* is to be considered equal to the resistance offered at the temperature of melting ice by a column of mercury 106·3 cms. in length, and having a mass of 14·4521 grammes." The uniformity of cross-section is, curiously, not specified.

Great Britain. In Great Britain, finally, the ohm is defined both as *having the value* of 10^9 in terms of the centimetre and the second of time, *and as being represented* by "the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice 14·4521 grammes in mass of a constant cross-sectional area and of a length of 106·3 c.m.s." In addition, a distinction is made between the *unit* of resistance and the *standard* of resistance, and for the latter purpose a particular platinum-silver coil preserved at the Board of Trade Electrical Standardising Laboratory, in London, and adjusted to represent the unit on an assumed relation between the standards of the British Association and the mercurial unit, is legalised.

It will thus be seen that the unit of resistance has been defined:

1. In terms of the absolute c.g.s. unit.
2. In terms of the mercurial column.
3. In terms of the resistance of a particular coil.
4. In terms of combinations of the above.

The objections to the first method have been recognised as long as the subject has been under discussion. For, while the unit is theoretically fixed, resort must in practice be had to material standards, in the absolute measurement of which, errors amounting to at least 0·01 per

cent. are introduced. Errors several times as great are even met with in different series of observations with the same apparatus, and the difference of the results obtained by different methods may differ still more.

To overcome this objection a suggestion was made in 1893 by Professors W. E. Ayrton and A. V. Jones, that the unit of resistance be defined in terms of a particular Lorenz apparatus preserved in a National Physical Laboratory; but even then an uncertainty of at least 0·01 per cent. would remain, if this practice were adopted by a single Government, whereas its general introduction would certainly introduce greater differences.

Another objection to this method lies in the limited range within which accurate measurements of resistance may be made with a given apparatus, so that in practice the measurements would have to be referred to material standards, the constancy of which might from time to time be checked to within the above stated limit of accuracy.

Notwithstanding these objections we find that a number of Governments have defined the unit of resistance in terms of the absolute unit, indicating that the above principles are not fully appreciated.

The accuracy with which resistance comparisons can be made has for a long time far exceeded the above limits, and the need of an accurate standard, reproducible at any time and at any place to a higher degree of accuracy, has been recognised, as this would enable measurements the world over to be expressed in terms of the *same* unit—a result of much greater importance than in absolute measure, with its limited accuracy. In the definition of the concrete standard it is only necessary to assume for it a value in accordance with the best absolute measurements. This once done with a sufficient approximation, the definition of the concrete standard need not be modified.

The metre was originally intended to represent the one-ten-millionths part of the earth's quadrant; but in the actual construction errors of measurement were introduced, which will, however, not affect the *international* metre defined in terms of a particular platinum-iridium bar, of which accurate copies exist the world over, and to which all linear measurements are referrred. In a similar manner, the kilogram was intended to represent the mass of a cubic decimeter of water at the temperature of its maximum density; but the *international* kilogram is the mass of a particular cylinder of platinum-iridium, to which all measurements of mass are referred.

It has, therefore, been generally recognised that *reproducibility* should be the first requirement for any international standard, and this qualification is fulfilled to an eminent degree by the mercurial unit, as defined by the Chicago Congress.

When this definition was adopted it was generally assumed that such mercurial standards would be constructed by the various Governments represented; but this has been done by only two—Germany and Great Britain—each of which is provided with an institution equipped to undertake this task. The construction of primary mercurial standards is also to be undertaken by the Bureau of Standards, more recently organised, and no doubt by other institutions.

The mercurial standards at the Reichsanstalt agree with one another to within a few parts in 100,000, as do those of the English National Physical Laboratory; and it is, in addition, most gratifying to know that the standards of the two institutions agree with each other almost equally well.

There is, however, one criticism which might still be made of the definition of the unit of resistance in terms of the mercurial unit. Some form of terminal must be applied to the tube to connect it to the circuit containing the resistance standard with which it is to be compared. The method used at the Reichsanstalt consists in employing spherical bulbs, each provided with a current and potential lead, which necessitates the application of a correction, the value of which can be calculated approximately, as Lord Rayleigh has shown, or may be experimentally determined. Unfortunately, the value experimentally found is less than the minimum limit according to Lord Rayleigh's calculations, so that a different result would be obtained according to the correction factor employed.

In addition, there is another method which might, and has been used, of applying potential terminals to the extremities of the tube, which is provided with prolongations previously continuous with the same. This method also introduces a correction the value of which would depend upon a number of conditions.

In any case, however, this source of uncertainty, although slight, could be eliminated by specifying the approximate cross-section or length of the tube representing the unit, the nature of the terminals, and the magnitude of the correction factor to be applied.

The Unit of Current.

The Chicago definition of the ampere as one-tenth of the c.g.s. unit has been followed almost verbatim by the United States, Canada, France, and Austria, and the specifications for the silver voltameter are essentially the same except in Austria, where no specifications have been legalised.

In Germany the ampere is simply defined in terms of the electrochemical equivalent of silver, and in addition the specifications for the silver voltameter are considerably modified.

The proposed Swiss law has been copied from the law adopted by Germany.

The Belgian law differs from the German law only in that the ampere is defined, not as being equal to, but as being sufficiently well represented for practical purposes by "the intensity of a constant current which precipitates in one second 0·001118 grammes of silver from an aqueous solution of silver nitrate."

In Great Britain it is defined both as one-tenth of a c.g.s. unit, and *as being represented by* "the unvarying electric current which, when passed through a solution of nitrate of silver in water in accordance with the specification appended hereto and marked *A*, deposits silver at the rate of 0·001118 of a gramme per second." In addition, a distinction is made between the *unit* of current and the *standard* of current, the

latter being defined in terms of a particular standard ampere balance preserved in the Board of Trade Electrical Standardising Laboratory.

It will thus be seen that the ampere is defined in three distinct ways, and in some cases the same country has defined it in two or more ways. As has been pointed out, if the ampere is selected as the second fundamental unit, it should not be defined in terms of the absolute unit, but simply in terms of the silver voltameter, for which, according to a number of investigations since 1893, the specifications are quite insufficient, as differences amounting to more than 0·1 per cent. may be obtained. The cause of these variations was first shown by Kahle at the Reichsanstalt to be due to secondary reactions in the voltameter, as indicated by differences when freshly-prepared silver nitrate solutions and solutions previously used are employed. Richards, Collins, and Heimrod have traced this influence to the secondary reactions at the anode, and have shown that they may be reduced, and possibly eliminated, by surrounding the same with a porous cup in which the solution is always kept at a lower level than outside, to prevent diffusion.

This subject has been further investigated by Dr. K. E. Guthe, of the Bureau of Standards, who confirmed the results of Richards, Collins, and Heimrod, and who showed, in addition, that the secondary reactions at the anode are further decreased by the use of a large anode, the best results being obtained with a silver plate in contact with granulated silver. Variations in the results have been attributed by Leduc to the filter paper, with which in the older forms the anode is surrounded. That the silver nitrate acts upon the paper cannot be questioned, but its influence on the solution can hardly explain the results.

The filter-paper, however, fails to prevent the secondary products formed at the anode, the exact nature of which has not been established, from reaching the kathode, while the porous cup prevents it almost entirely. The results obtainable with the Richards and the modified forms seem to be reproducible to within about one part in 20,000, so that unit current may be defined in terms of the electrochemical equivalent of silver to within this order of accuracy.

The two arguments most frequently advanced in favour of concretely defining the ampere, instead of the volt, are as follows:—

1. According to Faraday's laws of electrolysis, the amount of a given metal deposited in a given time by a given current is constant; but, while Faraday's laws may be fundamental laws of nature, as seen above, complications are introduced by secondary reactions at the electrodes, which vary with the metal and the current density employed. Constant results are, however, obtainable by specifying the form and manner of employment of the voltameter, at least in the case of the silver voltameter.

2. Current intensity can be determined in absolute measure directly by the electrodynamometer, while electromotive force can only be measured directly in absolute units of the electrostatic system, and the accuracy with which the results can be reduced to electromagnetic units depends upon the accuracy with which "v," the ratio of the units

of the two systems, or the velocity of light is known, and this is uncertain by possibly as much as one part in one thousand.

While this argument would have considerable weight if the fundamental units were to be defined in terms of their absolute values, a practice which, as pointed out above, should be abandoned entirely, it has little bearing on the definition of either current or electromotive force in terms of a concrete standard, for which a value may be adopted which agrees with the best results of absolute current measurement.

The objections which might be urged against defining the ampere, instead of the volt, are as follows :—

1. With a given silver voltameter the range is limited, and only currents lying within certain narrow limits can be accurately measured.

2. Enough time must be allowed for the deposit of at least several grammes of silver, so that accurate weighings may be made.

3. The duration of the experiment must be at least one half-hour, in order that the time may be accurately measured.

4. During the experiment the current must be kept constant by continuous regulation, or the variations from its mean value must be determined at frequent intervals, so that the average value may be calculated.

5. Tedious double weighings must be made to determine the amount of silver deposited.

6. The result finally obtained applies to the average value of the current employed during the experiment, and cannot be utilised for the accurate measurement of other currents except by reference to a standard cell and a standard resistance, or to some form of apparatus for current measurement, such as the electrodynamometer, in which case the accuracy is not as great as that obtained by direct reference to a standard cell.

Unit of Electromotive Force.

The definition of the volt adopted at Chicago has been legalised almost verbatim by the United States, Canada, and France. In Germany and Austria it is defined simply in terms of the ohm and ampere, as is also the case in the proposed Swiss and Belgian laws.

In England the volt is defined as 10^8 c.g.s. units, in terms of the ohm and ampere, and in terms of the Clark cell. In addition, a distinction is made between the *unit* of electromotive force and the *standard* of electromotive force, the latter being defined as the 1/100 part of the pressure producing a certain deflection of a Kelvin electrostatic voltameter of the multicellular type preserved at the Electrical Standardising Laboratory of the Board of Trade.

Here again the definitions legalised differ considerably. Of the various definitions only two need be considered—that in terms of the ohm and ampere, if these units are taken as fundamental, and the definition in terms of the standard cell, if the ohm and volt are taken as the fundamental units.

The arguments in favour of the latter alternative may be briefly summarised as follows :—

1. The facility with which any voltage may be directly measured in terms of the standard cell by the potentiometer.

2. The accuracy with which such measurements may be made, which is practically limited only by the accuracy with which resistance is measurable and by the reproductibility of the cell.

3. The accuracy with which the standard cell can be reproduced, which even to-day exceeds all practical requirements, and which may be still further increased by specifying more precisely the manner of purification and preparation of the materials employed, etc.

4. The resulting definition of the ampere in terms of the ohm and volt, which corresponds to the actual method employed in precision measurements of current intensity by the potentiometer method.

5. The facility and accuracy with which any current may be thus measured.

These considerations have led to the adoption of the Clark cell in Germany as the practical standard of electromotive force, notwithstanding that the ampere is legally defined in that country in terms of the electrochemical equivalent of silver and the volt in terms of the ohm and ampere.

For the electromotive force of the Clark cell, the value 1·4328 was adopted in Germany as equivalent to the legalised definitions of the ohm and ampere.

In the United States the legalised value of the electromotive force of the Clark cell is 1·434 volts at 15 deg. C. Either this value had to be taken or that of the electrochemical equivalent of silver. The latter was out of the question owing to the insufficiency of the legalised specifications for the silver voltameter and the large variations reported.

The specifications for the Clark cell legalised in the United States were drawn up by the National Academy of Sciences, and refer to the λ type; while those in England, Canada, and France are essentially those drawn up by the Board of Trade committee and refer to the Board of Trade type.

Standard Cells.

Of the standard cells proposed, only the Clark and the Weston cells are to be considered at present. In the former the electrodes consist of zinc amalgam covered with a layer of zinc sulphate crystals, and pure mercury in contact with a paste of mercurous sulphate, zinc sulphate crystals and metallic mercury, the electrolyte being a saturated aqueous solution of zinc sulphate and mercurous sulphate.

In the Weston cell the electrodes consist of cadmium amalgam covered with a layer of cadmium sulphate crystals, and pure mercury in contact with a paste of mercurous sulphate, cadmium sulphate crystals, and metallic mercury, the electrolyte being a concentrated aqueous solution of cadmium sulphate and mercurous sulphate.

The investigations thus far reported indicate that differences between individual cells of either type, set up from materials obtained from various sources and at various times, agree with each other to within 0·0002 volt, corresponding to a slight advantage in favour of the Clark cell on account of its higher electromotive force. The

constancy and reproducibility of both types have also been established by the constancy of the ratio between them.

While sharing with the Clark cell these most essential qualities, the Weston cell has a number of marked advantages.

1. The higher temperature coefficient of the Clark cell is a serious obstacle to measurements of the highest precision, while that of the Weston cell at ordinary temperatures is less than one-twentieth as great, so that errors due to temperature uncertainties are correspondingly reduced.

2. Clark cells are subject, particularly when a number of years old, to large hysteresis effects attending temperature variations. In the Weston cell the error due to this cause can only amount to a small fraction of that in the Clark cell, owing to the relatively slight influence of temperature on the solubility of the cadmium sulphate.

3. The average life of Clark cells is quite short, owing to the tendency of the cell to crack at the point where the platinum terminal is fused into the amalgam limb. This objection might be obviated by suitable modifications in the construction, as have been suggested, but not without introducing some complication. No such tendency has been observed with Weston cells.

4. In Clark cells a layer of gas is formed at the amalgam surface, even when carefully-neutralised solutions are employed, which may interrupt the circuit, thus rendering the cell useless. In the Weston cell no gas is, apparently, formed.

Owing to these marked advantages, the Weston cell is certain to displace the Clark cell in the laboratory, and no doubt many advocates of the adoption of the former as the standard of electromotive force will be found among the delegates to the St. Louis International Congress.

Specifications for the Standard Cell.

If either the Clark or the Weston cell be adopted as the standard of electromotive force, the specifications will have to be to some extent redrawn if the highest accuracy of reproduction is sought, as the differences between individual cells set up with different materials at present far exceed the relative errors made in current and electromotive force measurements by the potentiometer method. It therefore seems desirable, as stated above, to specify more precisely the methods of purification and preparation of the materials employed.

Fortunately the metals entering into the composition of Clark and Weston cells—mercury, zinc, and cadmium—are among the few which can be obtained by special methods so pure that the foreign metals in them do not exceed more than 0·001 per cent. Zinc sulphate and cadmium sulphate can be obtained from the specially purified metals and pure sulphuric acid. Even considerable quantities of the impurities usually accompanying the above materials, when purchased as "chemically pure," exert a relatively small and even insignificant influence on the electromotive force of the cell. In defining the standard cell, however, the method of preparation, or purification, and the degree of purity of the materials, should certainly be specified.

The principal source of variation of the standard cell has lately been shown to be due to differences in the electromotive properties of the mercurous sulphate. The "chemically pure" mercurous sulphate of commerce contains, besides nitrates, etc., basic mercurous sulphate, mercuric sulphate, basic mercuric sulphate, and possibly sulphites. According to the Chicago specifications, since generally adopted, the mercurous sulphate is washed a number of times with distilled water, which converts the mercuric sulphate into basic mercuric sulphate, which is not removed. Moreover, the water hydrolises the mercurous sulphate, converting part of it into basic mercurous sulphate. Both these materials having a definite solubility in the zinc sulphate and cadmium sulphate solutions, must exert an influence on the electromotive force of the cell. The basic mercurous sulphate, when present in excess, will exert an influence on the electromotive force, while the basic mercuric sulphate is gradually decomposed and eliminated, thus introducing a variable factor.

Pure mercurous sulphate, however, may be obtained from pure mercury and sulphuric acid, by an electrolytic method independently devised by Carhart and Hulett, and the author, and the results already obtained indicate that the agreement of cells set up with this material is within a few parts of 100,000.

It is, therefore, most important, if the unit of electromotive force is defined in terms of the standard cell, to specify the manner in which this material is to be prepared, and to modify some of the specifications relating to its treatment.

Besides new specifications for the ampere or volt in terms of the electrochemical equivalent of silver, or the electromotive force of some particular standard cell, respectively, it will be necessary to adopt a new value for one of these constants. This may be based either on the absolute determinations already made, applying to the accepted values corrections determined by the modifications in the specifications which may be adopted and a correction in order to bring the unit into closer agreement with the absolute value upon which it is based, or by new absolute determinations. If the latter alternative is decided upon, considerable delay would probably ensue, and in addition not much could be gained, owing to the relatively large errors of all absolute measurements and the differences likely to be found between the results obtained by different investigators using different methods and apparatus.

Two determinations of the electromotive force, of the Clark cell in absolute measure, made by Kahle, at the Reichsanstalt, and by Carhart and Guthe, indicate that the value adopted by the Chicago Congress, 1·434 volts, is too large by about 1 millivolt; and in addition, several redeterminations of the mechanical equivalent of heat in electrical units give values for the latter which can only be brought into accord with the values determined by the direct mechanical methods if electromotive force of the Clark cell be taken as 1·433. If this value be adopted for the Clark cell, or the equivalent value for the Weston cell, the international units would be defined with a quite sufficient absolute accuracy, as the above value is most probably known to at least one

part in 2,000, and as at the present time a much higher absolute accuracy can hardly be predicted. It seems, on the other hand, that the main question is to define the international units with the prime object of *reproducibility* to the highest order of accuracy, and it is hoped that in this an accuracy of a few parts in 100,000 will be realised.

Derived Units.

It will be generally agreed that units of capacity, inductance, power, energy, and any others that the St. Louis Congress may decide to include, should be defined in terms of the definitions adopted for the fundamental units.

The joule and watt have, however, been defined in terms of the c.g.s. units by some countries, and objections will probably be raised to defining them in terms of the electrical units. Such objections could be met by making a distinction between the absolute joule and international joule, and the absolute watt and the international watt—a distinction already used to some extent in distinguishing between the absolute units and the international electrical units. As the system becomes established the designation international will gradually be dropped. Moreover, if the values adopted for the international units agree with the absolute values upon which they are based to within even one part in 1,000, as will be the case, the objections will be mainly theoretical, as all practical requirements will be met.

Magnetic Units.

The only official action thus far taken in defining the magnetic units is that of the Paris Congress of 1900, by which the c.g.s. units of magnetic field intensity and magnetic flux were adopted, the names Gauss and Maxwell being assigned to them.

The St. Louis Congress may, however, consider the adoption of additional units of magnetomotive force and magnetic reluctance and the definition of all the magnetic units in harmony with the practical system of electromagnetic units. None of the units, either of the practical or c.g.s. system, is of a convenient magnitude, but this should, of course, not determine the choice. If there is any need of decimal multiples or sub-multiples, these can be supplied by the use of a suitable prefix.

It must, however, be emphasised that the definition of the magnetic units directly in terms of the c.g.s. units, or in terms of multiples or sub-multiples of the same, is open to the serious objection that it would lead to an inconsistency if the fundamental electrical units are defined in terms of concrete standards, and that it would be equivalent to a redefinition of these electrical units, interconnected as they are with the magnetic units, in terms of the units of the c.g.s. system.

This can only be avoided by defining the magnetic units in terms of the fundamental international electrical units—the ohm, volt, and ampere—and any resulting ambiguity might be removed by designating the units thus defined as international magnetic units.

DISCUSSION.

Professor Nichols.

Professor E. L. Nichols (Chairman of the Section): As the points brought out by the preceding papers touch very directly upon the subject of the discussion which is to follow, I will not ask for a discussion on the papers individually, but on the subject of Units and Systems generally until the hour for adjournment. Any remarks upon this subject are now in order.

Dr. Glazebrook.

Dr. R. T. Glazebrook: I ask to be permitted to speak, though I confess I find it a somewhat difficult task because the subject is large and important, and at the same time the proposition before us is somewhat vague in character, and necessarily indefinite. By way of preamble, I think I ought to state to the Congress that while I am here in the honourable position of a delegate from Great Britain, I and my colleagues are only authorised to adhere to any decision the Congress may come to personally, and such decision is not of course binding on His Majesty's Government, but must be referred to the proper authorities in England for their consideration and decision.

To turn, however, to the subject definitely before us, that of Electrical Units. I take it that in Professor Wolff's able paper there are two main points which he has opened for discussion. One of these is the question whether, either now or at some future date, the Clark cell is to be replaced by the Weston cell; and the other is perhaps the larger and more important question as to whether, as one of our fundamental definitions, we ought to replace the definition of the ampere by a definition of the volt, because, as it has been pointed out to us several times this morning, the three, the ohm, the ampere, and the volt, are connected together, and it is important that at any rate within practical limits, the three definitions should be consistent definitions.

Now, I think some slight difficulty has arisen as a consequence of the form of definition adopted by the Board of Trade in England, and perhaps I may clear the ground a little if I explain the position with regard to these three definitions. I think it was felt, when they were settled, that it was of great importance to adhere to the C.G.S. system of units, and therefore that the definitions adopted should be distinctly based on the C.G.S. system. So, turning to the legal definitions in Great Britain, the preamble of the order in Council recites the C.G.S. definitions of the ohm, the ampere, and the volt as the fundamental definitions upon which the standards to be used are to be based. Then it was quite clear from the discussion which had taken place in Edinburgh a year previously, when we had the great advantage of the presence of Dr. von Helmholtz, and representatives as well of this country—it was clear that we must introduce in some way into the definitions the mercurial resistance, the ampere as measured by the silver deposited, and some form of standard cell, but at the same time we were informed by the legal advisory authorities that it was necessary to have concrete standards to represent each of these quantities defined. Therefore, although the mercurial resistance and the electro-chemical equivalent of silver, and the Clark cell, are referred to in the preamble of the law, the schedule, which is the effective portion of the law,

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reads, "Now, therefore, Her Majesty, by virtue of the power vested in her by the said Act, by and with the advice of her Privy Council, is pleased to approve the several denominations of standards set forth in the schedule hereto as new denominations of standards for electrical measurement."

These, then, are the legal standards for Great Britain, and these are (1) a wire coil of platinum silver, which was measured by myself, and which has since been preserved at the Board of Trade; (2) a certain standard ampere balance, which was constructed under the supervision of the Committee advising the Board of Trade, and was calibrated not by any absolute measurement, but by comparison with the ampere, as given by the electrochemical equivalent of silver; and (3) since we were told there must be concrete standards for all the units, the third standard is a voltmeter of special type devised and constructed under Lord Kelvin's personal supervision. This will perhaps explain how it is that while very often the units depending on the mercury resistance, the electro-chemical equivalent of silver, and the Clark cell are referred to as British units, still, strictly, the standards recognised by law are these three instruments which now exist and have been carefully preserved by our Board of Trade.

Turning now to the proposals: The first one is, I take it, a proposal of some kind to substitute the Weston cell for the Clark cell. I think no one who has worked at the subject will differ from me when I express the opinion that for practical purposes, the Weston cell is superior to the Clark cell. I don't know that I agree entirely with all that Dr. Wolff has said, but in the main I agree, and should accept that position. At the same time, it is a very serious matter to change a unit which is more or less legalised in a number of countries, and I feel that I should have considerable difficulty in going to our authorities in England and asking for this change, unless I could speak very positively indeed as to the value to be assigned to the electromotive force of the Weston cell, and as to the advantages that would follow from the adoption of the cell, and further as to the exact specification to be adopted for making the cell. It is clear, I think, that the specification, which was adopted by the Board of Trade, and which was followed here in this country after the Chicago conference with some modifications, as to the construction of the Clark cell, is at fault. Dr. Wolff has indicated that this is so, and Professor Carhart, I believe, agrees with him. At the same time, I am not clear that either Dr. Wolff or Professor Carhart would be prepared at the present moment to draw up a specification for a Weston cell which either would look upon as completely satisfactory. I think they could do it in no very long time.

In my own laboratory for some months past a series of experiments have been made on the various defects or causes of defect that arise in the Weston cell or in the Clark cell, and in the main we have arrived at the same results as those obtained by Professor Carhart. It ought, I think, to be pointed out that Lord Rayleigh in one of his early papers referred to the mercurous sulphate as being the chief source of error in the cell. I don't think that has been sufficiently recognised either in England or elsewhere lately. I think it will be found that is the

Dr. Glazebrook.

case, and for this reason it seemed desirable to attempt in the first place to obtain a proper mercurous sulphate. I am glad to say (I am not going into details) that Mr. F. E. Smith has succeeded in my laboratory in getting samples of mercurous sulphate which give us, if not absolutely, to within a few millionths of a volt, consistent values, by three distinct methods. It seems to me to be important that we should make the sulphate not merely by one method and get consistent results, but that we should vary our methods. One method is the method of fuming sulphuric acid; another is that referred to by Professor Carhart and Dr. Wolff. The third method is described in a paper here at the disposal of any gentleman especially interested. I think that we may claim that the rôle of the mercurous sulphate is satisfactorily established, and difficulties arising from it have been satisfactorily overcome. I may say that cells prepared by these methods agree to within a few millionths of a volt. I have a table of values here, showing the electromotive forces of some of the cells; in the first case within three minutes of being put together, then when they were half an hour old, and then after some hours, and they only differ by quite trifling amounts.

But this result has shown us something else of importance. We have in the laboratory a considerable series of cells put up some three years ago with mercurous sulphate, as carefully prepared as we knew how to prepare them, and, as far as our tests go, the electromotive forces of these cells have remained practically constant since that date. They agree extraordinarily well with some of the well-known standards, but they differ appreciably from the electromotive forces of cells put up by the new method of preparing sulphate. Now that is a matter that perhaps needs some further investigation. It occurred to me yesterday that something of that kind might easily explain some of the discrepancies pointed out by Professor Barnes in his paper.

Then, again, what is the electromotive force of the Weston cell? Dr. Carhart has described to us his method of determining its value. I am afraid I failed to catch his last words. Has he completed the work, and is he prepared to give the value? I gather that he is not. With regard to the method, I should say that I feel a little doubtful as to whether the electrodynamometer method, depending on the torsion of a wire, and involving, as the formula shows, the square of the radius of the small coil, is as likely to give us as accurate a result as the method described by Lord Rayleigh, in which only the ratio of the radius of the small coil to that of the large coil comes into consideration. However, be that as it may, an apparatus designed by Professors Ayrton and Jones is now in course of construction in my own laboratory. It has not proceeded very far, and it is rash to prophesy, but Professor Ayrton hopes in a few months to be able to give to the world the results of the observations made with that instrument. By that time I should suppose we might have come to some agreement as to the absolute value of the electromotive force of a cell, and as to the specification for putting up the same, not merely with regard to the mercurous sulphate, but in regard to the other ingredients, which do exercise a certain small influence which is now being investigated in

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my own laboratory and elsewhere. Therefore, I shall not advocate the Congress coming to any resolution at the present moment in favour of actually now making a change, though I shall be quite prepared to support a proposal at the proper time, when these factors are better known.

I have the honour to represent here the Institution of Electrical Engineers, among other British institutions, and also the Electrical Standards Committee of the British Association, which took these various matters into careful consideration at their last meeting, and the resolution which was then passed I will now read to the Congress:

> "The Committee is not prepared at present to displace the Clark cell, and prefer to wait for the conclusion of the Experiments at the National Physical Laboratory, and with the new balance, before coming to a decision as to the value to be assigned to the E.M.F. of the cadmium cell.
>
> "With regard to the choice of magnetic units the Committee is of the opinion that the only two systems which need to be considered are the C.G.S. system and the Ampere-Volt-Ohm system, and that the quantities to be named of any are (1) magnetic potential, (2) magnetic flux, (3) magnetic reluctance. Of the above two alternatives the Committee is in favour of the C.G.S. system as that on which to base any nomenclature of magnetic units, but is of the opinion that a system of nomenclature is not called for."

So much for the first question—that is, the question as to whether the Clark cell is to be replaced by the Weston cell. The second question appears to me to be one of greater difficulty. I think Colonel Crompton, who is with us, is to be congratulated on the general acceptance of the method he has established, and which he has pressed in season and out of season for so many years. But I am not clear that it follows that we ought to make the definition of the volt rather than that of the ampere the second fundamental definition. I should like to go back, for these fundamental definitions, to simple facts as far as possible, and it seems to me that the fact which I take to be one, that the chemical changes which go on in the silver voltameter are simpler and more readily understood and, I think, more easily controlled than those which go on in the cell, might, at any rate, make us hesitate before we accept the proposal to replace the silver voltameter by the cell as our fundamental standard. I am confirmed in this contention, because I understand from some correspondence which I have had with President Kohlrausch as to the German legalised standards, and, I should add, from a paper recently published by the Reichsanstalt, that the German authorities adhere to the belief that the ampere as defined by the electrochemical equivalent of silver should be retained as a second fundamental definition. I gather, also, from what I know of Professor Mascart's views that he thinks likewise. I do not see the practical advantages which would follow from the change, and I do not think it is so important as to make it desirable for us to support it, or to urge it, at any rate at the present juncture.

Dr. Glazebrooke.

I hope I have made clear to the Congress my own views as to this very important proposition, and I trust I may be allowed to express my thanks to Dr. Wolff for the extremely interesting and valuable paper which he has laid before the Congress, and to Professor Carhart for what he has done at the Congress and previous to it, to enlighten and clear up the points before us for discussion.

Dr. Webster.

Dr. A. G. WEBSTER: Mr. Chairman, I should like to support and express my hearty agreement with all that Dr. Glazebrook has said from the practical point of view, to which he has exclusively confined himself, and, lest his remarks should be supposed to be tinctured with English conservatism, I should like to make, from my own point of view, a few remarks from a practical standpoint. This is a practical country. The question is, when we have adopted the changes suggested, what good do we get from them? Now, these laws of the different countries, as Dr. Wolff has shown us, differ somewhat. How much do they differ? Practically less, probably, than one part in one thousand. Now, I am very sure that I do no insult to any electrical engineer here present when I say that very few engineering measurements of any sort are at all influenced by an amount of one part in a thousand. Another thing —the next point. Suppose that the laws are to be changed. We are to have a certain cell, and a certain piece of mercury and a certain amount of silver deposit, or else we are to have three standard instruments, as have been adopted in Great Britain. It seems to us that we could not use these instruments universally, however good they might be. We may make laws specifying the amounts of silver, or of mercury, or of what not; but, after all, what do our specifications and our laws depend upon? They depend upon certain perfectly ideal things, namely, the definitions of the C.G.S. units. These things, it seems to me, must be followed for definitions, and it seems to me that the method adopted in the United States laws, if I am not mistaken, and practically in the English laws, is that the units are C.G.S. units and are represented *for practical purposes* at the present time, that is until the next change of the law, by so-and-so.

Suppose you define them to-day in terms of mercury and silver. Do you mean to say that in twenty-five years we shall not have one or two more figures? It seems to me that it would put us in a most untenable position. Now, I firmly believe, and I am sure that everybody who knows the work that has been done by the German Reichsanstalt, by Dr. Kahle on the silver voltameter, by means of the electrodynamometer of Professor von Helmholtz, and in the British National Physical Laboratory under Dr. Glazebrook—and this work is going on with greater rapidity now than ever before—will see that in the next five or ten years they can confidently expect the next figure on the Weston cell, the next on the ampere in silver, and I do not suppose we can expect another figure on the ohm for some years. One point more. Dr. Wolff has spoken of the analogy between the practical definition of the metre and these standards. This is an unfair analogy. The metre is a fundamental standard. You cannot redefine the metre, because that would throw out every measurement of every length and everything depending

Dr. Webster.

upon lengths ever made. We can say the earth is now supposed to be so many metres around. The metre is a definite thing. There it is, and there is the kilogram; that is one and indivisible. The second is derived from the motion of the earth. You cannot make standards of these any better; you cannot redefine them. But these electrical units are quite different. They are not a bit like a metre. Can you make a standard ampere, bring it here, and say, "There is an ampere"? You cannot do it with a volt; you cannot do it with an ohm. But it is different with a metre bar. It seems to me, therefore, that the British conservatism voiced by Dr. Glazebrook is the best course to be adopted by the International Congress. In five years or ten years your Government would have to change your laws, for it would satisfy you no better. I do not believe any electrical engineer will ever be satisfied any better by the suggested laws than by the laws we have now. The only laws fixed are the laws of nature.

Professor Carhart

Professor H. S. CARHART: I want to preface what I have to say by remarking that the province of the official delegates from this country, of which I have the honour to be one, is not to make laws at this Congress, or to determine anything for our Government, but only to make recommendations. It agrees, therefore, precisely with the province of the delegates from Great Britain. And, moreover, we have nothing of a revolutionary character, I may say, in mind; but we have thought that it would be very useful if this International Congress could discuss these questions, and if out of these discussions comes ultimately greater uniformity in the definition of units and electrical standards than exists at the present time.

I am quite willing, therefore, to agree with what Dr. Glazebrook has said with respect to the definitions of the units, or with respect to the volt, particularly as outlined in the first part of his remarks. It may be well to emphasise a little the fact that standard cells, either of the Weston normal type or of the Clark type, can now be made with greater uniformity, and show greater consistency than has been possible until within the past year; and I am quite convinced that the very strong adherence shown to the silver voltameter by some of our European friends is based upon difficulties encountered in the past in the reproducibility of standard cells. Fortunately now all over the world the difficulty has been traced to the mercurous sulphate. I should like to emphasise that point a little, because Dr. Hulett and I have found in our investigations that we can contaminate the solution of cadmium sulphate with 1 per cent. of zinc sulphate solution without affecting the electromotive force at all. We can contaminate the cadmium amalgam by using one-hundredth part of zinc to 99–100 parts of cadmium, with a difference in the result of perhaps not more than five in one hundred thousand; so that the difficulties on the score of contamination by zinc, which is the impurity to be feared, are almost inappreciable. Also, I think we have devised a method of preparing the amalgam which obviates the difficulties of oxidation. We have now a standard method of preparing the cadmium amalgam and zinc amalgam, and I believe the difficulties in the way of the preparation of the mercurous sulphate have also been removed. Either

Professor Carhart.

our method or one of the methods tried at the National Physical Laboratory in London will be satisfactory. No simpler method than the electrolytic method could be devised if proper precautions are taken. With cells set up in accordance with the method of preparation to be outlined in my paper in Section C on Thursday, we can be sure of an agreement to within one part in ten thousand; and my colleague, Dr. Hulett—who unfortunately is not here on account of sudden illness—believes we can be sure within five parts in one hundred thousand. I believe we shall all agree that this is quite near enough for practical purposes. The difficulties, therefore, in the reproducibility of the Weston normal cell, with a saturated solution, have been removed. I am quite willing, however, to wait for the results of the investigations now in progress at the National Physical Laboratory relating to the electromotive force of the Weston normal cell, as we have agreed to call it. Perhaps the National Bureau of Standards at Washington will also undertake to measure this electromotive force; and if we all get concordant results in three different places, I hope every one will be prepared to accept these results for practical and possibly for legal purposes.

Now, with respect to the other point raised very properly by Dr. Glazebrook, as to which of the two electrical units we shall employ as fundamental, though they are both derived units, for purposes of electrical measurement. I do not find myself in accord with his position, and I shall endeavour to give my reasons. We are all agreed to take the ohm as one of them. On that point there is no difference of opinion or practice; but I hope we may come within a short period to an agreement to adopt the volt as the second of these units rather than the ampere; and there are several reasons why we should employ the standard cell for these purposes rather than the silver voltameter. We wish to have concrete standards. It certainly is easier to have concrete standards of electromotive force than it is to produce a concrete standard for an ampere. You cannot keep an ampere locked up in a case; and while you may represent it by an ampere balance, it is only indirectly, after all, a standard ampere. The silver voltameter does not measure current directly at all, and any one who has attempted to keep a current constant, as indicated, for example, by a potentiometer, for half an hour, will agree with me that it is at present utterly impossible to maintain an absolutely constant current for five minutes. So the ampere as derived from a silver voltameter is only an inference, which all of our friends must admit. Now can we establish the electrochemical equivalent of silver with sufficient accuracy? I doubt very much if it is known within one part in one thousand, so as to agree with our ideal definition. If it is not known, the scientific man will never be satisfied until he knows it to a nearer approximation than it is known at present; and even though it might be sufficient for electrical engineering to allow it to stand as it is, it will not be sufficient, I am sure, for Dr. Glazebrook and Dr. Stratton and for others interested in these subjects. We must pursue this subject if we are true to the scientific doctrines that we believe in. We do not know the electromotive force of the standard cell to within something like

one part in one thousand. Then the question is, how shall we better determine these constants? Now, it has been admitted, and no less freely by our German friends than others, that the practical method of measuring currents is by the potentiometer and standard resistances. Everybody thinks that we must use this method in order to have a wide range of accurate measurements. We are all agreed then that we are going to use a standard cell, whether we define our legal volt from it or not. Now then, if we admit the silver voltameter as our second fundamental unit, we must determine the electrochemical equivalent of silver by means of some ampere balance, such as is now being constructed in the National Physical Laboratory for example, or by some form of electrodynamometer, such as my colleague and I are using, and then we must proceed from that by the use of the silver voltameter, and by a second step, to determine the electromotive force of the standard cell. That is the method that has been adopted in the Reichsanstalt, a method which I am sure you must admit is not above criticism, if you have examined the exact process by which the electromotive force of the Clark cell was determined. I am not disposed to say that this was not the best method that could be used at the time; but I do not think it is the best method possible now. Professor Carhart.

Instead of arriving at the electromotive force of the standard cell by a second process in which all the difficulties of the silver voltameter are introduced, on top of the difficulties in measuring current in absolute units, why not proceed directly to the determination of the electromotive force of the standard cell without the intervention of the silver voltameter at all? We should then have the standard ohm, upon which we all agree, and we should determine the electromotive force of the standard cell by absolute measurement with some form of ampere balance in exactly the same manner that we determine the electrochemical equivalent of silver. Why not eliminate this intermediate step of determining the electrochemical equivalent of silver, in order to arrive at a practical current-measuring device? I do not see how the argument can be avoided. When we have reached an agreement as to the specifications for setting up the Weston normal cell, it will be much easier and simpler to proceed directly to measure its electromotive force by means of an absolute ampere balance, and to say, "That is one of our fundamental standards," rather than to pass through the electrochemical equivalent of silver, and then to the electromotive force of the standard cell, and to a practical method of measuring current.

Mr. H. E. HARRISON: I can add little or nothing to what has been said by Dr. Glazebrook about the standard cells, but the subject of the units has also to be discussed. This subject we have also considered carefully. The C.G.S. system is used too universally to be readily changed. It would be useless to tinker with it; it must be kept as it is, or it must be scrapped entirely. The only justification for scrapping it would be the introduction of a new system which had none of the defects of the old. Mr. Harrison.

For example, there is the fundamental difficulty of the 4π, discussed by Dr. Kennelly in his paper, and again referred to by Professor Ascoli

Mr. Harrison. this morning. If 4π is made to disappear in one place it crops up in another. We do not see how to get rid of it. We can only look to our more inventive cousins on this side to devise a sphere whose surface shall *not* be 4π times its radius squared.

Again, Dr. Kennelly suggests that the absolute units shall be named, because all germs and even weeds have names. I differ from him. A noxious germ is not used because it is named. But the naming of the two absolute systems would cause them to be used so that when reading an electrical paper we should frequently be in doubt as to which of three units was meant. The advisability of naming the magnetic units will, I believe, be discussed in another place. Up to the present very little difficulty seems to have arisen in their use.

I would remind you that at the Paris Congress of 1900, names were given to two of these units; yet in England, at all events, only about one in a hundred engineers would know the meaning of the Gauss or the Maxwell.

Personally I should like to see these units named, but if engineers will not use these names already given, it seems useless to name others or to change those already given.

Dr. Kennelly. Dr. A. E. Kennelly: There are two distinct questions involved in this discussion which should be dealt with separately. The first is the volt question, and the second is the unit question.

In regard to the volt question, I speak from the standard of the practical man. In various countries, I believe, and certainly in this country, the Clark cell is legalised as 1·434 volts at 15° C. I believe it is generally admitted, not only in this country, but also in England, in Germany, and in France, that a closer approximation of the electromotive force of the Clark cell is 1·433, and that, to the best of our knowledge to-day, 1·433 is the electromotive force of the Clark cell at standard temperature. That means that, to the best of our knowledge, we are inaccurate in our legal value by, roughly, one-tenth of 1 per cent., or, roughly, one part in a thousand. That does not affect some countries, because some countries have not legalised their Clark cell. They have legalised the ampere and the ohm. In Germany, for example, I understand that the Clark cell is taken at 1·4328 volts at standard temperature. But we all use Clark cells, and in every practical commercial laboratory the Clark cell is the basis of measurement. Only in the best standard national laboratories is the standard ampere the basis of reference.

Now it is a matter of importance from a practical standpoint, from an engineering standpoint, that there should be a discrepancy, between our best knowledge and our laws, of nearly one part in one thousand with the practical working standard that we all employ and shall continue to employ. If we maintain that condition of affairs unchanged, the result will be that we shall either infringe the law by employing our best knowledge in our practical work, or, if we follow the law, we shall be acting in opposition to the best of our practical knowledge. One part in a thousand is by no means unimportant, because one-tenth of a volt has an appreciable effect upon the lifetime of an incandescent lamp, and contracts based upon voltage and life-

Dr. Kennelly.

time of incandescent lamps are liable to be appreciably affected by an error of nearly one-tenth of a volt in 120 volts, which is distinctly discernible on a Weston Laboratory standard voltmeter.

This is a discrepancy that should be eliminated in some way. There is difficulty in doing this internationally, because of the difference of laws in different countries, and we should have some method of overstepping this difficulty.

There are two proposals. One is to abolish the Chicago recommendation of 1·434 volts by substituting a new standard, the cadmium cell or Weston normal cell. That would remove, of course, the 1·434 and its discrepancy. This course would be advantageous if it could be followed. If this course could not be followed, the next best thing would be to have the 1·434 changed somehow to 1·433 for the present. That would at least bring our best practical knowledge into conformity with working standards and with national law. Moreover, with an error of one in 1,000 for the standard cell, an error of one in 500 is introduced into power measurements. This error of merely one part in 1,000 becomes one-fifth of 1 per cent. when power is involved, depending upon the electromotive force of the Clark cell or its derivatives. It would surely be unworthy of this International Electrical Congress if, agreeing that 1·433 was the best value for the electromotive force of the Clark cell, it should leave the legalised existing value of 1·434 untouched in their recommendations.

The question of whether the volt standard should take the place of the ampere standard is more serious, because it affects the legislation of different countries. I agree with Dr. Carhart and a number of the other gentlemen here that the concrete volt standard, since it can be carried about like an ink-bottle, is better than the fundamental ampere standard, which must be set up under very careful conditions in a standard National laboratory. For this and other reasons the combination of the standard volt and standard ohm would be better as concrete standards than the standard ampere and the standard ohm. We have to consider, however, the requirements of the laws adopted in other countries. It might not be practical to make a change of that sort at the present time. But whatever the conditions may be in regard to the legislation of the different countries, we should all try to get as closely together as possible. I believe that when some countries take the Clark cell as 1·433 volts and others take the Clark cell as 1·434, the discrepancy is one which affects commercial and practical interests to a small but appreciable extent, and, if only provisionally, should be overcome.

Now as to units in general. The first question naturally arising in connection with that subject is as to whether the units should be rationalised or not. There is much to be said on that question. It is a large subject, and there is much literature upon it. My personal opinion would be that we had better leave well-enough alone at the present time, and not try to upheave all of our standards, our ohms, volts, and amperes, for the sake of a 4π. Whatever may be the outcome in the future, I believe it almost impossible at this time to upheave legislation and universal practical applications by changing

Dr. Kennelly.

units from irrational to a rational basis, however much the rational basis might be desired. So I will, with your permission, pass that question by and come to the actual C.G.S. units as they stand, and assume that it is our desire to maintain and to perpetuate these units. In the scientific world generally, in chemistry and mechanics, etc., all units are coming more and more, I believe, to be founded upon the C.G.S. system. Consequently it would not be desirable for us to draw the electrical branch out and put it apart from the C.G.S. system used in the other divisions of the scientific world.

We learn the electrostatic and electromagnetic C.G.S. systems, and having learned therein the relation of magnetic and electrical units to each other, we are told by our teachers that this subject is altogether for the priesthood—that it is too good for us, and that we must content ourselves to work with certain derivatives of these units, in which the ohm is 10^9, and the ampere 10^1, and the volt 10^8. This is a great misfortune. It is true we must be content with the practical units. No one urges that we should try to upset the ampere, ohm, and volt. We owe a great deal to the gentlemen who established the ohm, ampere, and volt, and I would do nothing to depreciate the magnitude and importance of their work, which must stand for the present. I do not for one moment advocate the upsetting of the practical system, because it has come to stay, and all our instruments are marked therein. It is good enough for most purposes. The plea I put forward is that we are hampered in the development of electrical knowledge—hampered in the development of electrical theory and in the apprehension of electrical phenomena, by carrying on trains of thought in this artificial system—artificial because in this system the unit of length is approximately the length of a quadrant of the earth, and the unit mass is an extremely small subdecimal of a gramme.

The difficulty which everybody must have encountered in dealing with new electrical applications of any kind is so serious that hardly any one will advocate carrying out a long process of reasoning in the practical system. In such cases every one will naturally fall back upon the C.G.S. system. It seems only reasonable that fundamental units which have to be used, at least in theoretical investigations, should receive names, and perhaps the simplest method of naming these units is to employ prefixes in connection with the practical units.

Professor Perry.

Professor J. PERRY: Dr. Glazebrook explained his views to a class of people interested in this subject before leaving England, and I think I may say that pretty well everybody in England is in agreement with him. I think also that most of the people I have talked with on this subject would be in perfect agreement with Dr. Glazebrook, and would have been delighted to hear the remarks of Dr. Webster.

I think our position ought to be that there should be as few concrete units as possible. Can we base everything upon the three—the centimetre, the gramme, and the second? Are all others derived from them? If so, as times go on, should we not be able to define these units with greater and greater accuracy? Do you mean to say, if we settle on 1·434, for example, that possibly twenty-five, fifty, or one hundred years hence we should not want to alter these numbers?

Professor Perry.

Now, Prof. Carhart's whole argument seemed to me to be based on the idea that we must have the ohm, the ampere, and the volt, all concrete illustrations, defined by law. I differ from him here altogether. We have the three fundamental concrete examples, and regard all others as derived from them. We say 10^9 C.G.S. units of resistance make an ohm, 10^8 C.G.S. of electromotive force make a volt, and one-tenth of a C.G.S. unit of current is an ampere. We say that these are derived, and, as time goes on, will be more and more accurately derived, from the C.G.S. system. We can and do put forward three concrete illustrations as giving for the time the very best examples of the three definitions. As close as we can get now we give the numbers which enable people to follow our instructions in preparing examples of these derived units. Dr. Kennelly's remarks illustrate, I think, what I would call the British point of view. 1·434 has been settled upon by the United States. It ought to be the easiest thing in the world to alter 1·434; but as part of the law system of the United States at the present time is that it must be 1·434, you cannot, without altering the law, use a better approximation. You are going now to make it 1·433 by a change in the law of the country, and in the course of a few years you will want to alter it to something else. I think, on the whole, we have been wiser in England, basing everything on the C.G.S. system, than you have been in America, where the C.G.S. system is not referred to at all.

I am, and always have been, in favour of giving up that wretched 4π. I know perfectly well that one hundred years hence everybody will be blaming us because we did not take the bull by the horns. Face that difficulty and get rid at once of it, even now. I am in favour of the change being made now. I think that, sooner or later, it will be done.

I was one of the first—probably the first—to point out the importance of our having magnetic units agreeing with the volt and ampere and ohm. I might therefore be supposed to be prejudiced in favour of that system. I may say that I have completely given up the idea that it is good to have that system. In my practical work I have always found that it is easy, not troublesome, not giving rise to any kind of complication, to convert everything into C.G.S. units. It strikes me that a number of people in this country are too logical. They have the ohm, the ampere, and the volt, exceedingly convenient, I believe, in spite of what has been said, exceedingly convenient units, but they want to make a complete set of units to correspond. I think it is a mistake. I think we always ought to look at the C.G.S. units as our real units, and that those convenient units, the ohm, the ampere, the volt, and the henry, are sufficient.

Now as to this other point about names. Is not Dr. Kennelly asking for far too many names? I am not quite sure of what a man means when he talks of so many minimums, and so many maximums. If you are going to give names to the C.G.S. units, is there anybody who will say that they are necessary? Would they even be convenient? I think not. Anybody knows the C.G.S. units. I am in perfect agreement with Dr. Glazebrook and Professor Webster.

Dr. Wolff.

Dr. F. A. WOLFF: Mr. Chairman, in the main, I agree with those who have participated in the discussion as to the desirability of not taking any radical action at the present time. At the close of my paper I expressed the hope that this Congress would give a definite expression to its opinions, which might form a basis for further discussion, and eventually lead to international agreement, so that all nations might have exactly the same laws.

Now, if any one will take the trouble to examine the present laws* he will see that there are numerous points of difference. I am quite sure that our English friends will agree that it is not wise to define a unit in a number of different ways. It certainly must lead to inconsistencies. For example, if the volt is defined in terms of the ohm and the ampere, or in terms of the C.G.S. units, it will not be in agreement with the value 1·434 of the Clark cell, which is legalised in various countries. I hope that the result of the deliberations of this Congress will be a set of resolutions expressing the views of the Congress, and some arrangement for a future international assemblage at which some final action can be taken.

I cannot quite understand the attitude of some of the gentlemen who prefer the C.G.S. units (which I will admit are the best theoretically) to practical units defined in terms of concrete standards. It seems to me that if we have reproducible, concrete standards, and determine their values as well as we can by absolute measurements in C.G.S. units, it enables every one who has a Clark cell and a standard ohm to express all his measurements in such units to as close an approximation as he possibly can. In other words, Professor Webster would find that the simplest method for determining the value of a quantity in absolute units is by comparison with concrete standards in terms of which the legal units should also be defined. Then again, the question has been raised as to whether it would not be necessary to modify the values adopted for such concrete standards, as new absolute measurements are made. I think that is answered pretty well by the fact that it has not been found necessary to modify the value of the ohm, and in fact I do not think that any one now thinks of suggesting a modification. Suppose we should even find that a number of absolute determinations all agreed in the value 106·28 cm. for 10^9 C.G.S. units of resistance. I do not think it would even then be necessary to make any change, because measurements might still be expressed in terms of 106·3 cm., and in the few cases where they would have to be reduced to absolute measure the correction could be applied.

There is this same question in relation to the cubic decimetre and the litre. I do not think we are very seriously hampered by the fact that the latter is our practical standard of capacity, and not the cubic decimetre. In other words, the suggestion that it would be necessary entirely to surrender the C.G.S. system is answered, as definite reproducible concrete standards furnish also the best means of carrying out measurements in the C.G.S. system.

It was asserted that it would be best to accept a standard which is

* See Bulletin, Bureau of Standards, No. 1.

based on the fundamental laws of nature. I think that was Professor Webster's argument for the silver voltameter; but is not the electromotive difference between the metal and the salt of the metal just as fundamental as the electrochemical equivalent of silver? If methods can be devised for obtaining materials sufficiently pure, we can be sure that they have uniform electromotive properties, and recent work shows that such is the case for the materials employed in the standard cell. Dr. Wolff.

In conclusion, I might say that my stand is that the laws we now have differ so radically that I think all nations would agree to their repeal and to the substitution of a new law which has the support of all.

Professor H. S. CARHART: I do not want to take up the time of the section except for a few minutes. I am sorry that Professor Perry has gone out. I rise particularly to compare the definitions in the laws of the United States and those adopted in Great Britain. I fail to see the difference. The language is slightly different, but they are essentially the same, and I wish to say that I do not include now in my remarks the Schedule of Standards one, two, and three. Those, I suppose, are peculiarly characteristic of the snug British Islands, but with them we have nothing to do. They are concrete standards that they wish to preserve in the Board of Trade. I refer to the definitions preceding the Schedule of Standards. Now, in the United States laws we have defined the terms in C.G.S. units. You will find them in the first two, that is the unit of resistance and the unit of current, but not in the third, the unit of electromotive force; we followed the recommendations of the Congress of 1893 in saying that these were represented *substantially, sufficiently for practical purposes*, by such and such concrete standards. We did not assume or assert that there was an exact agreement between the theoretical definitions and the practical units. Now, what do you find in the definitions in the laws of Great Britain? You find, first, the ohm 10^9, expressed in slightly different language, but exactly the same thing. You will find it is represented—not substantially, but by the resistance offered, so-and-so. There is the other half of the definition, almost exactly as we have it. With respect to the ampere, we have again the same thing—the theoretical definition, and then the practical definition. We have them, also, in the British statement about the volt, and both of them we now know to be incorrect, as far as the practical definition is concerned, at least to one part in one thousand. For the volt, we have no theoretical definition in the laws of the United States; the British have stated it as 10^8 C.G.S. It is stated that this is represented by 0·6974 (1000/1434) of the electrical pressure at a temperature of 15° C. between the poles of the voltaic cell known as Clark's cell, set up in accordance with the specification appended hereto and marked "B." That is exactly equivalent to the statement of our law; both are wrong. I do not see that they adopted any different position from ours, except as to their standards, which I take it is a special and individual thing, and has nothing to do with these definitions. The positions they have taken and the positions we have taken are identical. Professor Carhart.

Dr. Glazebrook.

Dr. R. T. GLAZEBROOK: May I be permitted to ask a question? I do not see in the United States definition any statement that the ohm is 10^9 C.G.S. units of resistance. I do see that in the British statement, and that, I understand, is the difference between Professor Perry and Professor Carhart.

Professor Carhart.

Professor H. S. CARHART: "Which is substantially equal to one thousand million units of resistance of the C.G.S. system."

Dr. Glazebrook.

Dr. R. T. GLAZEBROOK: I see. I beg your pardon. There is practically no difference on that point. There is the point, however, that the actual British legal standards are the instruments described in the schedules and not the definitions. I think a legal question would turn on that schedule and not on the definitions.

May I mention the other fact which I omitted when speaking before? It is not a matter of argument at all. Just as I left England, I was handed by Mr. Trotter, of the Board of Trade laboratory, a note of a determination of the electromotive force of the Clark cell which he has completed. He passed a current through his ammeter and a standard resistance, and the value thus obtained for the electromotive force of the Clark cell is 1·4329.

Dr. Wolff.

Dr. F. A. WOLFF: One further remark. The law of the United States is as follows: "The unit of resistance shall be what is known as the international ohm, which is substantially equal to one thousand million units of resistance of the centimetre-gram-second system of electromagnetic units, and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice fourteen and four thousand five hundred and twenty-one ten thousandths grams in mass, of a constant cross-sectional area, and of the length of one hundred and six and three-tenths centimetres." The English law defines the unit of resistance as 10^9 in terms of the centimetre and the second of time, and defines it also as represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice 14·4521 grams in mass, of a constant cross-sectional area, and of a length of 106·3 centimetres. This is the main point I wish to emphasise.

Dr. Webster.

Dr. A. G. WEBSTER: I should like to inquire from Professor Kennelly and Dr. Glazebrook what the chances are in any court in England or in America where a decision would be taken involving one part in one thousand on quantities having to do with electrical engineering, and whether Professor Kennelly knows of any supply company in this country which maintains a voltage to one-tenth of a volt. I know of none, and I believe that such a suit at law is extremely unlikely to come up. I do not want it interpreted that I am in the least out of sympathy with the policy of defining certain units. But to close the discussion of Professor Wolff on the C.G.S. system as against the theoretical units, I should like to call the attention of the members present to the results of the adoption of that policy in the determination of a magnetic field. To think we could no longer swing a magnet in Gauss's old method! This dates back to the beginning of electric measurements. We could not swing a magnet and find out what its field was without looking back to determine the weight of mercury and of silver and of all that,

to make the correction on this determination of our result of this magnetic field. That is the most fundamental electric measurement made in any laboratory. Dr. Webster.

Dr. F. A. WOLFF: In answer to Professor Webster's last objection, it might be stated that the accuracy of magnetic measurements hardly exceeds, in most cases, one part in one thousand, and that we are fairly sure that the fundamental electrical units, even with our present knowledge, would be defined to that accuracy; so we would go on making our magnetic measurements by the time-honoured methods, and we would not need to think of the column of mercury corresponding to the ohm, or of the specifications for the standard cell. I think this sufficiently answers the last objection. If the values adopted for the concrete standards are in close enough agreement with the absolute units on which they are based, say one-twentieth per cent., I think there will be no further practical difficulties. Dr. Wolff.

Dr. A. G. WEBSTER: I think it is, Who will determine your volt? It is determined by somebody—Lord Rayleigh, or Dr. Kahle, or some one else. I am perfectly sure that is the case. If not, what is your volt, and what is the difference whether it is 1·424 or 1·428? Dr. Webster.

Mr. H. E. HARRISON: To throw some light on the legal side of the discussion, I may state that I have acted for some years as an inspector appointed by the Board of Trade. I have occasionally had to test meters under conditions such that I could not rely upon my own measurements to within much under 1 per cent., yet during the whole time I have so acted the results I have given have never been questioned in a court of law. Mr. Harrison.

Dr. C. H. SHARP: Regarding Professor Webster's remarks, I wish, in the first place, to call attention to the fact that these electrical units are things to be used. They are things to be used by the practical man. I must say that I am entirely out of sympathy with the hypothetical troubles of Professor Webster in swinging a magnet. If it should be necessary in this connection for him to consult his conversion tables, that can be no great hardship. It would be a lesser hardship than for the busy practitioner to be obliged to speculate as to the value of the volt used in calibrating an instrument or a lamp which he has imported from Germany. Dr. Sharp.

Furthermore, as regards the significance of the quantity of one-tenth of 1 per cent. in engineering, I think that Professor Webster's idea of the importance of such a quantity is quite erroneous. The quantity one-tenth of 1 per cent. is of commercial significance. Suppose, for instance, that a change of the fundamental standards were made, by which all the energy sold to consumers in this country were affected by a change of one-tenth of 1 per cent. in the standard of voltage. That corresponds to one part in 500 in the energy, and in summing up the cost to millions of consumers it becomes quite significant.

Professor Webster asks what company maintains its voltage constant to within one-tenth of 1 per cent. No company does this, but many companies maintain their standards correct within one-tenth of 1 per cent. by the use of potentiometers and standard cells.

Roughly speaking, one-fourth, perhaps more, of all incandescent

Dr. Sharp. lamps manufactured in this country are sold under contract specifications under which the price to be paid for the lamps is regulated within certain limits, according to the life and to the candle-hours value which they show, the latter value being determined by life-tests of representative lamps.

In making these tests of lamps, a work in which I am personally interested, and with which Dr. Kennelly is also perfectly familiar, we find it absolutely necessary to maintain our voltage correct within one-tenth of 1 per cent. We cannot maintain our pressure always within one-tenth of 1 per cent., but we can maintain our average pressure within one-tenth of 1 per cent., and keep all variations within extremely small limits. Now, a variation of one-tenth of 1 per cent. in average pressure makes a difference of roughly 2 per cent. in the candle-hours output of incandescent lamps, which difference might involve a forfeiture on the part of the selling company or of a premium to be paid by the consumer. Considering further that the total number of lamps so affected amounted to about 10,000,000 last year, it will be seen that a difference of one-tenth per cent. in standards has a decidedly commercial bearing in this field.

In our testing work we have had to face the question, "What is a volt?" We are obliged to say, in accordance with the specifications of the American law, that a volt is obtained from the Clark cell which has an electromotive force of 1·434 volts. I may say personally that I do not like to do this, because I believe it is not so. Moreover, we find that a photometer lamp imported from Germany, standardised, according to their laws, must be held at a different voltage from its marked voltage, because their volt is different from ours, and we have to take this into account in our measurements.

The standards which the practitioner requires must be in the first place concrete, such that he can take hold of, that he can transfer from one point to another; he wants standards which he can use directly, and not through the intermediary of secondary standards exclusively. He wants standards uniform throughout the world. As another consideration, if he can get standards which represent pretty closely multiples of the C.G.S. system of units, he is all the better pleased; but if they are a little bit off, he does not trouble himself greatly about that, as long as everybody else is using the same ones. Consequently I think it would mark a distinct step in advance as far as electrical engineering practice is concerned—and it must be remembered that the accuracy attainable in engineering practice has increased rapidly from year to year, and the accuracy demanded is increasing as well—that it would be a step in advance if we could abolish the silver voltameter, which few use practically, and, beside the standard ohm which we already have, adopt a standard cell, say the Weston normal cell, and secure the universal acceptation of a fixed value for its electromotive force. Then we would be on a uniform basis all round. It is very much to be hoped that some action will be taken at this Congress which will result in the course of a reasonable period of time in the adoption of some such system or its equivalent.

I think the point raised has been pretty well covered by Dr.

Kennelly. It is a question of the accuracy of the standard from which we work. What is the normal? We must have that correct. We do not have meters in this country that register accurately to the tenth of 1 per cent., but we do test them with standards of that degree of accuracy. A difference of that magnitude may be of importance under certain conditions. If, for instance, a meter is guaranteed accurate within 2 per cent., and on test its error is found to be nearly that amount, differences of one-tenth per cent. begin to count, and our standards must be accurate within that limit of error. Dr. Sharp.

Dr. G. W. PATTERSON: I wish to add a few words about the relative advantages of the standard cell versus the silver voltameter. I assume we all accept the present value of the ohm as the standard of resistance. To determine in absolute measure either the electrochemical equivalent of silver or the electromotive force of a cell, we use some form of absolute electrodynamometer, calling it a current balance if we will. To determine the electrochemical equivalent, we must manipulate the silver voltameter, observe the time and weigh the silver deposited. To determine the E.M.F., we must balance the cell's E.M.F. against the fall of potential of the absolutely determined current over a standard resistance. This balance can be obtained fully as accurately as the time of deposit of the silver could be measured, to say nothing about the difficulties of holding the current constant, manipulating the voltameter and weighing the deposit. In addition I believe the E.M.F. of the standard cell is more accurately reproducible than the deposit of silver in a voltameter. But the principal argument in favour of the standard cell is its greater convenience in the laboratory; for in making laboratory measurements the reverse process must be gone through. This involves an enormously greater time for each observation when using the voltameter, with no compensating advantage. Consequently I favour the standard cell over the silver voltameter. Dr. Patterson.

Mr. W. DUDDELL: There has been a lot of discussion on the legal position of the matter which I will leave entirely on one side. I will say nothing about it. I will say this, however, that I think we all agree that the primary standards are those of the C.G.S. system. These are our primary standards, and therefore any other concrete standards set up for any purpose whatever are secondary standards. These secondary standards are required to have special features to make them useful for international interchange, so that we can make sure that all the nations of the world are working in the same standards in their practical work. It is evident that these secondary standards will always be more or less at variance with the C.G.S. standards, for, as we work, year after year, we are getting more perfect apparatus for determining true values, and we shall find errors in our secondary standards. It seems to me, therefore, that it would be unwise to tie ourselves too tightly to these secondary standards. Mr. Duddell.

The next question that arises is that the secondary standards must be easily reproducible, and they must be permanent. I look upon permanency as more important than the quality of being easily reproduced, as I think these secondary standards will be mainly kept by the large laboratories of the world which all nations are setting up.

Mr. Duddell.

Then there is the third set of standards, which seems to have become confounded with the second. Such as the cell—an apparatus which is going to be carried about for practical purposes. The accuracy of these tertiary standards need not be very high. One half of 1 per cent. is a pretty high accuracy for commercial purposes. An accuracy of one-twentieth of 1 per cent. will satisfy all practical ends.

Among secondary standards is the mercury ohm. Nobody is going to suggest that we take a mercury ohm into a workshop. We make a tertiary standard, say a manganin coil, from it, which we carry about and use. The same will apply to the question of the ampere and the volt. It rather looks as though we should finally end with some form of dynamometer, or weighing apparatus rather than an electrochemical phenomenon, as our secondary standard in conjunction with the mercury ohm. Most people seem out of favour with electrochemical standards. The silver voltameter is certainly not convenient even as a secondary standard at present. Whether, in the end, the silver voltameter will prove to be more convenient to determine the ampere than the cell to determine the volt, I think is a matter which will have to wait to be settled by the results of the work which is now going on all over the world. When that point is settled I think the practical man will take the cell as his tertiary standard. It can easily be carried about, and can be used with the potentiometer. All the arguments as to which one is derived from the other, whether the volt standard is to be determined from the ohm and the ampere standard, or *vice versa*, will not apply to the standard laboratories at all. They will establish the method which gives the most accurate result in making secondary standards. We have got very good tertiary standards of resistance, and from the results which have been brought forward here by Professor Carhart and others we seem to be going to have cells as standards of E.M.F. almost as good as wire resistances.

The question of the permanency of the cell is a matter of great importance, if it is to be used in practice. I feel that these electrochemical apparatus, which are liable to be changed if short-circuited or ill-used, will have to be referred back to the standard laboratories to make sure their value is right if we want to be certain of accuracy.

The next question is that of magnetic units. Dr. Kennelly, in the beginning of his remarks, made a great plea for consistency in his units, and then, having gone so far with the volt-ampere-ohm system, he proposes to be inconsistent, and names the C.G.S. system, and he gave the reason on the blackboard why there would be difficulties if he went on naming the volt-ampere-ohm system.

If we take the three fundamental electrical units, namely, the electromotive force, the current, and the resistance, to start from, we can make corresponding ones in the magnetic system which will be perfectly consistent. Although this system is more consistent, I must admit I am in favour of Dr. Kennelly's proposal of using the C.G.S. system for magnetic units. But I do not agree with him in giving them names. I feel that the C.G.S. system requires no names at all. If you are working with the C.G.S. system, it is self-evident what you

mean, without having to name each quantity. I think a very good example of that is, when I go to ask for an avenue in the town I do not ask for Vandeventer Avenue; I simply ask for Vandeventer. It is never Thirteenth Street; but Thirteenth. And I am rather surprised that an American should want to add another term to our names for our units when they are cutting words off in regular ordinary things of life. Mr. Duddell.

As to another proposition of Dr. Kennelly, in his original paper, which has been referred to here this morning, namely, the prefixes ab and abs, I totally disagree with any prefixes without definite numerical meanings. It would only lead to complications in the future. If we adopt prefixes at all, and name the whole of the C.G.S. units, I think the prefixes should indicate clearly what fractions they are of the practical units, as we are naming them from the practical volt, ampere, and ohm.

In conclusion, I should like to ask Dr. Sharp one or two questions. He has made reference to the enormous amount of money one-tenth of 1 per cent. would mean in the supply of energy in the United States. I should like to ask where they buy their meters in this country, which will measure to one-tenth of 1 per cent. Unfortunately, on the other side, we have no meters that work to that degree of accuracy. He also speaks of lamp life testing accuracy, and refers to 2 per cent. It seems very extraordinary to me. I do not know of anyone in England or on the Continent that can approach figures of that sort at all. I would also remark that a small variation from the mean voltage makes a considerable difference on the life of the lamp.

Professor Nichols: Allow the chair to announce that the time for adjournment has passed, and although there is doubtless very much more to say, the chair thinks that the debate should be closed by simply allowing Mr. Kennelly and Mr. Sharp, against whom definite points have been made, to reply very briefly to these points. Professor Nichols.

Dr. A. E. Kennelly: I would like to say that a question of one-tenth of a volt raised to my knowledge a very serious question some months ago, which might have involved a large sum of money, in regard to a contract concerning incandescent lamps. Of course, ordinary voltmeters are not read to one-tenth of a volt, nor are lamp-testing voltages maintained to one-tenth of a volt, but contracts for incandescent lamps are based upon their laboratory performance with a specified voltage, and a change in the standard voltage by one-tenth of a volt at the laboratory when the average lifetime is under test might involve disputes and loss of money. Dr. Kennelly.

Dr. C. H. Sharp: I think the point raised has been pretty well covered by Dr. Kennelly. It is a question of standards that we work from. What is the normal? We must have that correct. We do not have meters in this country that register to the tenth of 1 per cent., but we do test them with such standards, and that difference may be of importance when they lie close to the line. If, for instance, you say a meter is guaranteed within 2 per cent., and if it fails very close to that value, then one-tenth of 1 per cent. begins to count, and our standards must be correct. Dr. Sharp.

On the motion of the Chairman the meeting then adjourned

Dr. Guthe. Dr. K. E. GUTHE (*communicated*) : We are all greatly indebted to Dr. Wolff for his valuable paper, for the collection of the divers laws relating to the electrical units, and for the clear statement of the shortcomings of the various legalised systems.

That the present chaos cannot be continued for an indefinite period is clear, but the time is not ripe for any radical changes in the present laws. If I understand Dr. Wolff correctly, he urges simply the adoption of some resolutions looking toward a future change, and to make suggestions as to the nature of the proposed changes.

It is indeed gratifying that so much progress has lately been made in preparation of the materials for the standard cell, but there is still a great deal to be done before we can be absolutely sure of the superiority of the cell over the silver voltameter. First of all, a thorough comparison of cells set up by different men, with materials prepared independently, is required; and in addition, the new cells must be kept under observation for a much longer period than has been possible so far.

The silver voltameter is not such a very inaccurate instrument, as it may appear from the objections which Dr. Wolff has carefully collected and so forcefully stated. I have shown, in a paper before this Congress,* that there are two types of silver voltameters giving results differing as much as one in two thousand, but with any definite form, for example, the one recommended by the Chicago Congress, the agreement is much closer, while the porous-cup voltameter can be relied upon to within one part in at least ten thousand.

It is true the measurement of a current by means of a standard cell and a potentiometer does not require as much experimental skill, and is more elastic than is the case with the voltameter, and it will always be resorted to in practical electrical measurements. But we should not strain a point. We are speaking of fundamental measurements, and not of ordinary laboratory practice, and for the former the all-important question is the one as to the accuracy obtainable. For the practical engineer, it makes very little difference whether or not the E.M.F. of his standard cell is determined directly in absolute measure. It is always a secondary standard.

While deploring the present unsatisfactory state of the question, I fully agree with the propositions made in a recent publication† by the Reichsanstalt, warning against too precipitate action.

Redeterminations of the different electrical units in absolute measure are in progress, and, therefore, it seems wiser to wait for the completion of these investigations before the question as to the desirability of establishing concrete units is taken up, a question on which the engineer and the theoretical physicist will always differ.

* Paper before the Section of Electrochemistry.

† *Elektrotechn. Zeit.*, vol. 25, p. 669, 1904.

PRESIDENTIAL ADDRESS.

Delivered at the Joint Meeting of the American Institute of Electrical Engineers and the Institution of Electrical Engineers, at St. Louis, U.S.A., on September 14, 1904, by BION J. ARNOLD, *President Am.I.E.E.*

(Reprinted from the *Transactions of the Am.I.E.E.*)

President BION J. ARNOLD called the meeting to order at 10.30 a.m., and by his invitation the chair was occupied by President R. Kaye Gray.

President B. J. ARNOLD: This is the second joint meeting of the Institution of Electrical Engineers of Great Britain and of the American Institute of Electrical Engineers. It was our pleasure to meet with the Institution of Great Britain in England, in 1900, and on the grounds of the Paris Exposition, in Paris, having an adjourned meeting there, wherein took place a joint discussion. At that time our British friends were our hosts and at this time it is our pleasure to have them with us in this country. This meeting is a joint meeting, and, therefore, neither Institution takes precedence—it is entirely a joint affair, and will be presided over by the executives of both Societies. This is not connected with the proceedings of Congress, but the members of the Congress are cordially invited to be with us this morning and take such part in the discussion as they desire. It is hoped that this will not be the last meeting between the Institutions represented, but that we may have many of them in the future at as frequent intervals as practicable.

Eleven years ago this summer it was our privilege to meet under the auspices of a great Exposition, located upon the shores of Lake Michigan, organised not only to commemorate the 400th anniversary of the discovery of this country, but also to direct attention to the advancement made in the various fields of the world's activities, and especially in those arts in which we, as workers, were most interested.

To-day we meet under the auspices of another great Exposition, brought into being to commemorate the 100th anniversary of the peaceful acquirement by the Government of the United States of a large portion of the territory now contained within its borders, to have our attention directed to the development of the various industries of this and other countries that have taken place during the intervening years.

For a few years preceding the former Exposition, engineers and others engaged in electrical pursuits had had their energies absorbed in attempting to show the owners of street railways that operation by electricity was cheaper and better than by means of the horse or the cable. We, at that time, had seen the horse practically disappear from street railway service and the cable supplanted in some instances.

The more ambitious engineers were then advocating the use of electricity on elevated railways, and making figures to prove to the owners of such railways that electricity was cheaper in operation and more desirable for such conditions than steam locomotives, then universally used for such work.

At that Exposition was placed in operation an elevated electric road, known as the Columbian Intramural Railway, which though the City and South London Underground, a road of light equipment, was started some time before, and the Liverpool Overhead Road soon after, was the first practical commercial application on a large scale of electricity for the propulsion of heavy railway trains. The success of these roads gave the electric railway industry an impetus which has since resulted in the abandonment of steam and the adoption of electricity on every elevated railway now in operation, and practically on all of the underground roads, thus effectually proving the soundness of the theories of those engineers who pinned their faith to the correctness of the conclusions which their figures showed, and who staked their reputations upon the future to prove them true.

The interval between these Expositions has also been one of great activity and development in the field of interurban railways, which has brought into being the extensive use of the alternating current, rotary converter sub-station system of operating direct-current roads, resulting in the interlinking of thousands of cities with each other and intervening points, thus not only affording a new field for the investment of capital, but bringing to most of the inhabitants of the territory through which these roads pass greater facilities for the prosecution of business and the widening of their social life.

With the introduction of the suburban railway came an increased volume of passenger travel, induced by the increased facilities, which may well be noted by the managers of great steam railway properties as an example of what may be expected in increased revenue when frequent and pleasant service is available to the public.

The energies of those engaged in electrical industries have thus far been absorbed in fields which now seem to have been naturally theirs, and their success has been such that they now aspire to enter the field occupied by the steam locomotive as a legitimate field of conquest.

The question now is whether this field is one in which the advantages of electricity will be sufficient to overcome the obstacles which seem almost insurmountable, and enable it to win as it has in the cases cited. Those who have given the subject little thought, or who are unable to analyse it carefully on account of the lack of the technical knowledge necessary to appreciate the difficulties to be overcome, are most apt to predict the early supremacy of the electrically driven train over the steam locomotive. That the fields referred to have been apparently formidable yet quickly overcome is not necessarily proof, or even good evidence, that the legitimate field of the steam locomotive can be entered and successfully achieved.

Those most familiar with the subject are now prepared to admit that our great steam railway terminals, where many switching locomotives are shunting back and forth continuously, and those portions of the steam roads entering our great cities, where suburban trains are numerous, frequent, and comparatively light, can be more economically operated by electricity than by steam. This is evident to most of those engaged in the work, for the reason that it simply means duplicating, on a large scale, the systems which have proved successful in our street

railways, operating, as they do, numerous units running at frequent intervals.

Proof that this field is recognised as a legitimate one for electricity is furnished in the examples of steam railway terminals that are now being equipped electrically, such as the lines of the New York Central and Pennsylvania Railroad Companies in the vicinity of New York, involving an expenditure of something over $70,000,000, where not only suburban service will be operated electrically, but where, in the case of the New York Central, the main line trains will be brought into the city from points thirty to forty miles distant.

While these are great examples of electrical operation on steam railroads, and heroic instances of faith on the part of the railway managers in the ability of electricity to successfully meet the conditions of steam railroad work, where the trains are sufficiently frequent, they are by no means conclusive evidence that electrically propelled trains can be made to successfully meet the conditions of trunk line passengers and freight service, the field now so successfully held by the steam locomotive. The best conditions for electrical success are a great number of units moving at a practically uniform schedule, at equal intervals, within a limited distance.

The legitimate field of the steam locomotive is now one in which there are few but heavy units moving at uneven speeds over long distances at unequal intervals and at high maximum speeds. The amount of energy transmitted to any great distance and used by electric cars that have been put in use until recently has been small when compared with the amount of energy that it takes to propel a steam railroad train of five or six hundred tons weight at the speeds ordinarily made by such trains.

It may be taken as axiomatic that when investment is taken into consideration, power cannot be produced in a steam central station, under conditions that exist to-day, and transmitted any great distance to a single electrically propelled train, requiring from 1,000 to 2,000 H.P. to keep it in motion, as cheaply as a steam locomotive, hitched directly in front of the train, will produce the power necessary for its propulsion. Therefore, there must be other reasons than the expected economy in power production to warrant the adoption of electricity on a trunk line railway unless it can be shown that the trains are frequent enough to make the saving in the cost of producing power greater than the increased fixed charges made necessary by the increased investment due to the adoption of electricity.

There are undoubtedly in existence to-day conditions where water power in abundance is available along the right of way of existing roads, in which the substitution of electricity for steam could be made a paying one, with apparatus now available, even on roads having a comparatively infrequent service ; but these are special cases, and only tend to prove the correctness of the position, for in these special cases the cost of power would be but little over half the present cost of producing it by means of a central steam-driven station.

The ideal conditions for any trunk line railroad having a traffic heavy enough to warrant the investment in a sufficient number of tracks

to properly handle this traffic in such a manner as to get the most efficient service out of its rolling stock, would be to have four or more tracks between terminal points, upon which, in pairs, could be run the different classes of service at uniform rates of speed. Thus, if six tracks were used, the through line, passenger, and express service would be run on one pair of tracks ; the local passenger, local express, and local freight service upon another pair of tracks ; while the through freight service would be run upon a third pair of tracks, and all the trains upon any pair of tracks would run at the same average speed and stop practically at the same places. If these conditions could prevail and the traffic were sufficient to warrant this investment in tracks, such a service could be operated more economically and more satisfactorily electrically than by steam.

The difficulty is that few roads in existence have sufficient traffic to warrant such an investment in permanent way, and the result is that all of their traffic must be handled over one or two tracks, thus necessitating trains of all weights and all speeds running upon the same rails. This results in a tendency to bunch the cars into as few trains as practicable, in order not only to reduce the cost of train service to a minimum, but to give the fast-running trains greater headway to allow them to make their time safely. Such an arrangement of trains necessitates the concentration of large amounts of power in single units, which is leading away from the ideal conditions for the application of electricity to the propulsion of trains ; and it is this element, combined with the fact that the traffic on most roads is not great enough to warrant the investment necessary in electrical machinery to produce and transmit the power to the distances necessary to keep a few heavy trains in motion, that makes the trunk line railway problem so difficult, as it is more economical to propel these heavy trains by steam-driven locomotives, which are practically portable power-houses.

It being admitted that electricity becomes most economical when a sufficient number of trains are available, and that the steam locomotive is most economical when the trains have become few and heavy, the problem then resolves itself into one of the density of traffic, and the question then is : where is the dividing line ?

It was my intention to attempt such an analysis of this subject as to be able to formulate some general law which could be readily applied to any given case, and thus enable one to decide whether electrical operation would be more economical than steam in any concrete case.

After carefully analysing the subject I have become convinced that no general law or formula can be laid down which will apply to all cases, for the reason that the elements entering into different cases vary so greatly that any formula would contain too many variables, dependent upon local conditions, to admit of a general application. I shall, therefore, only attempt to point out a way in which the dividing line between steam and electricity can be determined after the elements of each case are known.

It will readily be seen that with steam locomotive operation the fixed charges, and cost of fuel and engine labour increase almost directly proportional as the train-miles increase, for in this case an

additional locomotive means simply a given amount of increased investment, a given amount of increased fuel and labour, and this total investment is least when the number of locomotives is small.

On the other hand, with electricity it is necessary to invest at once a large amount of capital in the power-houses and transmission systems, which amount must be great enough to provide for handling the maximum number of trains required upon the line, and unless this number of trains is great enough so that the economy effected in the different method of producing and applying the power is sufficient to offset the increased fixed charges, due to the additional invested capital, it will not pay to equip and operate electrically. Any problem, therefore, must be analysed for the relative cost in operation. In case this does not show a saving, the advisability of equipping electrically will depend entirely upon the probable increased traffic to be derived from the adoption and operation of electrically propelled trains.

That electricity will be generally used on our main railway terminals, and ultimately on our main through lines for passenger and freight service, I am convinced, but I do not anticipate that it will always be adopted on the grounds of economy in operation; neither do I anticipate that it will come rapidly or through the voluntary acts of the owners of steam railroads, except in special instances. At first the terminals will be equipped for special reasons, due either to the voluntary act on the part of the terminal companies to effect economy in operation, or to public pressure brought to bear upon the owners through an increased demand on the part of the public for better service, on the grounds that the use of the steam locomotive is objectionable in our great cities.

Those owners whose roads run through populous countries will either build new roads, or acquire, for their own protection, those electric railroads already built and operating in competition with them, and utilise them as feeders to their through-line steam trains. Thus the steam railroad companies will gradually become interested in electric railways, and eventually become practically the real owners of them. With these roads operating as feeders to the main-line system, and with the terminals thus equipped and the public educated to the advantages of riding in electrically equipped cars, the next step will logically be the electrical equipment of the trunk lines between the cities already having electrical terminals.

Thus some favourably located trunk line having a sufficient density of population will feel warranted in equipping electrically, and when this is once done the other roads running between the same competing points must, sooner or later, follow in order to hold their passenger traffic.

This may result in temporarily relegating some roads to freight service, so long as they operate exclusively by steam, but with the increased demand on the part of the public for better and cleaner service will come a corresponding increase in passenger revenue to the roads equipped for handling it until one road after another finds it advantageous to furnish an electric passenger service.

With the terminals and main lines equipped electrically, and the

desire on the part of the public for more prompt and effective freight service resembling that which is given by the steam roads in England and on the Continent, due to the great density of population, there will be developed a great high-class freight service conducted in light, swiftly moving electric trains which can be quickly divided and distributed over the surface tracks of our smaller cities, or through underground systems similar to that which is now being built in Chicago. Such a system would soon prove indispensable to the public and a source of great profit to the roads, as it is now getting to be to many suburban railways.

This class of freight service would soon prove so large a part of the freight traffic of a road that the operation of the through freight traffic by steam locomotives, though at present cheaper, would in time, as the cost of coal increases, grow less, until those roads operating an electric passenger service would ultimately use electricity exclusively.

It has not seemed advisable to me in an address of this character to attempt to furnish detailed figures to support my theories, for the subject is of such general interest that many able men are presenting papers upon it at the International Electrical Congress now in session here, in which papers will be found information of much value to those interested, and from which I believe the correctness of some of my assumptions can be proved. The principal problem before the electric railway engineer to-day is how to make the most effective use of the high-pressure transmission, and high-tension working conductor, and maintain safety of operation. Experiments conducted during the past year by engineers in this country and abroad have made this problem simpler than it seemed before, and to-day we seem reasonably certain of the solution.

Until recently the cost of electrically equipping a trunk line under the standard direct-current, rotary converter system, has been such as to practically prohibit its adoption, but recent developments in the single-phase alternating-current motor field have made it possible to eliminate a large part of the investment heretofore necessary, and the prospects for the application of electricity to long-distance running are better than ever before.

When it is recalled that the rotary converter, which was the means of reducing the cost of long-distance roads, was introduced in 1898, and that within the six years from the time of its adoption through the development of the single-phase motor it has been practically rendered obsolete for heavy railroad work, it will be seen that the dividing line between the steam locomotive and the electrically propelled train has moved several points in favour of the latter, due to the reduction which can now be made in first cost and the saving in operating expenses. With the single-phase motor and the steam-turbine a reality, with the transmission problem almost solved, and with the rapid development of the internal combustion engine now taking place, are we, as engineers, not warranted in believing that we can so combine them into a system which will ultimately supplant the steam locomotive in trunk line, passenger and freight service?

I do not anticipate that all roads will soon adopt electricity, for the

steam locomotive will hold its field in this country for many years to come; but I do expect, judging somewhat from positive knowledge, that a remarkable development will soon begin in the electrical equipment of favourably located steam roads.

From Richmond, where the first commercial electric road was built, to the present is but seventeen years, yet within that time the horse has been relegated to the past as a serious factor in transportation, the cable has served its usefulness and awaits its end, and the suburban railway has been developed and is now rapidly encroaching upon the field of the steam railway.

With the terminals of the two greatest roads in the United States now being equipped electrically and with an investment of something more than $4,000,000,000 in electrical industries, made within a quarter of a century, we have reason to feel satisfied with the past.

With several of the leading roads in this country, of England, of Sweden, of Switzerland, of Italy, and Australia, electrically equipping branch lines and seriously considering changing large portions of their present systems from steam to electricity, we, as personal factors in this great industrial advancement, have every reason to be hopeful for the future.

President ROBERT KAYE GRAY: I do not know whether I am perfectly in order under the American procedure or not, but our habit on the other side, when we receive an address from our President, is to tender him our thanks. As President Arnold has said, during the Paris Exposition we had a joint meeting of the two Institutions, and I am very glad indeed to say that we have in this hall to-day the two gentlemen who presided on that occasion, namely, Mr. Carl Hering and Professor Perry. I do not think that any one could even have wished to criticise, in any way, the address which has been so ably given by your President, because if any man, either on this side or on the other side of the Atlantic, is pre-eminent in connection with the subject he has treated, I think it is President Arnold. His name is exceedingly well known to us on the other side, and I think I am not giving away any secret in telling you that the evidence of his work which he has been tendering to us has received a very warm reception there, and the evidence is considered to be the best that can be obtained in relation to the matters with which it deals. I therefore wish, in the name of the Institution of Electrical Engineers of Great Britain, to tender to my colleague, President Arnold, our very sincere thanks for his exceedingly able address; and, with your permission, I will ask the senior Past-President of the Institution of Electrical Engineers of Great Britain to second the motion—Colonel Crompton. President Kaye Gray.

Colonel R. E. B. CROMPTON: It is with the most heartfelt pleasure that I rise to second the motion of President Gray, that the thanks of the American Institute of Electrical Engineers, as well as our own Institution of England, be given to President Arnold for his address, which I personally feel is worthy of this great occasion—the meeting of the two Institutions. Colonel Crompton.

President BION J. ARNOLD: I assure you that your expression of President Arnold.

President Arnold.

approval is very much appreciated indeed. We have for our discussion this morning a subject similar to that which I have treated in my address; in fact the address was written as a sort of introduction to the discussion. This subject, "Different Methods and Systems of Using Alternating Current in Electric Railway Motors," has received the attention of engineers interested in electric railways for the past three or four years. During the past two years it has received very energetic attention on the part of leading engineers of Europe and this country, and it bids fair to be one of far greater importance as we get more thoroughly into heavy railway work. Since I have talked to you quite a while, notwithstanding the fact that my name appears first on the programme to discuss the question, I am going to ask a gentleman to open the discussion who is one of the most distinguished engineers in this country, and one of the most distinguished living authorities in electrical matters. I have the pleasure of introducing Dr. C. P. Steinmetz, of the General Electric Company, and Past-President of the American Institute of Electrical Engineers.

Discussion on "Different Methods and Systems of Using Alternating Current in Electric Railway Motors.

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Dr. C. P. Steinmetz: The problem which we have before us here for discussion—the problem of the direct application of alternating currents to electric railways—is not a new one, but it has become of primary importance and interest in the last few years. The early pioneers in electric rail-roading, ten or fifteen years ago, started the development of the alternating-current railway motor, and prominently among them I may mention Mr. R. Eickemeyer and Mr. Vandepoele, who designed alternating motors for railway purposes and investigated their characteristics. However, very little progress was made in this field for many years, for a number of reasons; one being that in those early days frequencies of 125 to 130 cycles were customary, far higher than we are using now, and the difficulties of the problem were thereby increased so formidably that advance was necessarily very slow. In addition, the very rapid development of the direct-current railway motor fully occupied the attention of all electrical engineers, and therefore the less urgent field of the alternating-current motor was necessarily somewhat side-tracked. Then the alternating-current polyphase induction motor came into the foreground, showed its superiority over other types of motors for stationary work, and impressed the engineers to such an extent that for a long time it overshadowed the work done by the early investigators on the variable-speed alternating-current motor, that is, on motors with series characteristic. Attempts then were made to introduce this very successful polyphase induction motor into electric railway work, attempts which have not been successful to any great extent. In the meantime, in the United States the synchronous converter was developed and became a standard piece of apparatus familiar to everybody—standard as much as the direct-current generator and the alternating-current generator, and experience with such synchronous converters shows that for electric railway work,

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for the violently fluctuating loads on the railway system, the synchronous converter is superior even to the direct-current generator : the absence of armature reaction, the phase control of pressure feasible in the converter, and corresponding close-pressure regulation makes it specially able to withstand and take care of very violent fluctuations of load and to carry overloads which no direct-current generator can carry. This apparatus became standard, and with its introduction the field of the direct-current railway motor—the distances which could be covered by the direct-current railway—was extended practically without limit, and a field opened which has been exploited in the last years, which was the field dreamed of by the early pioneers ; the difficulties, however, being overcome, not by the development of the alternating-current motor, but by the development of methods of transmitting alternating currents and transforming them into direct currents along the routes, in synchronous converter sub-stations. Now, however, in the last year or two, with the still further development of the electric railway we have approached, and in many instances reached, the limits of applicability of this synchronous converter. The synchronous converter is a piece of machinery which requires sub-stations, requires some attendance, and as a necessary result has a high economical efficiency only where the traffic is sufficiently condensed to warrant the maintenance of sub-stations within relatively short distances from each other. Where the number of trains is less or the power per train greater than can be supplied at 500 volts from sub-stations, without excessive expenditure in line conductors, and too excessive fluctuations of load, pressures are required higher than can be utilised efficiently in direct-current motors, and there we strike the limit of the synchronous converter, and the alternating-current motor has to come in.

Personally I do not believe that the alternating-current motor will make very serious inroads in the field now occupied by the direct-current railway motor. I do not believe that direct-current railway systems will be changed into alternating-current railway systems ; but what I expect of the alternating-current railway motor is that it will find a field of its own, a new field ; just as when the alternating-current method of distribution was developed in this country, it did not displace the direct-current method of distribution which occupied the centres of our large cities, but it found a field of its own, a field which has gradually developed so as to be equal in importance if not superior to the field occupied by the direct current. Hence, to conclude these remarks, what I expect of the alternating-current railway motor is that it will find and develop a field of its own, that field which the direct-current railway motor cannot reach—suburban and inter-urban service, long-distance service, secondary railway service. When considering the technical aspect of the subject before us for discussion to-day, the relative advantages and disadvantages of the direct- and alternating-current railway motors, we have to consider, first, the character of the problem we have to meet in electric propulsion ; secondly, the character of the apparatus which we have available to solve these problems ; thirdly, the additional features imposed upon the problem, or conditions more or less outside of the problem, as, for instance, the

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condition of the electrical industry at present, the existing investment in direct-current and in steam railroads, which have to be taken into consideration when discussing any new system of railway propulsion.

Regarding the characteristics of the different types of motors—the direct-current series motor now in universal use for railroad work, the polyphase induction motor proposed, and, to a certain extent, tried in recent years for railway work, a motor eminently successful in stationary

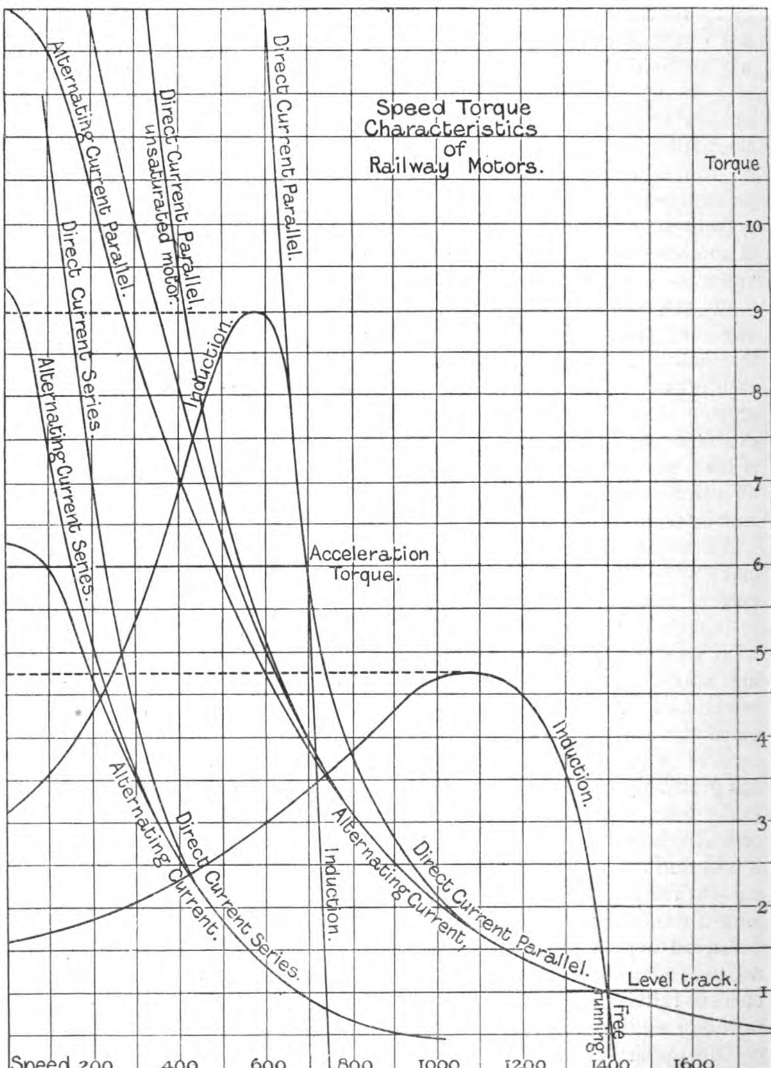

FIG. 1.

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work—and the alternating-current single-phase railway motor with commutator, I have, in a paper before the International Electrical Congress, given the results of a theoretical investigation and discussion of these different motors and shown the speed-torque curves, or characteristic curves, of these motors in relation to each other. In Fig. 1 is given a comparison of the typical speed-torque curves of the different types of motors.

In general, the characteristic of the polyphase induction motor is essentially that of a constant-speed motor, with shunt-motor characteristics; that is, it can efficiently operate over a certain limited range of speed only, cannot exceed the synchronous speed, and when operating below its normal speed, it operates less efficiently; that is, when operating at a lower speed than normal, or approximately synchronous, as can be done by a rheostat in the secondary circuit, the polyphase induction motor merely wastes that part of the power corresponding to the difference between its actual speed and synchronous speed. Or, in other words, at low speed the induction motor consumes the same power which it consumes with the same torque at full speed, though its power output is reduced in proportion to the speed, and its efficiency correspondingly.

In the direct-current series motor the torque developed by the motor decreases with increase of speed, and inversely with increasing load the speed of the motor decreases. The maximum torque of such a motor occurs in starting. All variable-speed commutator motors, alternating and direct, more or less differ from each other in the rate at which the torque varies with the speed, and that brings us to a consideration of the requirements of electric propulsion.

Important classes of work to which an electric motor may be put in locomotion are: first, city railway or tram-car work; secondly, rapid transit service as on elevated and underground roads; thirdly, suburban and inter-urban service; fourthly, trunk-line passenger service; fifthly, long-distance freight service, and sixthly, elevator service.

Now, discussing these briefly in succession. The city tram-car service is characterised by its frequent stops of irregular duration, at irregular intervals. To maintain good average speed it is therefore essential that the motor should get under way after the stop as rapidly as possible; that is, have a very high starting torque and accelerating torque, and carry this high torque up to a considerable speed. Beyond this speed, then, the torque of the motor should decrease fairly rapidly down to the torque required to run on a level track, which we may assume roughly to be at twice the speeds to which the high torque of acceleration should be maintained. In addition thereto, it is necessary that the motor should accelerate efficiently and that it should be able to operate efficiently at low speeds in those city districts where the general traffic is dense and where it is not possible to run at high speeds. The characteristics of this type of motor are pre-eminently given by the direct-current series motor. If we assume the torque required to run on a level track as 1, probably the starting or accelerating torque may be something like six times as high. At that torque we start and run up to considerable speed, and then strike what is called the *motor curve*

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and after cutting out all regulating devices, accelerate with decreasing velocity up to the free-running speed. Such a curve, that of a typical direct-current railway motor, is given in Fig. 1, marked "direct-current parallel" and "direct-current series." The induction motor, although it may accelerate with a high torque, at the end of acceleration, the speed is limited. It accelerates up to or near synchronous speed, and there the torque falls off to zero; and hence that part of the torque curve which is so essential to city tramway work, the curve of running with decreasing torque, from the limit of acceleration to the free-running speed, does not exist in the induction motor. We can indeed reach the free-running speed of a direct-current motor with a polyphase induction motor by gearing it to twice the speed, making synchronism the free-running speed, but this means that the available torque of acceleration, and therefore the rate of getting under way, is reduced by one-half, or, if we make it the same, the motor capacity is twice as great, requiring a motor twice as large. Considering that in this service a very considerable part of the running time is occupied running on approximately level track with torque very small compared with the accelerating torque, we see that the highest possible efficiency of the motor at light load is essential. Here, however, is the place where the induction motor falls down. A polyphase induction motor running at, say, one-tenth of its maximum output runs very uneconomically and with very poor power-factor. So in the polyphase induction motor, when used for railway service, you cannot combine very high acceleration with high efficiency in free running, and with the ability of running efficiently at low speeds, as you can in the direct-current series motor with series-parallel control. Therefore, the induction motor is not suitable to the class of work which we call city service or tram-car work.

The alternating-current commutator motor, of which two sets of curves are shown in Fig. 1, marked "alternating-current parallel" and "alternating-current series" has characteristics very closely similar to the direct-current railway motor, except that possibly the variation of torque with the speed is less. That means, with the same decrease of speed the torque does not increase at the same rate as with the direct-current motor; if we assume again the same free-running torque as 1, and the torque of acceleration six times the free-running torque, the direct-current series motor will carry the acceleration torque up to half speed; the alternating-current motor not quite as high. This means with the same maximum acceleration you will strike the motor curve at a lower speed, accelerating on the motor curve, you get under way, then, slightly slower, or to get the same average acceleration you have to start with a higher maximum acceleration. Now, this is an advantage in some cases in so far as you run for a longer period of time and over a wider range of speed on the motor curve, that is without controlling devices, hence in the most efficient manner possible, and thereby make up to a considerable extent for that power which the alternating-current motor inherently loses by its slightly lower efficiency due to the alternating character of the magnetic field, and the losses by magnetic hysteresis in the motor field of the alternating-current motor

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which do not exist in the direct-current motor. This difference in the speed-torque curve of the alternating-current series motor, compared with the direct-current series motor, is due to the lower magnetic density used in the alternating-motor field, and, at low speeds, also to the E.M.F. of self-induction. The first phenomenon, therefore, also occurs in an unsaturated direct-current series motor (Fig. 1), and such a direct-current series motor therefore has at high and medium speeds the same characteristics as the alternating-current motor. It is undoubtedly true that alternating-current motors can be designed to give very closely the same characteristics as the standard direct-current series motor. However, the motor as it is before us at present reaches the motor curve at a lower speed, therefore with the same maximum acceleration, gives a lower average acceleration up to full speed, or with the same average acceleration requires slightly higher maximum acceleration.

Coming now to the second class of service, rapid transit service, here the problem and the conditions of operation are almost identically the same as in city service, except that the units are larger, the speeds are higher, the stops not as frequent, absolutely, but about just as frequent relatively in comparison to the maximum speed of the motor, so that we can directly apply our considerations to rapid transit service—regarding a comparison of polyphase induction motors of alternating-current commutator motors, and of direct-current motors.

In inter-urban and suburban work, that is, in railroads running out from the cities far across the country into the suburbs or into other cities, we have a much lesser frequency of stops. That means that rapidity of acceleration is of lesser importance, and we can well get along with a lesser average torque of acceleration, but we must have the same surplus torque as on city service work, or rather a greater surplus torque, because, while in city service and in rapid transit service, where the distances are relatively short, we can count on maintaining fairly constant pressure in the supply system, we cannot to the same extent count on this in inter-urban and suburban service where we are far away across the country, except by investing much greater sums in line conductors and feeders than is commonly economically desirable or feasible. Hence, in this service the motor should have a greater surplus torque than in city service, so as to get a sufficient margin to start the train or the car under the most severe conditions on an upgrade or an overload, even if the pressure in the system is low. The motor which is most sensitive to pressure variation is the polyphase induction motor. The maximum torque which this motor can give necessarily cannot very much exceed the acceleration torque without badly spoiling the characteristics of the motor either electrically or mechanically; but the maximum torque varies with the square of the pressure and hence rapidly decreases if the pressure of the system is low. In the motors with series characteristics, however, like the single-phase commutator motor, the direct-current series motor, the torque does not depend on the pressure, or rather, while the maximum torque so depends, the theoretical maximum torque which you

Dr. Steinmetz. get from the motor when standing still is so far in excess of the torque of self-destruction, or rather of slipping the car wheels, that it is not reached, and the effect of variation in the supply pressure is merely a variation in the motor speed. That is, if the pressure is low in the system, the direct-current motor and the alternating-current commutator motor run at lower speeds, but still are able to give the same torque, while the polyphase induction motor runs at the same speed, but is not able to give the same margin of torque, and at a certain load falls down or does not start. That means that in designing a system of transmission and distribution for alternating-current commutator motors or direct-current motors we are permitted to design the system for the average drop of pressure in the system, while in designing it for induction motor service, we have to take into consideration the maximum drop of pressure in the system which is very much greater than the average.

For inter-urban and suburban service we require an excess overload in torque, but do not require an acceleration up to high speed. The alternating-current commutator motor appears to be pre-eminently satisfactory in this work, and this is where, I believe, it will be used extensively, and where the advantage of a high-pressure trolley and of the absence of sub-stations is specially important. In trunk-line passenger service the rate of acceleration as given at present by the steam locomotive is very much less than every-day practice in electric railway service. So here we do not need this excess acceleration torque sustained up to high speeds. Here, again, we find a field where we may apply the alternating-current commutator motors. The polyphase induction motors could be used if the question of pressure supply did not come in, as I discussed above; and if, furthermore, the limit in speed of the induction motor was not so objectional in passenger trunk service, where, more than anywhere else, we desire to get the full benefit of the track by running at the highest safe speeds wherever the track is level. This the induction motor with its limited speed cannot do. In trunk-line freight service, the same considerations come in, except there the speeds are relatively low, the train weights great; and it is more than anywhere essential to have a very large surplus torque available to get under way or to hold the train on a grade. You must, therefore, in this class of service, just as in suburban and inter-urban service, have a motor running efficiently at light load, but being able to give very high torque, although it does not need to carry this torque up to high speeds. On the contrary, it is not desirable in freight service that the motor should sustain a high torque up to high speeds, because that would mean the consumption of very large power. In freight service the highest possible economy is especially necessary, and the highest possible economy means the least fluctuations of power consumption; that means on up-grades you would desire to go slow and reduce the power consumption and get the high speeds on the level track. In mountain railways and such classes of work, the running torque is of the same magnitude as the starting torque, and so the load on the motor is more nearly constant than in any other class of railway work, and on the down-grade the motor is

preferably used for braking, by returning power into the line. Here then the polyphase induction motor appears well suited, and is indeed being used successfully. Such service, however, is in its character more nearly akin to elevator service than to railway service. Dr. Steinmetz.

In the discussion so far I have considered the requirements of the different classes of railway service, irrespective of extraneous conditions. When considering the alternating-current motor and the direct-current motor, we have to take in view what exists at the present time in this country and abroad. There exists the enormous network of steam railroad and of direct-current electric railways. The steam locomotive is a unit of very high efficiency, but a very large unit. It, therefore, for efficient operation requires the massing of traffic in heavy trains, and results in less frequent but large trains. This has practically rearranged and reorganised the whole system of locomotion by collecting it into a small number of very large units. That is not the most efficient manner of operating electrically propelled vehicles, but rather the contrary. Furthermore, you have to consider that every city and almost every village has a direct-current railway system. Now, the main and most important features by which the electric railway motor and electric propulsion has gained and is gaining rapidly in competition with the steam locomotive, appears to me to be the frequency of headway and the absence of passenger stations, not the speed, which frequently in electric lines is lower than that on steam railroads paralleled by them. The electric railway picks up its passengers anywhere in the city and deposits them anywhere, and it does not require them to consult time-tables, but runs its cars so frequently that the passenger can always find a car within a few minutes at any point; on the other hand, the steam locomotive requires you to consult a time-table and go to a depôt. As soon as the electric railway gives up this advantage which I have just mentioned, I believe one of the main advantages of the electric railway over the steam railroad will be lost, and this, therefore, is the feature which has to be kept in view. It means that whatever type of motor may be adopted in inter-urban or suburban service, etc., it must be able to carry the passengers through the cities over existing railways. The existing railways are direct-current railways, and I believe will remain so. That means that the long-distance motor, at least the suburban and inter-urban motor, must be able to run over the direct-current system. Hence, it must be a type of motor equally applicable and capable of operation on a high-pressure alternating-current or on the 500-volt direct-current system.

Taking this for granted, the methods of control must also be as simple as possible; that is, the same control for alternating as for direct current. Even if the motor could be used on direct current and alternating current, if we would have to carry a double system of control, one for city service and direct current, the other for long-distance service and alternating current, this would be a very serious handicap. It means that really to solve the problem before us, of extending the electric railway into inter-urban and suburban service, and into the field now occupied by the steam railroad systems, and into

Dr. Steinmetz.

new fields not yet developed, to a large extent not even dreamed of, that we must have a motor which, with the same controlling appliances and the same characteristics, can run either on the high-pressure alternating circuits or on the existing direct-current circuits.

Furthermore, the enormous investment in electric railway systems existing at present has all been made, in the large systems, on 25-cycle, three-phase apparatus. That means that we shall have to continue to operate at 25 cycles. It may be preferable, possibly, to run at lower frequencies, or it may be preferable to run at higher frequency in this instance or that instance, but regardless of whether it is preferable or not, if it can be done on 25 cycles, it will have to be done on 25 cycles, and if another frequency had to be used, it would be a very severe handicap to the new system. I am glad to say that there is no doubt that 25 cycles is the frequency best suited to the alternating-current single-phase railway motor.

Professor Perry.

Professor J. PERRY: Mr. President, ladies and gentlemen, I have to confess that I am not prepared to take part in the discussion. We have had the address of President Arnold and this excellent address of Professor Steinmetz, and I think I have never before heard two addresses so good on one morning. Clearly, they are men who have thoroughly studied the subject of traction, and it seems to me that they have both come to the conclusion that the electrification of steam railroads in the next ten years depends on the success of the single-phase alternating-current motor. I knew of the progress that had been made by the General Electric Company and the Westinghouse Company, in getting such motors to work well. I had heard a great deal about them before leaving England, and it is one of the subjects that I promised myself to learn something about during my visit here. I have not yet been able to do much in the way of getting accurate knowledge on the subject. I have been on a tram-car at Schenectady, the motor of which, I was informed, was driven by direct current, and the car ran well; and then that same motor was supplied with alternating current, and the car seemed to run just as well. But I had no means of experimenting or ascertaining what the efficiencies of the various arrangements were. Some ten or twelve years ago I was greatly interested in the single-phase alternating-current motor, perhaps for a selfish interest, as I had invented a system of traction which required the use of such motors. That interest is increasing. I wanted yesterday to go to the section in which Dr. Steinmetz was giving an account of this work, but I was told it was my duty to attend a discussion upon the subject of electro-magnetic units in another section, and as a man cannot be in two places at once, the doing of my duty robbed me of a great pleasure. Under these circumstances, I can only say that I should like to hear the discussion of this subject proceed further before I can feel able to take any part in it.

Mr. Lamme.

Mr. B. G. LAMME: Away back in the dark ages of electric traction, about 15 years ago, there was great confusion in the types of apparatus used. There were all kinds of motors and all kinds of apparatus on the car. They only had one property in common—they were all direct-current. After putting a number of these systems into

Mr. Lamme.

commercial use it was discovered that certain types of apparatus were superior to others, and those particularly interested in the manufacture of such apparatus followed up this matter to ascertain what properties were of the greatest value. It was gradually discovered that one type of motor was taking precedence of all others, namely, the series motor. Practically all development for a certain time was in the direction of the direct-current series motor. The reasons which led to this were partly based on theory and partly on practice. The series motor gave the effect of a cushion on a car. The motor is inherently a variable-speed machine and automatically varies its speed with the condition of the load. That was discovered to be a matter of first importance in the smooth operation of electric cars. Also the motor automatically increases its torque in a greater proportion than the current, which is of great importance in regard to starting and acceleration. These points were possibly not as well understood at that time as at present, but experience showed that certain equipments were superior to others and development was along that line. After a few years, when the motors had reached standard proportions and practically but one type was used, a second limitation was discovered; namely, in the transmission conditions. It was found that in the extension of the railway system, the ordinary 550- or 600-volt direct-current system was becoming cumbersome, and it was evident that some method of transmitting power at higher pressure and transforming to lower pressure for utilisation would be necessary. The most evident method was naturally to transmit by alternating current and convert to direct current, in order to use existing car equipment. This led to the use of motor-generators, and later to synchronous converters. The motor-generator was found to fit the existing alternating system fairly well, but in the development of the synchronous converter the manufacturers discovered a great difficulty in existing systems. The frequencies of 125 and 133, which were the standards for many plants, were entirely unsuited for synchronous converters and also not well adapted for synchronous motors. Another frequency, coming into general use, namely, 60 cycles, was found to be possible for use with synchronous converters, but the difficulties of design were very great in that case, and the synchronous converters were rather heavy and cumbersome.

At that time there was fortunately a new frequency adopted which was of prime importance in the development of the synchronous converter, namely, 25 cycles. So far as I know, the origin of that on a large scale was as follows: in the Niagara Falls power plant, when it was first laid out, the engineers for the power company had arranged for a frequency of 2,000 alternations per minute, or 16⅔ cycles per second. They wished to use 8-pole machines, running at 250 revolutions. The company which I represent, which was one of the prominent bidders on the contract, objected seriously to the proposed frequency, as it was considered entirely uncommercial and also not suited for the best design of machine. The engineers of this company recommended 4,000 alternations per minute, or 33⅓ cycles per second. That was considered extremely low compared with anything then in use. As we could not come to any agreement to use that frequency, we finally

Mr. Lamme. compromised on 3,000 alternations per minute, or 25 cycles per second, and the first Niagara machines were built in that way. There were various reasons for the adoption of a low frequency, one of which was that commutator type of motors might possibly come into use. Another reason was that it was better adapted to synchronous converters, but it was admitted that $33\frac{1}{3}$ cycles would also be satisfactory.

After the Niagara Falls plant was installed, there was then a precedent for the adoption of this frequency for large units, and the manufacturers began to build apparatus of this frequency for the Niagara Falls plant and also adopted it for other plants. This opened quite a field for the synchronous converter, and it soon began to be extensively used for railway work, as it was recognised that this was the link needed for extending the direct-current system. Even at the early date of 1893 and 1894 it was believed by many engineers that the synchronous converter was simply a machine to meet an emergency condition; that it would not last, that the time would come when synchronous converters would be dropped from the railway service, but as the most convenient and apparently the best solution of the problem, it was adopted extensively. About that time electric railway service began to be greatly extended and synchronous converters have thus come into very general use. By the use of synchronous converters, the advantages of the alternating-current system in transmission are obtained and the advantages of the direct-current system with the series motor are retained. Distances could be extended indefinitely by increasing the number of synchronous converter stations and raising the pressure of the alternating-current lines.

Shortly after this system came into general use it was recognised that a purely alternating-current system, in which purely alternating current was supplied to the motors, would be advantageous and considerable work was done along that line. The polyphase motor apparently had the field, and naturally the manufacturing companies took up the question of the application of the polyphase motor to traction work. The company which I represent, the Westinghouse Electric and Manufacturing Company, took up this question in an active way about 1895, and built two motors of 75 H.P. each for traction work. These motors were equipped with collector rings and rheostatic control and tests were made in regard to performance, both with straight rheostatic control and with the new well-known "tandem" control, in which the secondary of one motor is connected to the primary of the other to obtain half-speed conditions. Even with this latter arrangement it was found that the motors would not compare at all favourably with the direct-current motor or the system with the direct-current system using rotary converters, and this work was abandoned. It was recognised that the polyphase motor did not possess the proper series characteristics which long experience had shown to be so necessary for railway work. Other experiments along this line were made, using polyphase motors wound for two or more speeds, and two 100 H.P. motors were built which were wound for several speeds. While this was better than the other arrangements, it still appeared that this was not a solution of the problem. Previous to this

time the company had done some work in the direction of using single phase, but not as a solution of the problem which presented itself in 1895 and later. Mr. Lamme.

In 1892 the question of the use of the commutator type alternating-current motor for railway work was taken up. Two motors of nominally 10 H.P. each were designed and built. These were built for a frequency of 2,000 alternations per minute, or 16⅔ cycles per second. They were mounted on a car and were operated for awhile, but the system was not a success. In the first place the pressure used—400 volts as compared with 550 in the direct-current motor—was rather low. It was considered that as 550 volts was the limit in the direct-current motor, 400 volts would be the limit with alternating current. The motors were tested on a track of iron rails with practically no bonding. The track drops were excessive and the pressure fluctuations were great. The generator used—of about 20 k.w. capacity—was entirely too small for this work and it was not adapted to handle the inductive loads which were found with alternating-current motors. A series of tests was run, and it was finally decided that for city work, for which the system was then laid out, the motor could not compete with the direct-current motor. It was decided, however, that such a type of motor would probably furnish the solution of the heavy railroad problem, but as there was no such heavy railroad problem at that time, the work was dropped for awhile. But in 1897 the question of the use of the commutator type of alternating-current motor was again taken up—this time on a somewhat larger scale. Motors of 50 H.P. were built for variable-speed work, and given a long series of tests. Then, after sufficient experience had been obtained, the work was gradually carried to the larger sizes.

In 1900 and 1901, when the question of the polyphase traction in Europe was so extensively advertised, it became evident that there was actually a demand for an alternating-current railway system. It was therefore decided to continue the previous work with large motors of the commutator type, and two motors of 100 H.P. were designed and built. For these also, the frequency adopted was 2,000 alternations per minute, or 16⅔ cycles per second. This fractional figure was primarily adopted on account of certain steam-engine conditions. It was recognised that an even frequency of 16 or 18 would have been practically as good. In the earlier work, with the 10 H.P. motors at the low frequency, it was recognised that it would be absurd to put such a system on the market, as at that time even 25 cycles had not been adopted. The frequencies in common use were 50 or 60 and a drop to 16 cycles was considered prohibitive. In the latter work, as 25 cycles had come into general use, and 15 or 20 cycles had been talked of and proposed by certain companies, it was considered that in view of their advantages for railway work such frequencies should be adopted. The motors were hence built for the above frequency. The results obtained with these large motors were so satisfactory that a contract was taken for a rather large road and the apparatus prepared. Knowing that news of this would soon be abroad, it was decided that the matter should be brought before the American Institute of Electrical Engineers,

Mr. Lamme. and a paper was presented on the 26th of September—two years ago—which I believe was the first announcement of the application of the single-phase alternating current to railway motors. There was considerable discussion—mostly criticism—and it was generally considered by the engineering public that the weak point of the system was the commutation. At the present time, however, I believe this is no longer considered as a serious point. Previous to building the 100 H.P. motors we had had considerable experience with the commutation of such motors. Besides a long series of tests, we had run 40 H.P. motors at practically full load on a 60-cycle system for nine months, day and night. At the end of the nine months the commutators were in practically as good condition as in the beginning, showing that the commutator on such machines could be made to have a long life. The conditions of the 60-cycle machines were much worse than on the lower frequency, and the nine months of operation under the condition of steady service probably equalled two or three years of traction service; but the commutator stood up so well that we decided definitely that there was no difficulty on that point.

The principal reasons which led to the adoption of the single-phase motor were stated in the paper above referred to, and were that but one trolley wire would be required and that the motors had the series characteristics. It was considered that no motor, except one of the commutator type, would give suitable characteristics for the service; and it was stated that there were several types of motors, with commutators, which had the proper characteristics. All of these may be classed as series motors, although some of them are combined with transformers and may be considered as transformer series motors, or, under another name, as repulsive motors, and others are pure series motors. The pure series motor is one which can operate on direct current as well as alternating current. The repulsion motor can be modified so as to operate on direct current, but as ordinarily arranged it is not as well adopted for this as the other type. It was recognised in the first undertakings with this system that the motor would probably be required to operate on direct current at times, and the fact that the pure series motor was primarily a direct-current motor of a first-class design was one of the reasons which led us toward the adoption of that type. As both theory and experience indicated that such motors would probably be wound for 200 or 250 volts, it was recognised that the motors would probably have to be operated in series for direct current, and either in series or in parallel for alternating current as might be desired. The arrangements required for permitting operation on direct current as well as alternating are rather complicated, due to the fact that it is necessary to switch from one system to the other in passing from the alternating to the direct current. We did not suppose that the electrical public would consent to such a combination, but since that time we have found that in some instances they do not object seriously to the increased complication.

At the time that the alternating-current system was brought out it was considered that the principal field would be in heavy railway work, because this motor furnished what was considered a general solution of

the railway problem; as the railways would have their own terminals and their own rights of way, the system would be an alternating-current system throughout. At the present time, however, roads are being installed which operate primarily on alternating current, but at the terminals and where they pass through intervening towns they operate on direct current. Mr. Lamme.

The direct-current motor has never been considered as entirely suitable for the heavy railway problem, as usually but two speeds, and at most but three speeds, can be obtained with four motors, the third speed increasing the complication considerably. With the alternating-current motor of the commutator type any speed can be obtained for locomotive work, because any pressure can be applied to the terminals of the motor. As soon as alternating current is used for motors, we at once have a ready means of pressure transformation. As on locomotives for large capacity the difficulty of handling the current is considered a very prominent one, it was considered that some form of pressure control which varied the pressure without opening the circuit would probably be the best one. One form of pressure control permissible is what is called the induction regulator. This regulator varies the pressure without opening the circuit. The relation of the primary and secondary windings with respect to each other is varied. This gives a means of varying the pressure to the motors and varying the speed of very large motors with no tendency to sparkling at the controller. The only time the circuit is opened is at the end of the operation when cutting it off. Therefore it was considered as an important feature in the solution of the general railway problem.

The single-phase system is the one means presented at the present time as the solution of the heavy railway problem. It has all the advantages of the direct-current motor in the variable speed characteristic, and has also the advantage possessed by alternating current in the ability to use any line pressure desired, and to vary the pressure applied to the motor and thus vary the speed over any range desired. It also has the advantage of permitting a system of control that can be obtained without sparking.

In the adaptation of the alternating-current motor to direct-current service, two 250-volt motors can be connected in series for 500 volts; also in operating on alternating currents the motors can be connected in series, if desired, or in parallel. There is a possibility of danger in operating two motors in series in this way on alternating current, or even on ordinary direct current. In ordinary direct-current practice the use of two motors in series for part of the service is common practice, but there is this difference between the direct-current equipment and the alternating-current equipment. In the direct current we have motors wound normally for 500 or 600 volts. When operating in series the motors are connected, two in series, each one receiving 250 volts. Therefore, if one motor should slip its wheels and take the full pressure of the pair, it would still be operating at its normal pressure. But with two 250-volt motors connected across a 500-volt circuit, we have a different condition. In case one motor should take the entire

Mr. Lamme. pressure, we should have 500 volts across a 250-volt motor. That condition was considered early, and in the Washington, Baltimore, Annapolis project, a description of which was given in the American Institute paper read two years ago, we showed an arrangement by which this could be avoided. We had balancing transformers connected across the two motors in series. The balancing transformer was across the outside terminals, and a tap from the middle of the transformer was connected between the two motors. In this way equal pressure was supplied to the two motors in series, and the danger of a runaway was thus avoided. It is not yet determined how important this is, but I believe that something like this will be found advisable for the operation of motors in series, especially where high-power motors are used on medium weight cars for high-speed service. Possibly with comparatively low speed, and with very heavy cars, there may not be the same tendency to slip. On the direct-current part of the road, of course, the balancing transformer could not have any effect; but as the direct current is usually a very small part of the service, this danger would be lessened, due to the proportionate time in service.

In the application of the motor to use on both alternating and direct current, we have found some special conditions which affect the arrangement of control. Take, for instance, a large road being installed between Cincinnati and Indianapolis, where it is intended to run on direct current at the terminals and alternating current on the rest of the line. The normal speed on the alternating-current part of the line is so great that it would be prohibited in the towns, and it is found that to get the speed down to the desired rate in the city service on the direct-current portion of the road, it is necessary to connect the four motors all in series, and thus no series-parallel arrangement can be used. Pure rheostatic control is therefore necessary in the city. On the suburban part, a switch is used to throw the current from direct to alternating, simply throwing the four motors in parallel, and taps are used on the lowering transformers to get a number of pressures. In that way we get the effect of series-parallel control and even better, by having more than two steps. On a long line it is possibly of no great advantage to have many steps, but as a rule the more steps there are, the easier is the service on the controlling apparatus, and the more running speeds are available.

With regard to the application of the system to locomotives, on the steam roads where the systems are not tied up with existing electric plants, it is probable that in time the railroads will adopt their own pressures, and possibly their own frequency. This may not be 25 cycles but may be somewhat lower. I believe that the electrification of the steam road may be a controlling factor in the change from direct to alternating current in city service. If the large railroads with their own large power plants adopt alternating current throughout, then the towns lying along the roads will in time probably adopt the same power system, and even the large cities will sooner or later adopt the same system. At the present time the railroads, as far as they have gone, have adopted direct current because the cities through which they pass or enter are using direct current. When the railroads make the big

end of the project, however, then the cities will adopt what the railroads are using. When this comes about the direct-current railway systems in the cities will be superseded by the alternating. Mr. Lamme.

Dr. C. V. DRYSDALE: Mr. President and gentlemen, at this late hour in the discussion I do not propose to take up your time very much, especially as I am afraid that very few of us over in England have had much experience concerning this important subject. I should like, in the first place, to take this opportunity of congratulating you on this side of the water on having carried this important problem to such an extremely successful issue as has been recently shown in Balston and in other places. This subject has been worked on in several countries, but to America belongs the honour of having constructed the first line of any considerable length working on the single-phase system. We must still further admire the way in which it has been done, when we remember that the result has been achieved by getting over the great difficulties that were first encountered with the series-motor, and that in so doing it has been found practicable to use the same motor cars on direct- and alternating-current lines. That, in itself, is an enormous advantage over and above that of being able to use the single-phase alternating current. Dr. Drysdale.

It would be impossible to criticise the statements that have been made this morning, because they come from gentlemen who have had such exceedingly minute experience in the special branch of the subject, that their remarks must be taken as gospel, at any rate for the present. My object in taking part in the discussion is rather to bring the matter back to first principles. This subject has been worked upon in many different ways, and although the laminated series-motor seems to have been the first to give us results, and will probably survive as a practical solution of the problem, yet there are some interesting questions as to whether there are any other ways of fulfilling the requirements which may have other advantages. There is one thing that does not seem always to be sufficiently kept in view in traction matters, in relation to the starting of the cars, and that is the very simple matter that in the starting of the car you do not require power, you require force. If you wish to get anything into motion, what you require in the first instance is purely force, and until the body moves it does not require power at all. One of the great advantages which the steam engine has over any electrical system up to the present time, is the fact that when steam is first admitted into the cylinder you get the pressure on the back of the piston and get the starting force without taking any power from the steam. If it were not for the other disadvantages of the locomotive, there is no question that this one feature would give it a superiority over anything we have of an electrical nature, because if we turn to the ordinary direct-current motor, we find that we have to use half, or with one motor, the whole, of the full load power merely to secure full load torque. This has several objections. Not only is it uneconomical and wasteful of power, but it throws a sudden strain on the generating plant, and furthermore the unnecessary power has to be wasted in resistances, and these resistances sometimes attain a considerable magnitude. With alter-

Dr. Drysdale.

nating-current motors these evils are worse, although less power is taken, as we then have low power-factors and consequently difficulties in regulation.

The time is too short to refer to many other systems, but I will mention one, that known as the Ward Leonard system, which at first sight appears to be an unpromising one. In the Ward Leonard system, as I understand it, the system is to use a single-phase motor coupled to a direct-current generator which supplies direct current to the car motors; and, of course, the indirectness of the method seems to put it at fault, but on the Continent that method has been developed with considerable prospect of success, in fact with considerable practical success, and it has this great advantage that by the use of this arrangement you can get your starting effort, with very small power taken from your station. In this system—it is too well known for me to describe it here—you have your single-phase motor continuously running, and you can do the whole of the regulation of your speed, etc., by merely regulating the excitation of the generator. The result is that it is possible to get the full starting effort with only something like one-third or one-quarter of the full load current on the motors. That is so important a matter, especially in view of the huge strains liable to be thrown on the plant in the large schemes which we are hoping to see realised in the future, that I think we should give that method the consideration which it deserves, although it at first sight appears to be roundabout. In addition to that, we have the magnificent system devised by your President, Mr. Arnold, and I hope we shall hear more of that in the future. My chief object in rising was to ask that we should hear as much about these systems as possible.

President Arnold.

President B. ARNOLD: I am pleased to be put down as one of the speakers on this subject, but Messrs. Steinmetz and Lamme have so thoroughly covered the subject, and Dr. Drysdale has so kindly referred to the other systems known to most of you, that it is not necessary for me to say much more, particularly as the time is growing short. I will correct one statement by Mr. Lamme, which rather puts me on the defensive. I understood him to state that his announcement of the single-phase motor made in September, 1902, was the first announcement of a single-phase system. I beg to state that in the month of June preceding, I read a paper on a single-phase railway, known as the Lansing, St. Johns & St. Louis Railway, which was built at that time and which I have since put in operation. I do not think it is just for the statement to be placed on record just in the manner in which it was made. I think Mr. Lamme meant to say that his paper was the first formal paper on the subject, but my road was built and almost ready to operate at the time that he made his announcement.

Now, without further discussing the question, I am going to call upon a gentleman whose name is known to all of you, who may rightly be called the father of the commercial electric railway. I have pleasure in presenting Mr. Frank J. Sprague.

Mr. Sprague.

Mr. F. J. SPRAGUE: I feel quite embarrassed by this pleasant introduction by our worthy President and the reception which you kindly give me.

Mr. Sprague.

The subject under discussion is how best to use the alternating current in railway motors. It is largely a technical question. The alternating-current motor is like a somewhat brilliant boy, who being exposed to various diseases has contracted a number of them ; he has had a moderate experience in mumps and measles, and a touch of typhoid fever, and the various doctors, many able ones here and elsewhere, have administered, sometimes in homœopathic but oftentimes in allopathic doses, large measures of quinine and other drugs. Whether, as the child grows—and we are all hopeful of that child—and he is subjected to the various climatic conditions of commercial introduction and use, these undercurrents of disease common to all fevers will recur, or whether the child will outlive them and become strong and robust is a matter which must be left to future developments.

There is a large problem, and I will not take over two minutes to speak of it. It is perhaps a more popular one, but of vital interest to us as engineers who are called upon to advise managers and others as to their financial expenditures, and that is : will electricity be used on trunk lines ? Our worthy President, with whom I have the honour to be associated on some important work in that line, is very hopeful, and so am I. But what are the reasons which may dictate the adoption of electricity on trunk lines ? Will it be because an economical service cannot be gotten by steam ? No. Will it be because there cannot be obtained to-day an efficient service ? Again, no. Will it be because of æsthetic reasons ? Distinctly not. If electricity be adopted on any trunk-line service it will be because of the hard and fast rule of financial necessity, not because we engineers urge it. It will be because the men who raise the money, run the road and have to provide dividends find that it is the best way to do it, and the reasons which will apply to one road are not necessarily those which will apply to another. It is my belief that some of the largest expenditures, and those most fruitful of return to those who own the steam railroads of the country to-day, will be for the purchase and control of competing electric railways which, having in the past acquired franchises of undoubted value which cannot be duplicated, have built up a profitable business which they can hold and which will increase. Many a steam railroad will be better off financially and get bigger returns if it gathers in these franchises and systems, and operates its whole property with proper regard to the needs and capacities of each division than by electrification of its main lines, at least for a long time to come.

NOTICE.

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Proceedings of the Four Hundred and Fifteenth Ordinary General Meeting of the Institution held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 15, 1904—Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on December 8th were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

Donations to the *Building Fund* were announced as having been received since the last meeting from Messrs. F. W. Clements and C. W. G. Nelson, to whom the thanks of the meeting were duly accorded.

The meeting adjourned at 9.30 p.m.

The following paper was read and discussed :—

THE COMBINATION OF DUST DESTRUCTORS AND ELECTRICITY WORKS ECONOMICALLY CONSIDERED.

By W. P. ADAMS, Member.

(*Paper read December 15th*, 1904.)

Since the reading of Mr. Newton Russell's paper on the Shoreditch Combined Electricity and Destructor Works before the Institution of Civil Engineers in 1899, there have been added some four combined stations of high importance in London. The working results of these have not so far been described. A considerable number of combined undertakings has also sprung up in the provinces, and although several papers relating more particularly to the works supervised by the authors have been read at different times before other societies, nothing has so far been contributed on the subject to this Institution, and no attempt at all has been made to deal in a general and independent manner with the actual steam-raising values obtained in practice. I therefore trust that in placing before you the results of the investigations which I have recently carried out, they may prove of interest and may help to clear the air of some misconceptions which are very general.

The problem of cremating refuse in specially constructed furnaces was not seriously attacked much earlier than 1870, but as the tipping grounds near large towns became filled up, it was obvious that some new and more sanitary method of disposal would have to be found. It had been the custom to make small bonfires on the tipping grounds in which the more promising combustible materials were burnt, and as the difficulty of disposal became more pressing the burning of refuse in a closed furnace naturally suggested itself. The first attempt made at Paddington in 1870 was a complete failure.

The material to be dealt with is of the most unpromising nature, one-third at least being incombustible, and another third often consisting of water, consequently it is not surprising that those who attacked the question in the early days should have considered that a self-supporting fire with such material was an impossibility. One of the earlier types of furnace was the celebrated "Beehive," which had two sets of furnace bars, the lower for a coal fire, and a short distance above a second, upon which the refuse was deposited and stewed, and finally burnt with the aid of the coal fire below. This type of furnace was not a success, it was costly on account of the large quantity of coal burnt, and it was generally found to be troublesome. Some cells

erected in Richmond in 1884 had to be shut down the following year.

To Mr. Alfred Fryer belongs the credit of constructing the first furnaces which were successful in burning refuse without the aid of fuel, and to him also belongs the honour of inventing a new word, namely "Destructor," a word often cavilled at by pedantic folk but nevertheless an expressive one. The first successful destructor was erected at Manchester in 1876, and the progressive Midland towns of Birmingham, Leeds, and Bradford soon followed suit. The chief trouble experienced in the earlier forms was the difficulty of obtaining a temperature sufficiently high to destroy effectively all noxious qualities in the refuse and in the gases produced by combustion. Various modifications were developed, and the strong draught found to be necessary to secure high temperature was first obtained by the use of high chimneys in conjunction with flues and grates of ample dimensions, and afterwards by the addition of forced draught.

I have not been able to ascertain to whom belongs the credit of first attaching a boiler to the destructor furnace, but I believe it was Mr. Thwaite. Until recent years attempts to generate steam from the waste heat have been at the expense of the sanitary destruction of the refuse, and it is a principle that must never be lost sight of, that a destructor should be erected with the single purpose of entirely cremating all organic matter and foul vapours, and only after this has been satisfactorily done can the utilisation of any spare heat be attempted. With wider experience and improved methods of construction such high temperatures were sometimes obtained that it became plain that no inconsiderable amount of waste heat was available, and small boilers were added in the flues to give sufficient steam for driving the engines on the works, and in the course of time dynamos were added to provide light at the works.

Later it was found that such abundance of steam could be generated that sewage and water pumping was successfully accomplished on a considerable scale in addition to providing power for clinker crushing, mortar mixing, etc., and at last the town of Oldham, finding that far more steam was generated than could be utilised in the destructor works adjoining the electric light station, a steam main was carried into the latter and town electricity supply from refuse-raised steam became an accomplished fact in March, 1896.

The St. Pancras Vestry was the first local authority to erect electricity and destructor works at the same time with the intention of utilising steam raised from refuse for generating electricity for public supply, but fundamental mistakes were made and the joint installation is not a success from an electrical point of view. It is, however, to the Vestry of Shoreditch, London, acting upon the advice of their consulting engineers, Messrs. Kincaid, Waller & Manville, that the credit belongs of constructing the first actually combined electricity and destructor works. I say credit with the full sense of my responsibility, for I know that the wisdom of the combination is still frequently called in question, but I trust that the additional information which I have been able to gather will once and for all justify their action.

Since the installation at Shoreditch was erected, combined works have been constructed in increasing numbers, and there are in operation at the present date over 40 works, and a further 20 are in course of construction. Of those in operation, five are in the county of London, and it is to these that I shall devote the most careful consideration.

In the nine accompanying tables I have put together a mass of information. In Table I. are given particulars of the destructor plant and method of operation. Table II. gives particulars of the electrical plant. Table III. contains information relating to the steam-raising value of refuse, both on test and in practice. Table IV. is of special interest, giving particulars of coal and water consumption on the electricity works, the price of coal, the units generated from coal and refuse, and in column 23 the estimated ultimate value of the refuse in units per ton. In comparing the costs in various destructors, the hours of working and rates of pay are important. These are set out in Table V., with particulars of cartage and collection. Table VI. gives the destructor "Costs" in full detail; all costs of working destructors, whether combined or not, should be recorded in this manner: it is very usual amongst destructor makers to give the cost of labour only. Table VII. shows the destructor "Revenue" in detail. Table VIII. records electricity works "Costs" in combined undertakings, and sets forth in full detail the fuel value of the refuse. Table IX. attempts to show the whole financial gain of the combination; but with regard to the saving or otherwise of capital I have not been very successful. The information relating to London works is, however, of interest, and column 11, when studied in conjunction with column 9 of Table IV., is valuable.

A reference to Table II., column 3, gives the dates of completion of most of the best-known installations.

Any attempt to enter upon a general description of the various makes of destructors would be out of place in a paper dealing more particularly with results obtained, yet a brief description of the more generally known types will be useful in showing the difficulties to be contended with in securing good steam-raising results and the problems that still await solution before the perfect destructor is evolved.

The type of combined destructor furnaces and boilers employed by Messrs. Manlove, Alliott & Co., is well illustrated in the Shoreditch installation. Two furnaces are placed one on either side of a water-tube boiler erected in the customary manner, and the gases from the destructor pass sideways into the boiler furnaces and thence between the tubes to the flues. The ordinary boiler grate is available for burning coal both when the destructors are in use and when shut down. Dampers are provided between the destructor and boiler furnaces to cut off the former when not in use, and the boiler front is made as far as possible airtight. These precautions are taken to prevent the access of volumes of cold air to the boiler and flues. The destructor ashpits are closed, and forced draught is provided by fans driven either by independent steam engines or by electric motors.

The refuse is admitted from the top on to an inclined hearth at the

back of the furnace, whence it is raked forward on to the grate. The refuse is partially dried on the hearth, and all gases therefrom have to pass over the hottest part of the fire to the side openings into the boiler furnaces. This system of top feed on to an inclined drying hearth is common to nearly all modern destructors.

The Horsfall destructor consists of a number of independent cells with independent ashpits placed side by side, and where the number is large, back to back. The refuse is fed in the same manner as in the Manlove Alliott cells, and the gases pass from the furnace into side exhaust flues arranged between the cells, and thence into the main flue at the back of the cells. This main flue plays the important part of a combustion chamber where all the products of combustion are raised to high incandescence. The boilers are placed in the main flue as near as possible to the cells. An ingenious device to prevent burning away of the firebrick lining of the furnace immediately above the bars has been adopted in the shape of cast-iron boxes arranged on either side of the grate into which the air blast is drawn by steam jet blowers, the outlet from the boxes being beneath the furnace bars; by this means a hot blast is secured while the boxes remain comparatively cool.

The Meldrum Simplex destructor is arranged as one furnace with several independent grates with separate ashpits, one long firebrick arch covers the several grates, and clinkering doors are provided to each cell. Hand-feeding through the clinkering doors is preferred, but top feed is arranged when desired. At one end of the furnace, or at the back, is a combustion chamber in which all gases are mixed, and with the shortest possible length of flue this chamber is connected to the boilers. The grates are fed and clinkered alternately, so that the temperature in the combustion chamber and flues is maintained so far as possible at a constant point. Forced draught is provided in each ashpit with independent control, so that as each grate is fed or clinkered the blast can be stopped. Steam blast is preferred, but fans are provided when desired; in both cases a regenerator in the flues behind the boiler is provided to raise the temperature of the air to some 300° or 400° Fahr.

Another type of the furnace made by Messrs. Meldrum Bros. is the Beamen & Deas, which consists of a pair of cells with independent grates and ashpits, but with a common combustion chamber behind. The forced draught is provided by either steam blast or fan, as in the Simplex. The feed is from the top on to an inclined hearth, the grate is horizontal, and clinkering is carried out through side doors. Each pair of cells is generally provided with one water-tube boiler placed immediately beyond the combustion chamber; the usual boiler coal-firing grate is provided with firing doors at the side instead of in front. To secure some uniformity of temperature in the combustion chamber, charging and clinkering should be done in each cell alternately.

The Heenan & Froude destructor has some features in common with the Simplex, several cells are placed side by side to form a continuous furnace ending in a combustion chamber, the ashpits are independent,

and closed and hot air blast is provided by fans which draw cold air through a regenerator placed in the flues behind the boiler. During clinkering the blast is shut off below the grate and admitted above; this is said to prevent the inrush of cold air through the open doors and the consequent cooling of the furnace and flues generally which is a fault in some other types of destructor. A feature of this furnace is the building of the roof in a series of parallel arches, one over each cell; by this means incandescent gases from one cell are deflected on to the top of the refuse in the next, thus securing combustion of the refuse from above as well as from below. The boiler, generally a water-tube, is placed by preference immediately above the combustion chamber. The undermining of the walls in the furnace above the grate bars by blowpipe action is prevented by the provision of shelves of fire-brick.

The Hughes & Stirling destructor has in common with several other types the top feed, sloping hearth and grate, and closed ashpit. Forced draught is provided by fans, and the gases from the fires are brought into a common flue between each pair, or double pair, of cells; this flue serves as a combustion chamber and opens out into a main flue which conducts the gases to the boiler. Water-tube boilers are preferred, and one is usually provided for each pair of cells.

The points, however, in common with all modern high temperature destructors are, forced draught preferably heated, means of drying the refuse in part before placing on the grate, and a combustion chamber in which all gases are mixed and brought to a state of high incandescence and their temperature equalised so far as possible before passing to the boilers.

Steam blast for producing forced draught is claimed to have advantages over simple air blast in that it produces water-gas in passing through the incandescent fuel which materially aids combustion; it also tends to keep the fire bars cool, thus giving them an appreciably longer life, and clinkering is rendered easier. These claims have been sufficiently verified, but it is to be doubted whether the larger percentage of steam used in practice secures an adequate return. The Horsfall Company claim that their system of hot steam blast requires 5 to 10 per cent. of the total steam raised, but no figures, test or otherwise, have come into my hands showing anything approaching such results. Messrs. Meldrum Bros. will guarantee that the percentage of steam with hot air blast provided through a regenerator shall not exceed 15 per cent. of the total steam generated, and they claim that it is often as low as 12 per cent. It is an extremely difficult matter to measure the steam blast except on specially arranged tests, but it is a very simple matter to measure the power used by electrically driven fans, and column 20, Table I. will prove of interest. However, it is a question that does not turn entirely on the relative consumption of power, as the improved combustion with fuel of high calorific value may outweigh the extra percentage of steam used with steam blast. This is one of the interesting points upon which additional information would be of value, but I venture to think that it will be a hard matter to justify the application in the blast of 20–40 per cent. of the total steam generated.

Economy in the blast is of considerable importance, as the absorption of power is continuous, and if a wasteful form be used a large percentage of the power available is wholly lost.

The prevention of air leakage is another important matter. The clinkering, charging, and access doors of the furnaces and necessary openings in the boilers are all potential sources of leakage, and not a few recently erected destructors have their efficiency much impaired by faulty design or construction in this respect. Serious air leakages and consequent loss of efficiency sometimes result by the development of cracks in the brickwork of seatings and flues, and it is often a most difficult matter to localise and stop such leaks.

Long and badly arranged flues are another prolific source of lost power. A principle never to be lost sight of in the construction of destructors is that after the perfect cremation of the refuse has been secured, the heated gases should be applied directly to the boiler without having to pass through long flues. Besides being a possible source of air leakage, long and badly placed flues sometimes become very damp, and a large part of the steam-raising value of the refuse is absorbed by the evaporation of water outside the boilers. Again, the charging arrangements are sometimes crude and not designed or constructed with a full appreciation of the problem involved; the furnaces of the boilers are often a source of trouble through air leakage and unsatisfactory combustion effects, and not a few combined works show poor results through careless and indifferent handling of the refuse and management of the burning.

I have named only the more prominent points which may seriously affect steam-raising values in destructors, but they will serve to illustrate the fact that the general design, construction, and management of a destructor may have quite as much to do with the steam-raising results obtained as the presence or absence of a good percentage of carbon in the refuse; and for this reason steam-raising tests in different types of destructors in different localities are not necessarily a guide either to the efficiency of any particular destructor, or to the comparative steam-raising value of refuse in various towns.

The composition of refuse varies largely in different parts of the country; in the neighbourhood of coal mines it is often rich in carbon. In towns having a staple industry where much waste material from the factories is sent to the destructor, the calorific value of the refuse may be high or low, according to the carbon in such waste material. Suburban residents generally provide quantities of garden stuff to feed the destructor, and although the percentage of carbon in this stuff is large, so also is the percentage of moisture, and it is therefore not regarded with favour by destructor managers. It is a strange fact that some of the richest refuse comes from poor localities. Mr. Vincent, the Electrical Engineer of Bermondsey, claims that his refuse has an unusually high value because the neighbourhood is poor, and this, I have observed, is also the case in another London borough where good refuse is collected in a poor district near the destructor, and refuse of only average value is obtained from a wealthy residential district a little distance away. This anomaly is due to the more extravagant

habits of the working classes, who, unlike the middle classes, rarely sift the ashes from their fires before consigning them to the dustbin. It has recently been stated that where coal is expensive, there refuse will be poor and the utilisation of steam therefrom of doubtful value; but I hold that the exact contrary is the case. The quantity of coal and ashes is probably less, but the fuel value of what coal there is and of the other carbon in the refuse is of proportionately greater value. That this is actually so will be seen by a reference to Table IX., column 11.

Some of the finest samples of refuse which have come to my notice are those from Warrington and King's Norton, near Birmingham. At the former place the electrical engineer states that his Beamen & Deas furnaces generate 3 lbs. of steam per lb. of refuse all the year round, and as an explanation he gives the proportion of cinder in the refuse as from 60 to 70 per cent.; this doubtless includes a quantity of ash. This result is of some value, as it has, I understand, been arrived at over a lengthy test under working conditions. At King's Norton the refuse for several days was sampled, and the samples submitted to an analyst—Dr. Frankland, of Birmingham—who gave, as the result of his investigations, 36·8 per cent. of carbon, 7·3 per cent. of oxygen, 12·12 per cent. moisture. It will be observed that the samples were comparatively dry, but nevertheless an available 4,500 B.Th.U. per lb. is a fine figure. Some 2,500 B.Th.U. per lb. of refuse may be taken as a fairly representative average of English town refuse in winter; this would be sufficient to evaporate 2 lbs. of water from 50° Fahr. to steam at 140 lbs. pressure, neglecting the evaporation of moisture in the refuse itself, and the heat absorbed in raising the clinker to some 2,000° Fahr.; under varying conditions of moisture these two items would absorb from 500–1,000 B.Th.U.

In some places refuse of unusually low value is collected. At Llandudno, where the bulk of the refuse is collected in summer during the season, when fires are in little use, and therefore the percentage of carbon small, the evaporation on test was as low as 0·7 lb. water per lb. of refuse. At Royton, in Lancashire, where a careful calorimeter test was made, only 997 B.Th.U. were obtained per lb. Such figures as these point very forcibly to the desirability of a careful and independent examination of refuse before combined schemes are entered upon. This is all the more necessary if the scheme be for a town outside the limits of this kingdom, for astonishingly poor refuse is collected in some Continental towns where destructors have been erected, a residue of some 60–70 per cent. of incombustible matter being left after cremation in a high temperature furnace.

There is often a considerable difference between the value of winter and summer refuse in a given town, and also a variation in the quantity collected. These variations, where I have been able to secure information, are set out in Table I. (columns 5 and 6) and Table III. (columns 6 and 7); but it is worthy of note that these variations correspond very fairly with the fall and rise of the summer and winter output of electricity, the winter refuse generally being of the highest value (see Fig. 1).

Some of the difficulties to be met with in the utilisation of refuse-raised steam for electrical purposes are—uncertain delivery of the refuse, the varying value of the refuse from day to day and hour to hour, and the varying percentage of moisture contained; also in winter snow may prevent collection for many days, fog sometimes greatly hampers collection, and holidays occasionally prevent collection for days. By careful management and forethought some of these difficulties may be largely neutralised, but it is sufficient merely to call attention to them to demonstrate that only under favourable conditions may a combined plant be expected to run for any length of time on refuse alone.

To equalise to some extent the varying quantities collected, and also

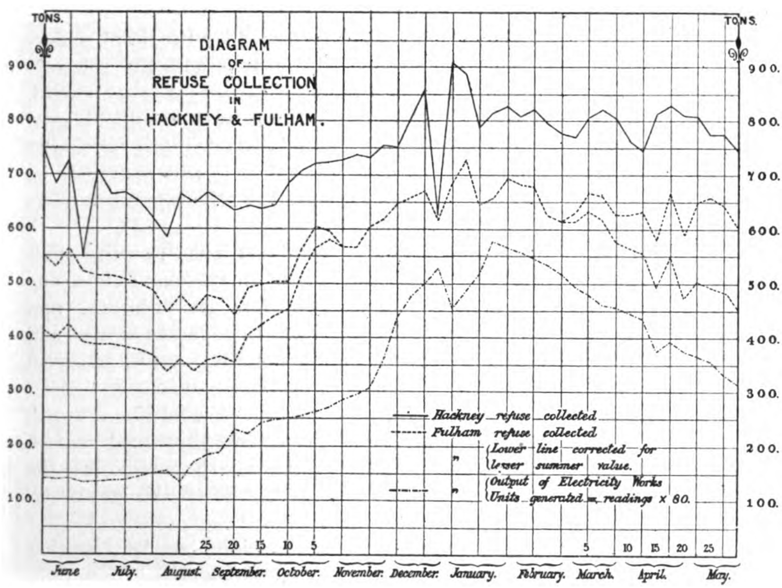


FIG. 1.

to adapt the quantity burnt to the varying demand for electricity, some system of storage is desirable; this is especially so where the electricity works has a small day load. The refuse itself may be stored and burnt as the demand requires, or, when direct-current plant is used, a battery may be employed with advantage. Large destructor plant is best run steadily throughout the twenty-four hours, and no great difference in the rate of burning during the day and on the peak is admissible. With a 12-cell plant, eight cells might be run during the day and twelve at night. This represents a maximum variation, and it will therefore be seen that where the day load is small, a peak load some five times greater could not be coped with in this manner. Personally, I am a strong advocate of the use of batteries, for they not only serve for

storage during the day, but with a suitably arranged reversible booster they equalise the load when the steam generation varies, as it does very rapidly sometimes. To secure the best results such a battery need not be abnormally large. A combined station burning some 30,000 tons per annum would require a battery of about 1,000 to 1,500 k.w.-hours capacity, assuming a simple lighting load with a day demand about one-fifth of the peak load.

Before I pass on to consider the commercial results achieved with joints schemes, I will deal briefly with the financial aspect of combined plants from the point of view of capital expenditure.

It is fairly obvious that in an undertaking of moderate size the combination of electricity works and destructor on the same site should result in several economies. These are embraced under site, building, chimney, and plant.

Site.—In most cases the actual ground covered by the joint undertaking is less in area than that covered by two entirely separate undertakings; and beyond this, some boundary space, approach road, and yard space would be considered indispensable in each undertaking. It is not easy to say exactly what land space is saved in any particular combined works, but, as a rough approximation, it will generally be found that one-third of the land space required for the separate undertakings is saved by combination.

Buildings.—An entire boiler-house is sometimes saved; on the other hand, the destructor with boilers will cover more space than one without. Sundry offices, mess-room, lavatories, weigh-house, and weigh-bridge would all require to be duplicate in separate works, and often no inconsiderable saving is effected in boundary wall and street paving.

Chimney.—In separate works separate chimneys would be erected, and a saving of a complete stack is therefore effected by combination. If a distinction is to be made, as would be right in large works, it is the destructor chimney that is saved, as this would generally be less costly than one to serve for the electricity works.

Plant.—Some boilers are saved to the electricity works, together with steam and feed piping, pumps, etc. In large works a generator to supply motors, and perhaps fans and lighting, are saved to the destructor; on the other hand an unusual length of steam main may be necessary.

So many different methods of apportioning capital outlay are in vogue that I would venture to suggest the following as a basis of arrangement in new works. Opinions will probably differ on this point, but I will give my suggestions for what they are worth in the hope that the already heavy labours of electricity and sanitary committees may in one or two cases be somewhat lightened when the burning question of apportionment of capital expenditure arises.

Site.—Actual value of ground space covered by the joint undertaking to be equally divided; spare land available for extensions to be debited to each undertaking in accordance with probable future requirements.

Buildings and Chimney.—An estimate of the cost of buildings for

each undertaking erected separately should be got out, the difference between these two added together, and the actual cost to be divided in the proportion of the estimate. The preparation of these estimates would involve far less labour than several protracted discussions of a large number of overworked town councillors.

Plant.—All boilers save one should be debited to the electricity works, together with steam and feed piping, pumps, etc. The entire saving in piping and pumps to the destructor would be a fair set-off against additional flues required by the additional boilers. The saving of an electric generator and the cost of its upkeep and attendance can best be dealt with by the electricity works making a small charge to the destructor for electricity supplied.

The cost of maintaining the buildings and chimney would be apportioned on the basis of the original estimate until extensions took place, and then the additional outlay would be debited to the department extended, and the cost of upkeep apportioned accordingly. I do not pretend that this is by any means a perfect adjustment, but there must of necessity be a certain amount of give and take in partnership of any description. It is highly desirable that each undertaking should be dealt with as a separate department with its own capital outlay, annual working costs, and income, if any, carefully set out.

The *Electrical Times*, which has already done so much in the tabulation of electricity works costs, has made an excellent start in giving an analysis of the costs at Hackney for both destructor and electricity works, and I have based Table VI. largely on this model.

Here will be found columns for fuel—for this is sometimes used in destructors—oil and stores, wages, repairs, rent and taxes, and management, in other words exactly the same classification as the *Electrical Times* has adopted for electricity works cost; and in addition I have included columns for three other important items, "Electricity for Light and Power," "Disposal of Clinker," and "Capital Charges." A highly important item in the costs of working a destructor is wages, an item as much debated amongst destructor makers and managers as coal amongst electrical engineers, but without some information as to wages paid and hours worked comparisons are not easy, so I have included some figures in this connection in Table V. It is very desirable that the wages costs in a destructor should be carefully separated from electricity works labour costs. The electricity department should certainly make some saving in labour in a joint undertaking, owing to the reduced stoking of coal-fired boilers; and in large combined undertakings the boiler superintendent is generally also the destructor superintendent, so in the matter of management some little annual saving should be effected. I have endeavoured to collect some concrete information on these points, but it is most difficult to strike a balance between the Electrical Engineer's estimates and those of the Surveyor or Health Superintendent. Such information as I think I am justified in putting before you I have embodied in Table IX. With regard to repairs and maintenance, the destructor department should bear all expenditure on furnaces and other destructor plant, one boiler, and all flues and

brickwork; even in maintenance a saving should be effected in joint works, as the fierce heat from the furnace is absorbed by the boilers and not turned loose into the flues and chimney to do damage to linings and brickwork. It has been necessary to provide a column for the cost of clinker disposal, which in some undertakings is an item of magnitude. I am hopeful that this column will eventually disappear. The supply of electricity and light and power to the destructor is a possible bone of contention between committees. By obtaining current from the electricity works some outlay on plant is saved to the destructor and also some running expense; on the other hand, the electricity used is generated from refuse steam and, apart from running costs, it ought to be supplied to the destructor free. The system adopted at Hackney seems fair: all electricity generated from the refuse is credited to the destructor at 0·39d. per unit generated, its ascertained coal value, and a charge of 1d. is made per unit supplied to the destructor; anything over 1d. is, I think, extravagant unless the credit to the destructor exceeds ½d. per unit.

The modern destructor, unlike the ancient, instead of battening upon the hard-earned gains of patient ratepayers has now become a respectable wage earner, and is making great efforts in the direction of self-support; an account has therefore to be given of revenue. This is set out in Table VII.

Revenue earned by a destructor combined with electricity works will be chiefly from the sale of current generated, and in addition there will be the sale of steam for other purposes; sale of clinker, flue dust, and other residuals; the destruction of trade refuse—which is paid for —and besides these items, which are more or less common to all combined undertakings, receipts are sometimes obtained from other sources. Fulham seems especially favoured in the matter of miscellaneous receipts, for no less than £300 per annum is received for the privilege of picking over the refuse as it is turned out of the carts. Is it not possible that other undertakings might secure a similar contract from a local rag and bone merchant?—it evidently pays the contractor at Fulham.

Very varied are the methods in vogue for crediting the destructor for electricity generated. In some undertakings not the remotest idea is held by those in charge as to what proportion of electricity generated is due to the refuse-raised steam, and while such a condition of things holds it is not surprising that the clumsiest of systems of crediting the destructor are devised. In every undertaking the average coal consumption can be ascertained from time to time when the destructor is out of use, or is purposely shut down, and then such average figure could be taken as a basis for ascertaining the units generated by each undertaking. In the larger works there really ought to be no excuse for not ascertaining what electricity should be credited, as the feed of the destructor boilers can with small cost be measured, and the average pounds of water per unit generated would form the basis of the adjustment; this method is less simple, perhaps, where steam blast is used in the furnaces, but a fair percentage of this could be agreed upon and deducted. The steam meter recently introduced by Mr. Bamber, of

the Manchester Electricity Works, should prove most valuable in conjunction with combined works.

The cash credit to the destructor for electricity generated should be on the basis of the actual coal cost; personally I can see no reason why the full ascertained coal value should not be credited as is the case at Hackney. It is often suggested that owing to variations in steam pressure and other causes, refuse-raised steam is of less value than coal-raised steam, but on a large output surely this cannot be a matter of moment. Then, again, it is suggested that the dust and general nuisance from the destructor causes extra work and increased repairs; judging from my own observations this would appear to need demonstrating.

The sale of steam for other purposes is a most difficult item to adjust in the absence—to the present time—of any means of measuring the quantity delivered to baths, laundries, disinfectors, etc. Here again I think the Bamber steam meter may be of great value; the need of some such apparatus will be readily appreciated when I am dealing with the London combined works.

In Table VII. I have included a column for the credit from the Health Department to the destructor. In some towns all the destructor costs are borne by the Health or Sanitary Department, and the electricity works gets the benefit of all the steam generated; in others an arbitrary allowance is made, and the difference between that and the actual cost of disposal is debited to the electricity works for steam; this debit is sometimes largely in excess of the coal value of steam. The fairest method is that in use at Fulham and Hackney, where the actual destructor revenue is deducted from the total costs and the balance is made up by the Health Department. As the utilisation of the steam and clinker increases the credit will diminish, and I have every hope that not a few of the large destructors combined with electricity works will become self-supporting in course of time. It is important to bear in mind that every penny earned by a destructor is direct saving or profit to the ratepayers, for a destructor is provided in the first instance as a sanitary appliance, and not as a commercial concern like electricity works; it is for this reason that I would urge that proper balance-sheets be provided for every destructor showing all costs and every item of revenue. It is very usual in the annual reports of local authorities to find brief statements made giving the net cost of disposal "after deducting value of electricity and sale of clinker," without stating what the various values are. I trust this system will soon fall into disuse, and that carefully kept destructor accounts will lead to the same emulation between the managers of combined works in the securing of records in costs that have been so important and valuable a feature in the development of electricity works and the reduction of the costs of generation.

Before leaving the financial aspect of combined works I must devote a few moments to Table VIII., in which I have set out, in perhaps unnecessary detail, particulars of units generated and sold, with values of coal- and refuse-generated electricity for comparison. A glance down columns 11 and 14 will show the relation between refuse-steam credit and actual coal cost. Beside fuel costs I have included wages and

management, both items which might reasonably be expected to be favourably influenced by combination; and, for the purpose of comparing the London combined undertakings with the uncombined, I have given the average of the six uncombined municipal undertakings now appearing in the *Electrical Times*. While not laying too much stress upon the fact, I will ask you to notice that the wages and management costs at Shoreditch, Stepney, and Hackney are the lowest on the list, thus tending to confirm my belief that in combined stations there is actually a substantial saving on both of these items. These remarks do not apply in the case of Fulham, but here the records given are for the first year, and I have reason to believe that Mr. Fuller, the new engineer, will show much better results for the next financial year.

I will now proceed to lay before you the results of investigations I have recently had occasion to make while preparing a large scheme for combined works. I believe these results will prove of no ordinary interest to those engineers who have not had occasion to inquire closely into the matter in the ordinary course of business, and I readily confess that when I began to draw out the various curves I had no idea that matters of such high interest were lying almost unsuspected in the log-books of some of the more recent works. While gathering information as to the actual quantities of electricity generated from refuse over a period of one year from the larger London and provincial installations, it became apparent that the average figures recorded in Table IV., column 21, did not represent the ultimate value of the refuse. To take an instance, the electricity works at Fulham are alternating, and the day load is small even in winter. In summer the load is still smaller, and the peak load of short duration. Where some 120 tons of refuse are burnt per diem, any substantial increase in the rate of burning during the peak is, as I have indicated, inadmissible; at Fulham there is no increase. It was therefore plain that if an all the year round figure of some 26 units per ton was obtained this value must at times be largely exceeded, for it was certainly not reached in summer and on the small day load. I therefore set to work to plot out curves so as to gather some indication of the ultimate value to be anticipated. One curve led to another, and I have now to show you quite a representative collection. I shall only have time to deal with the five London combined undertakings and one or two provincial.

Now, what really is the value of refuse for generating electricity? Specially arranged tests of steam generation give as a rule from one to two pounds of steam per pound of refuse, and on the assumption that these figures could be maintained in practice, various ingenious estimates have been made showing fine values; but so many factors come into play under working conditions that are non-existent in tests, that such calculations may well be received with some scepticism. Under ordinary working conditions from 20 to 40 units per ton are often obtained, and occasionally claims are made that better values are being secured and from 50 to 60 units per ton are mentioned with diffidence, as figures that will probably be received doubtfully. I have taken very great pains to record so far as possible correct results in

Table IV., and whenever possible I have verified claims made. In some cases where the units generated from electricity could not be stated by the engineer, I have had to decide from the average figures given of coal consumption per unit generated from coal alone.

Shoreditch.—" More Light, More Power," is the motto of Shoreditch, and very appropriately the Shoreditch Vestry adopted the first combined destructor and electricity works to supply more light, more power to the inhabitants. Their wisdom has often been called in question, and I suppose no one electricity works has given rise to so much controversy as these; mistakes were doubtless made in not rendering from the beginning accounts which were intelligible to the ordinary mind, but really I do not think all the hard things said have been deserved. Besides the awkward arrangement of the accounts, another matter which gave rise to no little controversy was the impression gained, how I know not, by some Vestrymen and others that the works would be able to supply all the requirements of Shoreditch without the use of coal—a very evident absurdity; and yet, even to this day, one often hears it laid to the charge of Shoreditch, as if it were a positive crime, that coal is used at the works and that therefore the destructor is an abject failure. I hope it is now generally recognised that only in the case of new works starting on a small load can a combined works be expected to run without coal, and that the second year of running is almost certain to bring a larger load than the destructor can cope with unaided.

As to the results obtained in the generation of electricity from refuse-raised steam I have little to add to Mr. Russell's figures of 1899 given to the Institution of Civil Engineers, for under the conditions of working it has not been possible to separate between refuse- and coal-generated units. I have one curve, however, that is of interest; Fig. 2 shows the output for December 16, 1903, and is a typical winter day load. Below is plotted Mr. Russell's historical curve of a day's test in 1899; it will be observed that the maximum value of the refuse was 55 units per ton, and that during the test the full value of the refuse was not obtained owing to the smallness of the load for the greater part of the day. The dotted line shows the rate of burning now in vogue throughout a winter day, assuming a value of 55 units per ton. It will be seen that the load has enormously outgrown the capacity of the refuse, and that therefore its full value should now be obtained. The summer load is also considerably beyond the capacity of the refuse. At the time of Mr. Russell's test all steam was being used for generating electricity, while this is not the case now; in fact a large and unknown quantity is supplied to the baths and washhouses, heating apparatus in the library, and to a deep-well pump. The whole of the exhaust steam is supplied to the baths gratis, and live steam, which is supplied *ad libitum,* is credited at £248 per annum. Steam for library heating is credited at £15 per annum; and that supplied to the deep-well pump at the baths at 1s. per hour. The whole credit for these purposes amounted for the year ending March 31, 1903, to £302. When it is remembered that the Shoreditch Baths are amongst the largest in London, and that the average coal

consumption at the baths at Islington, Camberwell, St. Mary's, Westminster, and St. George's, Hanover Square, amounts to £700 per annum, it will be seen how inadequate £248 per annum is for Shoreditch.

I have endeavoured to arrive at some idea of the true value of the electricity generated in the year from refuse. I do not think, in the light of Mr. Russell's figures for the test year, I should be justified in taking the average coal consumption per coal-generated unit at more than 9 lbs.—8 was the figure in the test year. Allowing £700 as the value of steam given to the baths, and accepting £15 for

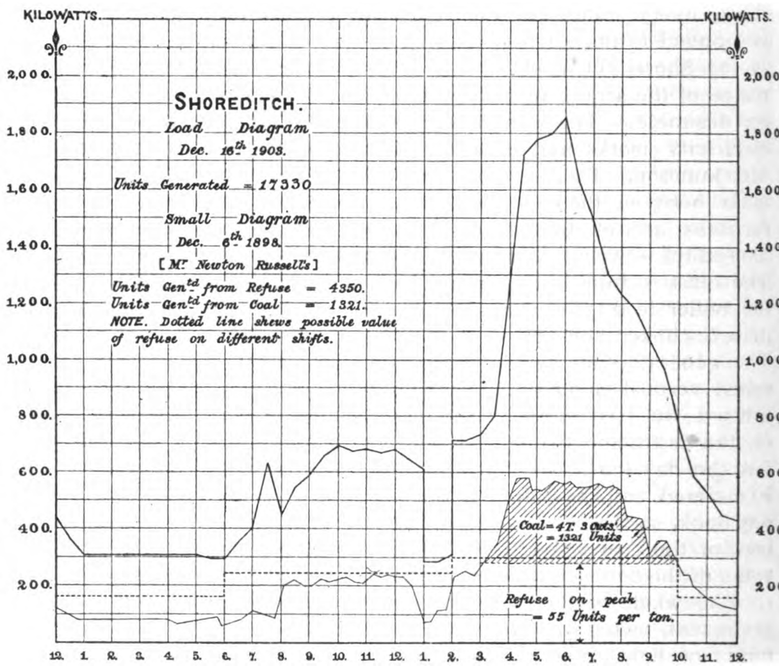


FIG. 2.

library heating and £39 for pumping as fair allowances, some 6,376 less 676 tons of coal will give 1,420,000 units generated, leaving a balance of 633,958 (out of a total for the year of 2,053,958) as generated from refuse, or 21 units per ton net—*i.e.*, after deducting units supplied to fan motors: this result was exceeded in the test year by some 7·5 units per ton.

Turning to Table VII. of Revenue, it will be seen that 2s. per ton is allowed by the Scavenging Department for destroying the refuse; adding this to other receipts and deducting the total from the total cost of destruction, a balance of £3,755 remains, and this was debited to the electricity works as the coal value of the refuse. At 9 lbs. of coal per unit generated the coal cost is

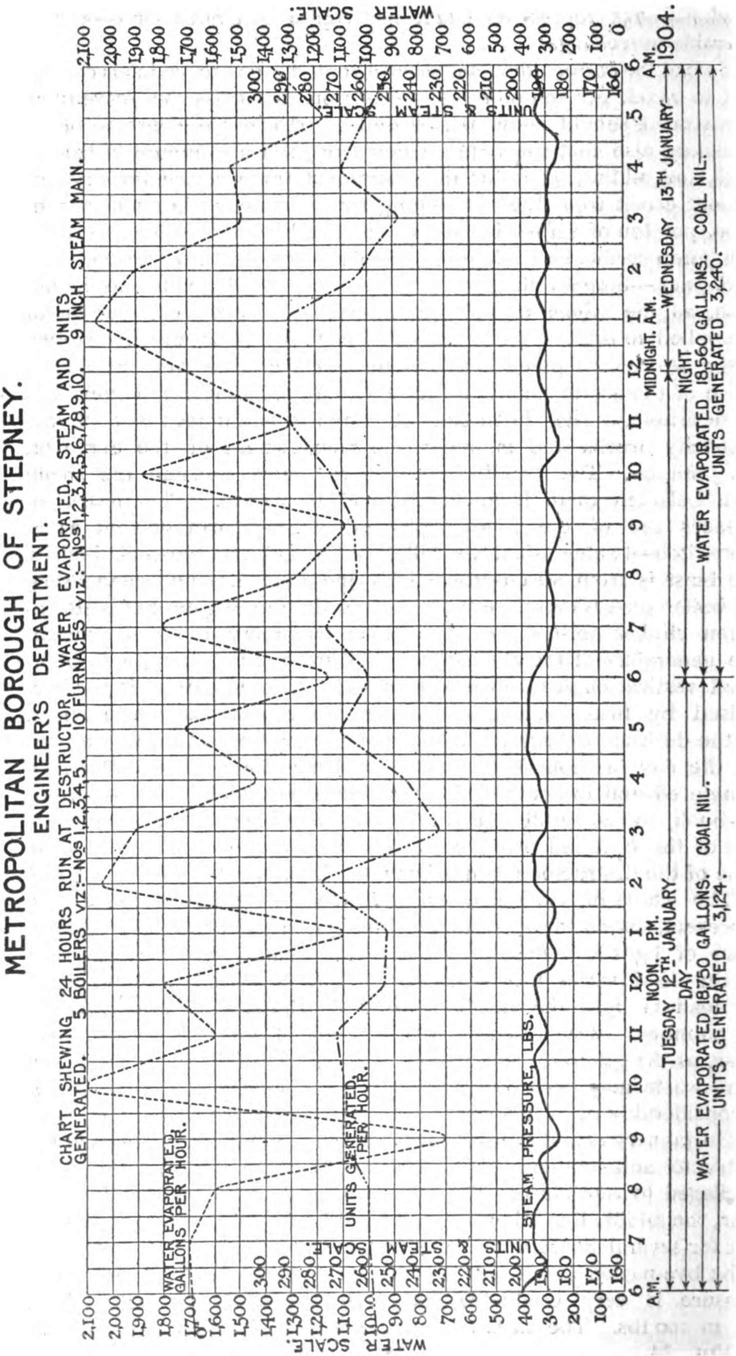
1·04d.; £3,755 represents 1·42d. per refuse generated unit—a considerable overcharge.

If 1·04d. be correct, the total charge would be £2,750, which is equivalent to 25·6d. per ton of refuse burnt; on this basis the Scavenging Department should allow Mr. Russell 2s. 4½d. per ton instead of 2s. Assuming also that the baths should pay £700 per annum instead of £258, and adding £201 for the disposal of trade refuse, the revenue without credit from the Scavenging Department would be £3,705, or 34·6d. per ton of refuse burnt; should the library heating and deep-well pump credits be inadequate, this figure should be increased.

Stepney.—Stepney does not present the same difficulties as Shoreditch, as the whole of the steam available outside the destructor is supplied to the electricity works. I have not given any description of the Shoreditch plant, as this is doubtless unnecessary, but a brief notice of the arrangements at the remaining London installations will be desirable. The destructor at Stepney is independent of the electricity works and is under the management of the Surveyor, Mr. Jameson. The buildings are on the same site, and the steam main between them is about 200 feet in length. The destructor furnaces are of the same type and general arrangement as at Shoreditch—twelve Manlove Alliott cells and six Babcock boilers. The blast is from steam-driven fans and there are no economisers, the boiler feed is therefore cold; beside the fan engines and a steam-driven clinker hoist there is no other machinery at the destructor. The generators at the electricity works are direct current, with high-speed vertical engines, non-condensing; the exhaust steam is partially utilised by feed heaters, and separate steam mains are provided for the destructor steam; generally one generating set is run throughout the day (24 hours) on destructor steam alone. The boiler feed is metered, and the output of the generator recorded in the generator log-book, so the results are readily ascertainable; and as records are kept of the coal and water consumption at the electricity works, the value of the destructor steam is known. A small battery only is in use.

The whole of the steam available from the refuse is not utilised at present, owing to the system of running on one generator. During hours of light load this takes the whole output of the works, and as the load varies the steam generation is varied to suit; when the load is light the surplus heat from the cells is bye-passed to the chimney. It will be seen that in course of time this waste should cease, as the generator will always be running at full load; even then some waste may occur, as often the available heat at the destructor is considerably in excess of the capacity of the one generator.

On January 12 and 13, 1904, Mr. Jameson took a set of readings at the destructor and set them out in the form of a curve (Fig. 3), which I am privileged to reproduce. The upper line gives water evaporation per hour, the middle line kilowatts, and the lower steam pressure. Seeing that for several hours during the day the heat from the furnaces had to be bye-passed to prevent blowing off, the regularity of the steam pressure is very creditable, the extreme variations being from 185 to 200 lbs. The variations in the supply of feed water are very



METROPOLITAN BOROUGH OF STEPNEY.
ENGINEER'S DEPARTMENT.
CHART SHEWING 24 HOURS RUN AT DESTRUCTOR, WATER EVAPORATED, STEAM AND UNITS GENERATED.
5 BOILERS VIZ:—NOS 1,2,3,4,5. 10 FURNACES VIZ:—NOS 1,2,3,4,5,6,7,8,9,10. 9 INCH STEAM MAIN.
WATER EVAPORATED GALLONS PER HOUR.
UNITS GENERATED PER HOUR.
STEAM PRESSURE, LBS.
WATER SCALE.
UNITS & STEAM SCALE.
A.M.
NOON. P.M.
MIDNIGHT. A.M.
TUESDAY 12TH JANUARY
DAY
NIGHT
WEDNESDAY 13TH JANUARY
1904.
WATER EVAPORATED 18,750 GALLONS. COAL NIL.
UNITS GENERATED 3,124.
WATER EVAPORATED 18,560 GALLONS. COAL NIL.
UNITS GENERATED 3,240.

FIG. 3.

considerable, and would appear to confirm my opinion that with larger load and a more elastic method of utilising the steam higher results would be obtained electrically. The results, however, are fairly satisfactory in view of the manner of working, and bearing in mind that economisers are not used, that the feed is cold, and that the engines are run non-condensing. The output for the twenty-four hours gives 40·2 units generated per ton, the water evaporated 1·05 lbs. per lb. at a feed temperature of about 50° F., and a water consumption of 58·7 lbs. of water per unit generated. This figure of water consumption includes boiler feed pumps, fan engines, clinker hoist, and also condensation in the 200 feet of steam main. The water consumption in the electricity works averaged about 43 lbs. per unit

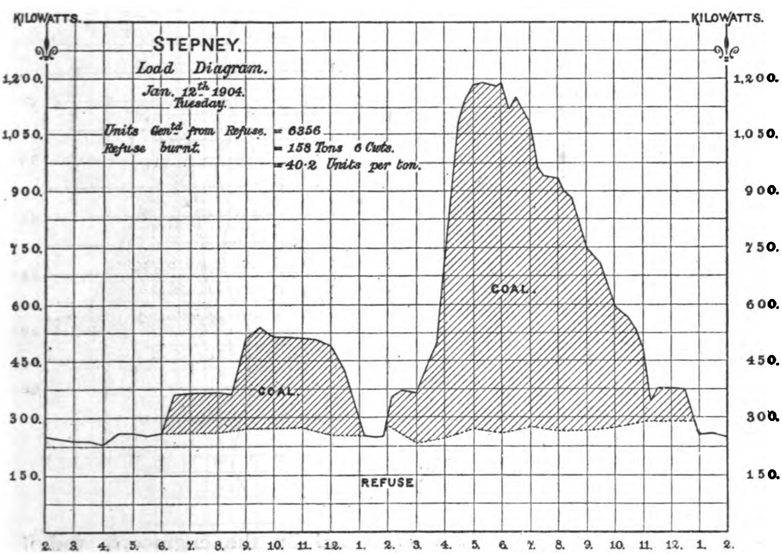


FIG. 4.

generated, including steam to boiler feed pumps, in the same week—the various losses therefore represent 27 per cent. of the total steam generated; assuming that the fan-blast could be run on 10 per cent. and the clinker hoist 5 per cent., the remaining 12 per cent. represents condensation losses. Mr. Tapper states that the steam is often very wet. The water evaporated by the destructor during the year gave an average figure of 72·5 lbs. per unit generated.

To illustrate the relation between the refuse value and electricity works load I have prepared a diagram for January 12th (Fig. 4); the uniform value of the refuse throughout the day is noticeable, and it will be seen that only during the small hours of the morning and the dinner hour can the refuse steam be counted upon to carry the station output. It would appear from this diagram that the ultimate value of the refuse

had been realised, but with a different system of running I think we may fairly conclude from Mr. Jameson's figures that a better result may be anticipated; in summer also there are several big gaps in the load, as the curve for June 18th (Fig. 5) will indicate. The solid line is the station output, and the dotted line the approximate value of the refuse under the present arrangements. The quantity of refuse destroyed and also its quality in summer and winter is unusually uniform, and Mr. Jameson considers that its average value all the year round may be taken at 1·2 lbs. per lb. from and at 212° F. The load curve for the year (Fig. 6) is remarkably like the day curve for its uniformity; the small difference between the summer and winter values is readily accounted for by the gaps in the summer load referred to. It would appear that the present value of 28·5 units per ton would readily

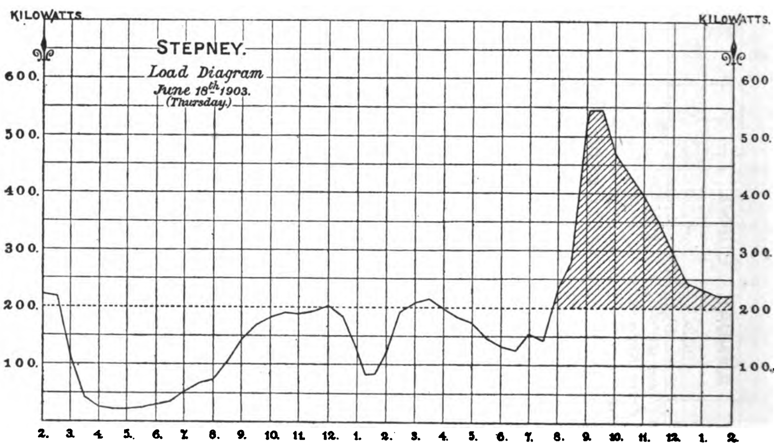


FIG. 5.

rise to 40, the ultimate value estimated by the engineers, and if improved methods are adopted this might easily reach 50.

It is a convenience to the electricity works when the whole load of the station is being carried by the destructor to be able to rely upon the supply of steam, and, when variations in the load or quality of the refuse result in a shortage of steam, coal is burnt in the destructor boilers; in the year under review 290 tons were so burnt, and probably with better economy than if steam had been kept up in coal-fired boilers. The destructor costs come out second best in the London combined stations, as will be seen by a reference to Table VI., and it is of interest to observe that the costs at the old destructor on the same site came to about 3s. 6d., or about 1s. per ton more. The present credit by the electricity works to the destructor is 0·5d. per unit generated. During the year 1902–3 the charge was first 0·3d., and then 0·5d., the average working out at 0·403d.; seeing that the cost of coal per unit generated during the same period was 0·8d., the credit was hardly adequate, as the Committee has recognised by fixing the price

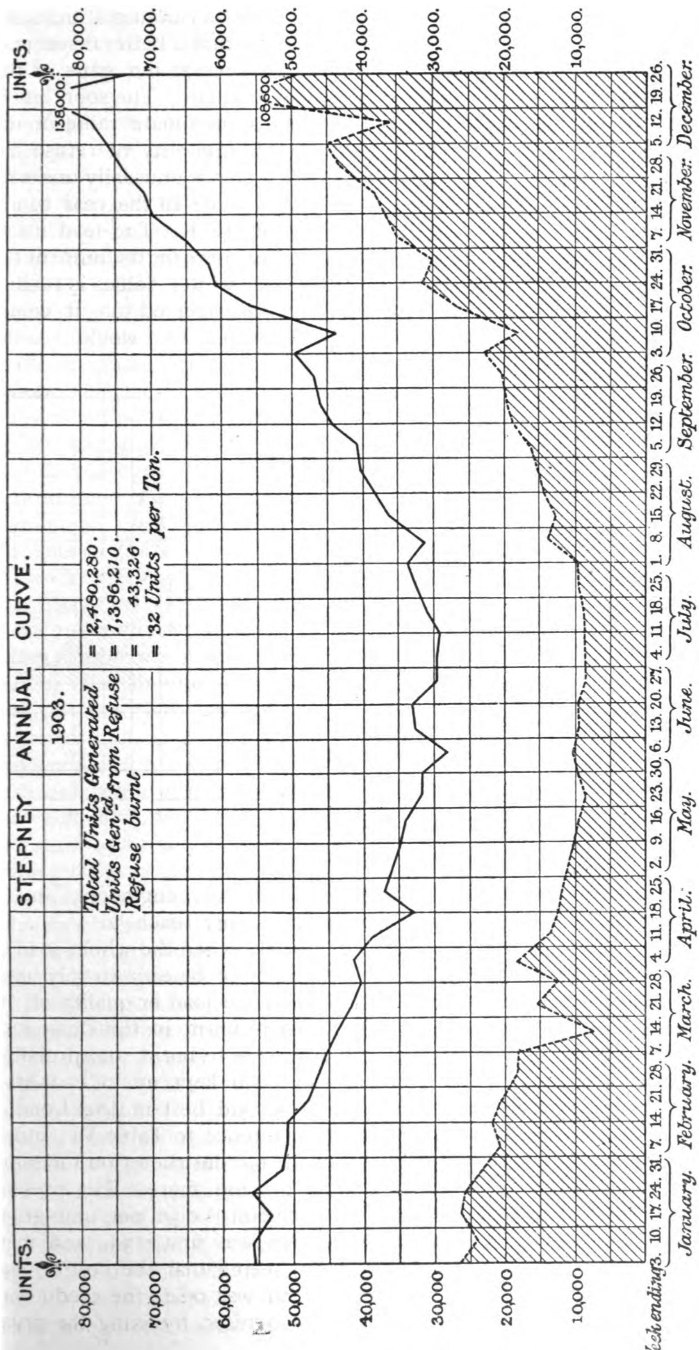

STEPNEY ANNUAL CURVE.
1903.
Total Units Generated = 2,480,280.
Units Gen^td from Refuse = 1,386,210.
Refuse burnt = 43,326.
= 32 Units per Ton.
UNITS.
80,000.
70,000.
60,000.
50,000.
40,000.
30,000.
20,000.
10,000.
138000.
108600
Week endings
3. 10. 17. 24. 31. January.
7. 14. 21. 28. February.
7. 14. 21. 28. March.
4. 11. 18. 25. April.
2. 9. 16. 23. 30. May.
6. 13. 20. 27. June.
4. 11. 18. 25. July.
1. 8. 15. 22. 29. August.
5. 12. 19. 26. September.
3. 10. 17. 24. 31. October.
7. 14. 21. 28. November.
5. 12. 19. 26. December.

FIG. 6.

at 0·5d. The capital charges on the destructor installation, including boilers, pumps, steampipes, etc., amount roughly to 0·2d. per unit, so the benefit of the destructor steam is divided equally between the destructor and the electricity works.

As the two works are to all intents and purposes independent undertakings, there is no saving in capital cost; on the contrary, assuming that one boiler would have been required in any case for the destructor and two of the remaining five have been saved to the electricity works, three extra boilers have been put down; there is also some extra cost in the steam mains between the works and in the pumps and water-softening apparatus. The old destructor chimney-stack, though a source of saving at the first, is not so now, as a second stack has been built for the electricity works.

Fulham.—These works were started in December, 1900, and the first supply of steam from the destructor boilers was given in February, 1901. The electricity works plant is two-phase alternating, the engines are horizontal by Musgraves, and, so far as possible, are run condensing. In addition to three 300-k.w. generators, there are three 100-k.w. direct-current exciters, used largely for lighting the works and driving motors on the works. There are five 20-H.P. electrically driven centrifugal pumps, one deep-well 20-H.P. electric pump, four 10-H.P. motors for driving mechanical stokers and coal conveyor, two 5-H.P. motors for driving economiser scrapers, and there has recently been added a 30-H.P. Belliss engine operating a fan at the base of the chimney-stack for assisting the draught; this runs continuously, sometimes non-condensing. The destructor consists of twelve Horsfall cells in two groups of six, arranged back to back in threes. Between the two groups there are six Babcock boilers. Mechanical stokers are provided and much care has been taken to render the fronts of the boilers air-tight when under steam from destructor gases. The main flue is of considerable length. Blast is provided to the destructor cells by steam jets of the usual Horsfall type, and on test the steam consumption was 17·8 per cent. of the total steam generated. Economisers are provided in the main flue. At the outset the six destructor boilers were the only boilers installed; three marine-type dryback have been added. In addition to the electric generators, a supply of steam is given to a steam disinfector some 40 yards away, also to a clinker-crushing and brick-making plant. The clinker crusher is driven by an 8-B.H.P. simple vertical engine. This engine is run 54 hours per week, and is supplied through 80 feet of 1¼-in. steampipe not lagged; it probably does not take less than 50 lbs. of steam per B.H.P. hour. The brick-making plant takes an unknown quantity of steam; the disinfector is generally in use some hours every day, and, although not liberal, £150 per annum is a fair charge for steam supplied. The Belliss fan engine is supplied through 100 feet of branch piping, and probably takes 30 lbs. of steam per B.H.P. on the average; this plant was started in October, and has been running continuously since November, 1903.

I have gone into some detail with regard to the plant installed, as this has an important bearing upon the results. The steam-raising test when the plant was newly installed gave 1·15 lbs. of steam per lb. of

refuse. The load was absorbed electrically, and an average of 60 units per ton was obtained throughout a day's run.

The water consumption is not often measured, a large part being taken from a deep well in the works, but a test was taken in October, 1903—when the destructor was not running—from 2 p.m. on Saturday to 9 a.m. on Monday, and the average water consumption was 59 lbs. per unit generated. The steam disinfector, the brick-making plant, and the clinker-crushing engine were shut down during this test, but the Belliss fan engine was in use.

In the case of Fulham my investigations began with a curve for the year 1902–3 (Fig. 7). The average units per ton given me by Mr. Fuller over the year ending March, 1903, amounting to 25·5 aroused my curiosity as to the relative figures obtained in summer and winter. I judged from the arrangement of the plant and the smallness of the day load in summer that probably the 25·5 units would be largely exceeded, and when the curve was set out and the carefully estimated coal value filled in, my suspicions were amply confirmed. I have taken great pains to obtain correct coal values, and although the average works out at 9 lbs. per unit generated, I am satisfied that this is correct. In the earlier part of the year—from June to September—I have ample evidence to show that 12 lbs. of coal were used per unit generated. From September to November the condensing plant was out of use, and the coal consumption has been taken at 10 lbs. per unit; from November onwards 8 lbs. is the ascertained figure, and, in view of the water consumption test in October, 1903, I have taken this value in all the later day-load curves. I wish to draw special attention, while this annual curve is before you, to the increasing value obtained as the output grew. It is also important to observe that the whole of the coal consumed on the works has been plotted, and that steam supplied to the clinker, brick plant, and disinfector is *not accounted for* in the diagram. The Belliss fan engine had not been installed at that period. The large dip in December is caused by the Christmas holidays; the big rise between January 15th and 22nd is due to the switching on of one hundred extra arcs for the street lighting. This increased public lighting load has doubtless some bearing on the fine results now obtained. In view of the undoubtedly very heavy water consumption when the destructor is working, 53 units per ton throughout the week is a very fine record figure.

As the refuse is delivered it is tipped on to a platform above the cells and burnt steadily throughout the twenty-four hours, no attempt being made to burn extra refuse during the peak. The delivery of refuse begins on Monday generally about 9 a.m., and ceases about midday Saturday. The destructor is generally entirely shut down from 11 p.m. on Saturday until the early hours of Monday morning; before the new refuse begins to come in, the cells are started on such refuse as was left over from Saturday. It has been the practice since the works started to weigh carefully the coal used on each shift, so the values of the refuse are readily obtainable on each shift. It will be seen from the diagram (Fig. 8) for Monday, December 15, 1902, that the day load is small, the early morning load large comparatively, and of course the peak shift the heaviest. On this day the cells were started at 1 a.m., and

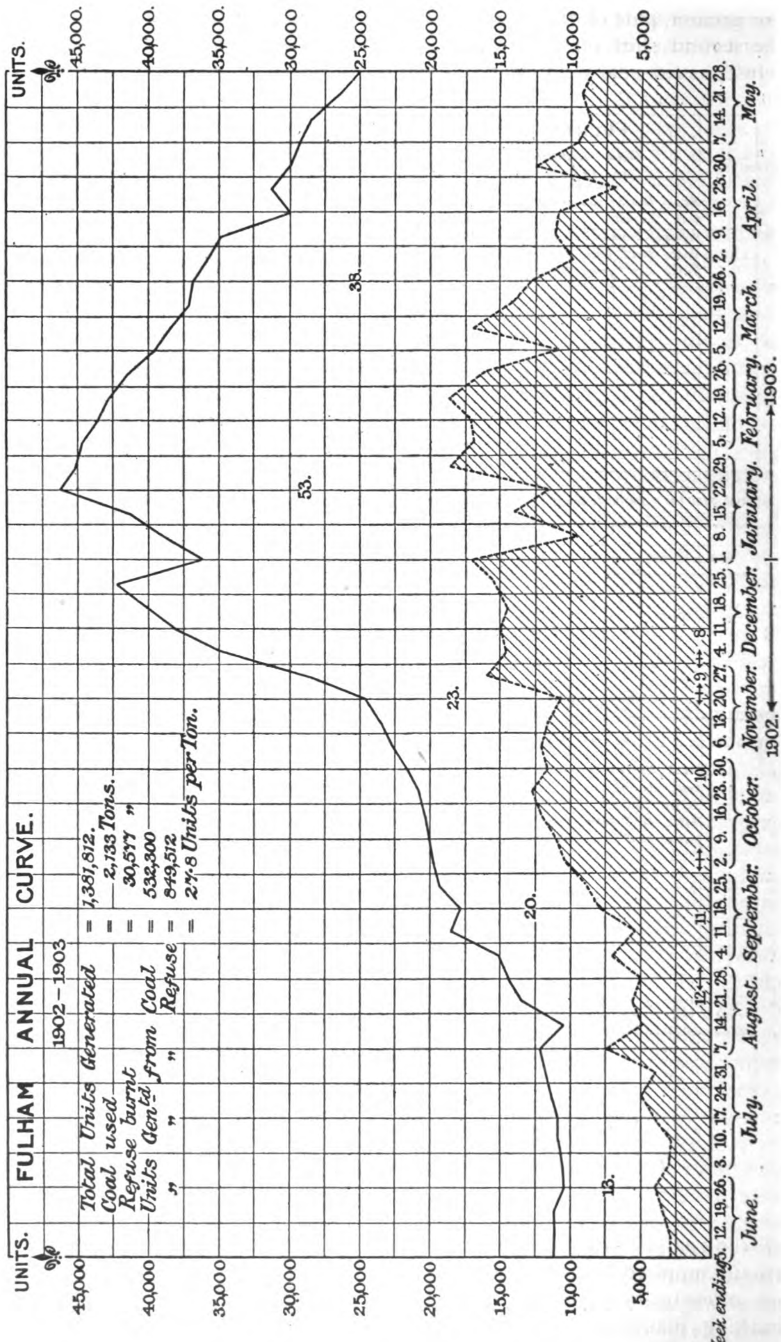

FULHAM ANNUAL CURVE.
1902 – 1903
Total Units Generated = 1,381,812.
Coal used = 2,133 Tons.
Refuse burnt " = 30,577 "
Units Gen'td from Coal = 532,300
" " " Refuse = 849,512
" " " " = 27·8 Units per Ton.
UNITS.
45,000
40,000
35,000
30,000
25,000
20,000
15,000
10,000
5,000
Week ending
June.
July.
August.
September.
October.
November.
December.
January.
February.
March.
April.
May.
1902.
1903.

FIG. 7.

the greater part of the load was run on coal during the first shift; on the second shift no value appears to have been obtained from the refuse, as the coal works out at about 9 lbs. per unit generated; 10 cwt.

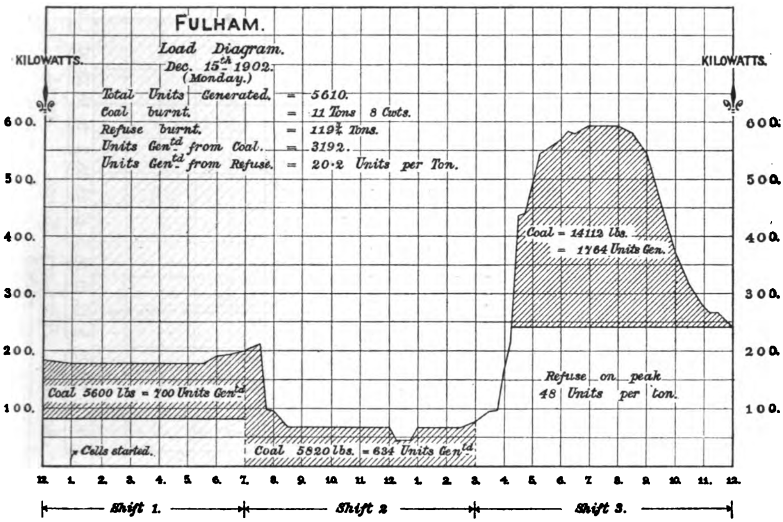


FIG. 8.

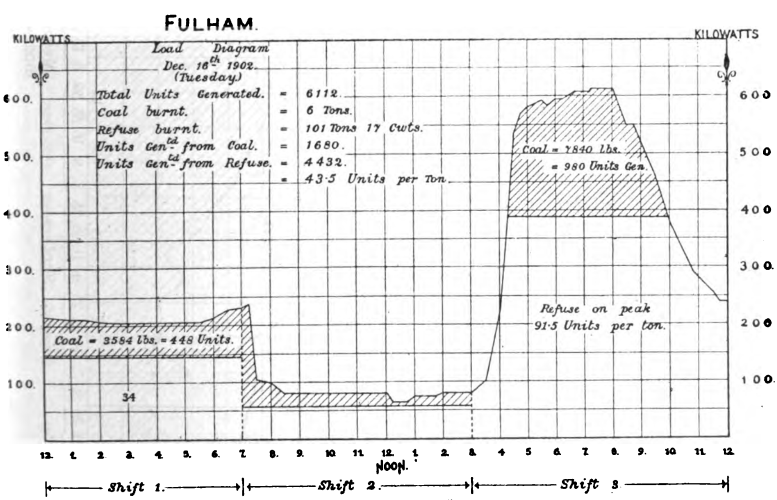


FIG. 9.

of the coal, however, was used for banking, and no account is taken of steam supplied to the disinfector from 9.30 to 4.45. In *all the curves no allowance* is made for steam supplied to the disinfector, brick-making plant, etc., and the coal is worked out at 8 lbs. per unit

throughout. On Mondays a poor value is obtained for the refuse, as the whole of the cells and flues have to be heated up from cold, and

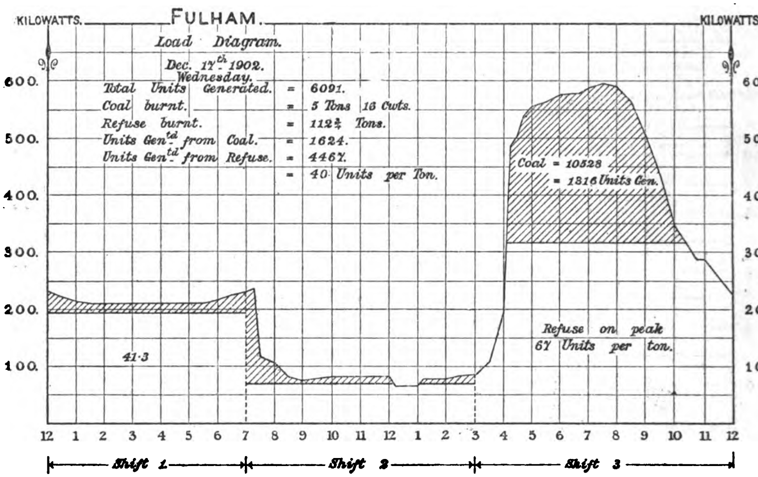


FIG. 10.

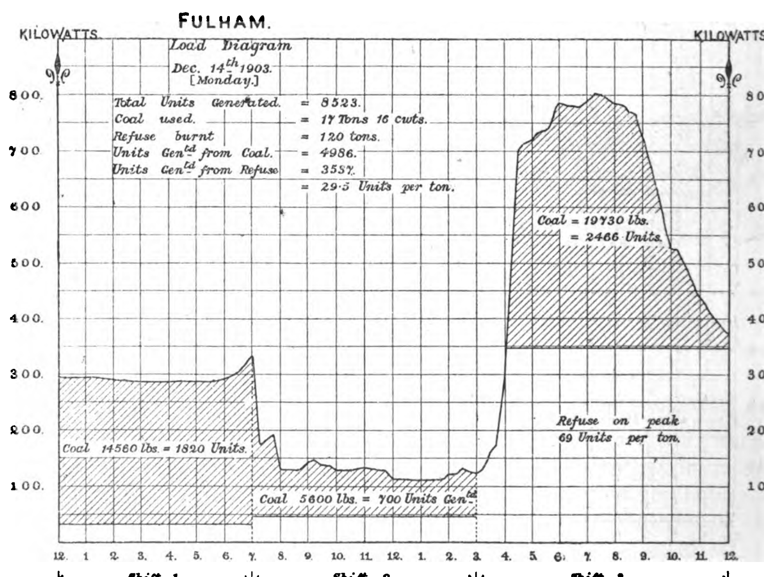


FIG. 11.

on Tuesday full value does not usually seem to be obtained, although (Fig. 9) December 16, 1902, shows an almost record value. Bearing in mind the maximum of 55 units per ton obtained by Mr. Russell at Shoreditch on test, this figure of 91·5 units per ton is not a little remarkable, as

it was obtained under ordinary working conditions, and until I drew out the curve such a result was not even suspected. I may say that most of the curves from Fulham were selected almost at random from the log-books, and had never been plotted before with the coal and refuse values filled in.

The first shift for December 16, 1902, gave 34 units per ton, a considerable improvement on the Monday. On the first two shifts on Wednesday (Fig. 10) it will be seen that the improvement is continued, although on the third shift only 67 units per ton is obtained. The whole of the coal on the second shift was used for banking, and the disinfector was in use from 9.15 a.m. to 4.45.

A second series of curves (Figs. 11, 12, 13, and 14) taken a year later

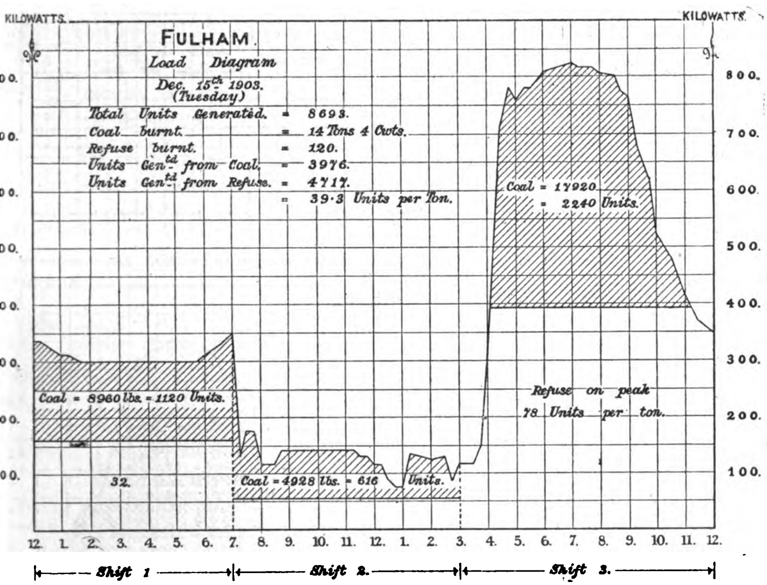


FIG. 12.

shows very similar results, but the highest value obtained on the peak is 78 units per ton; the average value, however, is some five units per ton higher, notwithstanding the addition of the Belliss fan engine and of the brick-making and clinker plant. On both first and second shifts the load has largely increased, and a better value is obtained in consequence.

To bring the diagrams up to date I have prepared a series in January this year (Figs. 15, 16, and 17), and in these will be found the best results; an average over three days of 50·4 units per ton is good, but the record value on the peak load of January 9th of 94 units per ton is very fine, and in view of the small day load an average for the day of 52·3 units is highly satisfactory. After studying these curves the first question that will be asked is, why, if such splendid results are obtained on the peak, are not better

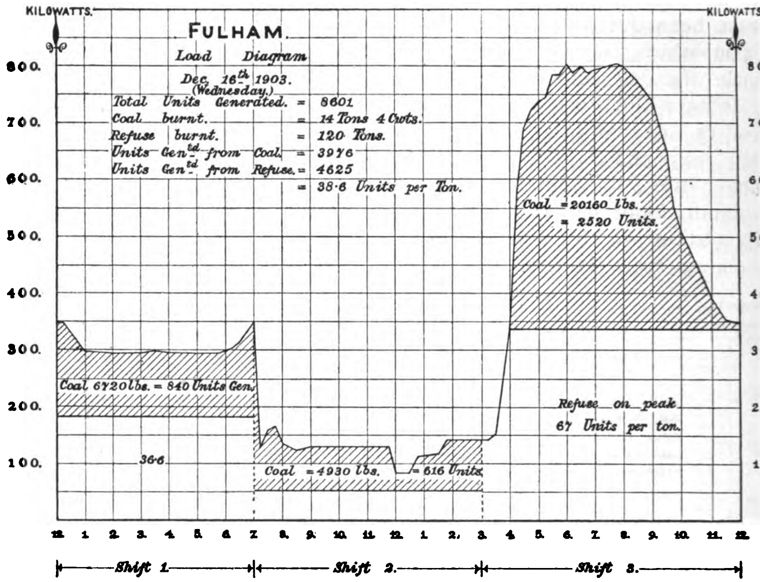

KILOWATTS.
FULHAM.
Load Diagram
Dec. 16th 1903.
(Wednesday.)
Total Units Generated. = 8601
Coal burnt. = 14 Tons 4 Cwts.
Refuse burnt. = 120 Tons.
Units Gentd from Coal. = 3976
Units Gentd from Refuse. = 4625
= 38·6 Units per Ton.
Coal = 20160 lbs.
= 2520 Units.
Coal 6720 lbs. = 840 Units Gen.
Refuse on peak
67 Units per ton.
36·6
Coal = 4930 lbs.
= 616 Units.
800. 700. 600. 500. 400. 300. 200. 100.
12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12.
Shift 1. Shift 2. Shift 3.

FIG. 13.

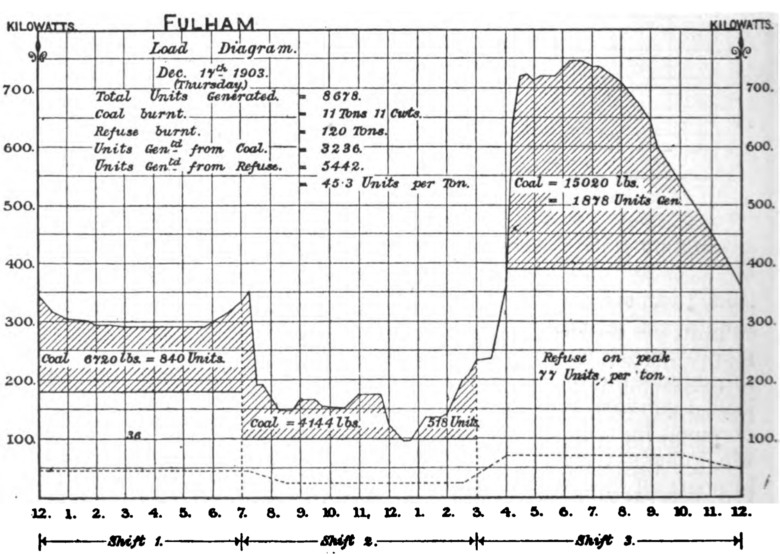

KILOWATTS.
FULHAM
Load Diagram.
Dec. 17th 1903.
(Thursday.)
Total Units Generated. = 8678.
Coal burnt. = 11 Tons 11 Cwts.
Refuse burnt. = 120 Tons.
Units Gentd from Coal. = 3236.
Units Gentd from Refuse. = 5442.
= 45·3 Units per Ton.
Coal = 15020 lbs.
= 1878 Units Gen.
Coal 6720 lbs. = 840 Units.
Refuse on peak
77 Units per ton.
36
Coal = 4144 lbs.
518 Units.
700. 600. 500. 400. 300. 200. 100.
12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12.
Shift 1. Shift 2. Shift 3.

FIG. 14.

figures recorded on the two first shifts? Air leakages, I am given to understand, are largely responsible for this; troublesome cracks have been developed in the flues through the shrinkage of the soil upon which they are built, and, notwithstanding the care taken to make the destructor boiler fronts tight, considerable quantities of air pass into the flues, thus cooling the destructor gases. When the boilers are coal-fired this air is utilised in combustion in the usual way, and thus, the larger the load, *i.e.*, the more coal burnt, the better the value obtained from the refuse. The air leakages are without doubt serious, and are engaging the attention of Mr. Fuller. In addition to this a fairly large proportion of the coal is used for banking spare boilers; this would appear to be a necessity in an

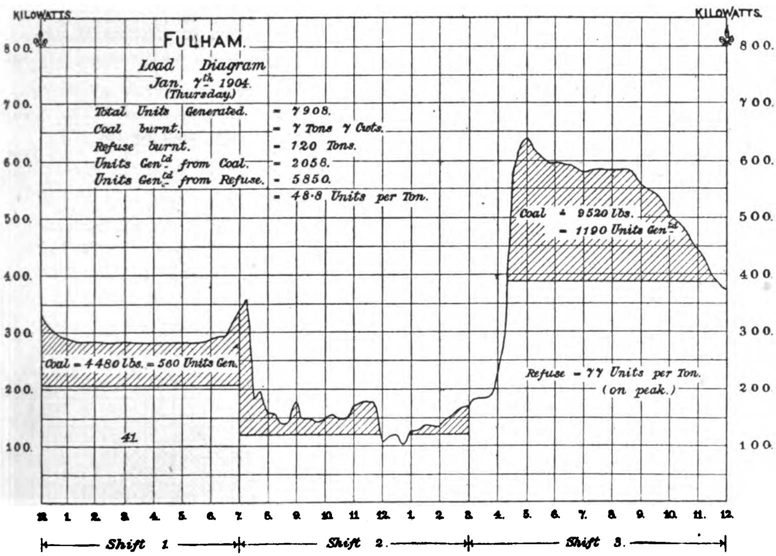


FIG. 15.

alternating station where a convenient battery is not available in case of necessity. In course of time, with a larger day and early morning load, I can see no reason why the fine values obtained on the peak load should not be maintained throughout the day, and, if it were at all possible to avoid shutting down on Sundays for cleaning purposes, throughout the week. Under existing conditions, but with a large day load, I consider that some 70–80 units per ton ought eventually to be obtained in winter and some 50–60 units in summer. In deference to Mr. Fuller's wishes, I have given in Table IV. 52 units per ton as the ultimate all the year round average. As I have nothing at stake in the matter except, perhaps, my reputation as a prophet, I should have put the figure some 25 per cent. higher; the average value obtained on the peak on the ten curves I have plotted is 75, and this at the present high water consumption.

During the year 1902-3 the clinker obtained was some 50 per cent.

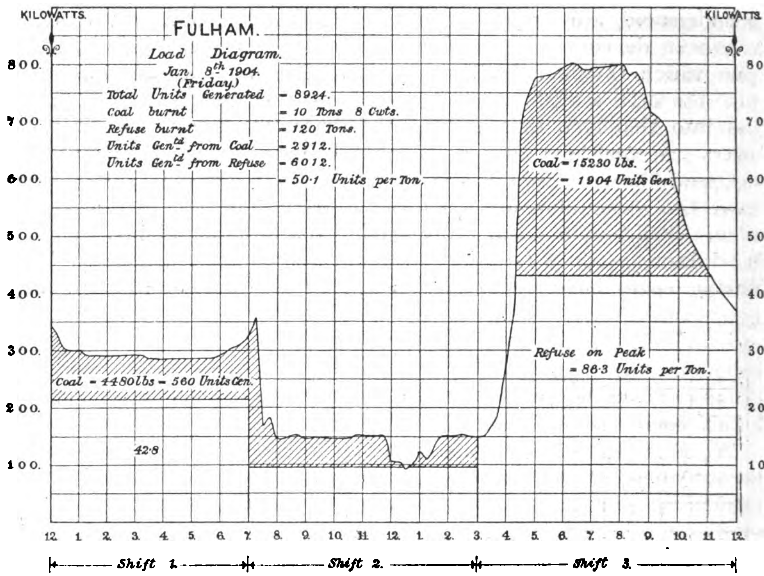

KILOWATTS.
FULHAM.
Load Diagram.
Jan. 8th 1904.
(Friday)
Total Units Generated - 8924.
Coal burnt - 10 Tons 8 Cwts.
Refuse burnt - 120 Tons.
Units Gentd from Coal - 2912.
Units Gentd from Refuse - 6012.
- 50·1 Units per Ton.
Coal = 15230 lbs.
= 1904 Units Gen.
Refuse on Peak
= 86·3 Units per Ton.
Coal - 4480 lbs - 560 Units Gen.
42·8
800. 700. 600. 500. 400. 300. 200. 100.
12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12.
Shift 1. Shift 2. Shift 3.

FIG. 16.

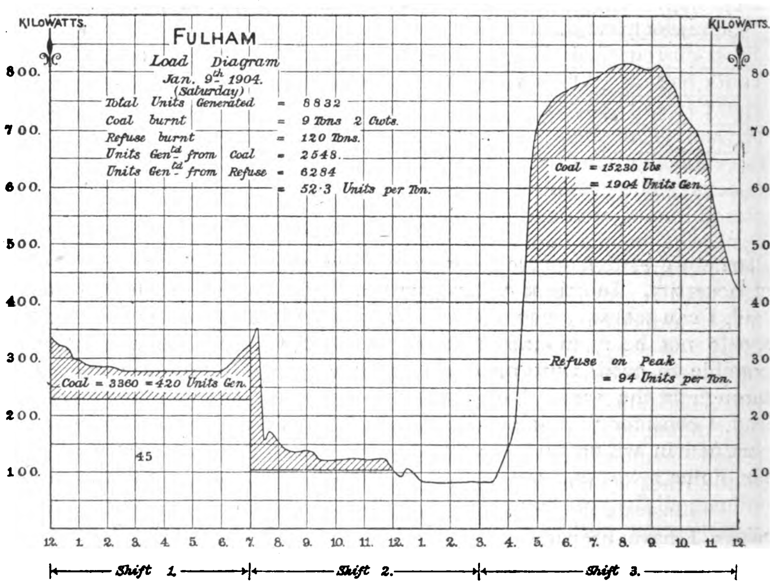

KILOWATTS.
FULHAM
Load Diagram
Jan. 9th 1904.
(Saturday)
Total Units Generated = 8832
Coal burnt = 9 Tons 2 Cwts.
Refuse burnt = 120 Tons.
Units Gentd from Coal = 2548.
Units Gentd from Refuse = 6284
= 52·3 Units per Ton.
Coal = 15230 lbs
= 1904 Units Gen.
Refuse on Peak
= 94 Units per Ton.
Coal = 3360 = 420 Units Gen.
45
800. 700. 600. 500. 400. 300. 200. 100.
12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12. 1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12.
Shift 1. Shift 2. Shift 3.

FIG. 17.

of the refuse burnt. Mr. Fuller tells me he has now reduced this to about one-third, and sometimes he gets as little as 28 per cent. This clinker is now being sold at a profit, so that on the present year's returns the rather heavy item for cost of disposal will disappear. A reference to Table VIII. will show that the credit for electricity is about 20 per cent. less than the ascertained coal value. The costs (see Tables VI. and VII.), after deducting revenue, amount to 26·3d. per ton; this deficiency is met by a credit from the Health Department. The ultimate value of the steam based on the present coal costs amounts to about £5,000; it is probable that the cost of repairs will increase, but after making a reasonable allowance in this connection the figures would seem to justify one in concluding that when the full heat value of the refuse is utilised, the Fulham destructor should earn a profit of approximately £1,000 a year after all costs of destruction have been paid. Such profit as results from the sale of clinker should be added. Fulham at present holds the record for lowest total costs of destruction in the London combined works.

Hackney.—As already stated, I have taken great pains to ascertain the actual coal consumed per unit generated at Fulham, but as no regular record is kept of the water consumption, it may perhaps be suggested that I have taken too high a figure in basing my calculations on 8 lbs. I am quite satisfied as to the correctness of this figure, but owing to the element of uncertainty, I turn with the greater pleasure to the Hackney records, for here the water is metered and the coal consumption ascertained. In addition to this, the figures of the water and coal consumption are quite exceptional.

The electrical plant at Hackney consists of high-speed engines and direct-current generators, and an interesting feature is the 1,200-unit Tudor battery. The plant is run condensing, and 30 lbs. of water are used per unit generated. The average coal consumption is about 5 lbs. per unit generated. The Hughes & Stirling destructor consists of three 4-cell groups, with a combustion chamber between each two pairs of cells. The combustion chambers lead direct to three Babcock boilers placed immediately behind the cells. The refuse is tipped into hoppers, from whence it is raised by elevators into three large bins above the cells; from these bins it is drawn as required and fed into the cells. The blast is provided by electrically driven fans, and is not heated.

The annual curve (Fig. 18) is not dissimilar from the Fulham; a better value, however, is obtained in the summer, and the Fulham record of 53 units per ton throughout the week is beaten by the Hackney 60 units per ton. The units per ton throughout the year amount to 41·1, as against the Fulham 27·8. It should be observed that the curves cover the same period—namely, from June, 1902 to May, 1903. The coal in this and all the Hackney curves is plotted in at 5 lbs. per unit generated.

To illustrate the varying values obtained on different days in the week, I have prepared a diagram (Fig. 19) giving particulars of four weeks. Owing to the extremely wet weather experienced during 1903, the records obtained at the end of 1902 have not, so far as I have been able to ascertain, been exceeded. The week ending November 8th was remarkable for the high average of 71 units per ton obtained on

the Wednesday, and for the fact that on the Thursday the whole load was taken by the destructor. It should be understood that all my curves give units generated on the day in question. The week ending December 20, 1902 is, I believe, the record week, and it will be seen what fine values were obtained on the different days. The two lower charts show two consecutive weeks in January, 1904, but the results, although good, are not remarkable, probably owing to the wetness of the weather.

The chief interest, however, lies in the Hackney day load diagrams, for at present the day load is small and the peak value almost beyond belief. The method of burning the refuse differs slightly from that in use at Fulham, in that a varying number of cells is used when occasion requires. The refuse is, however, burnt uniformly, without any

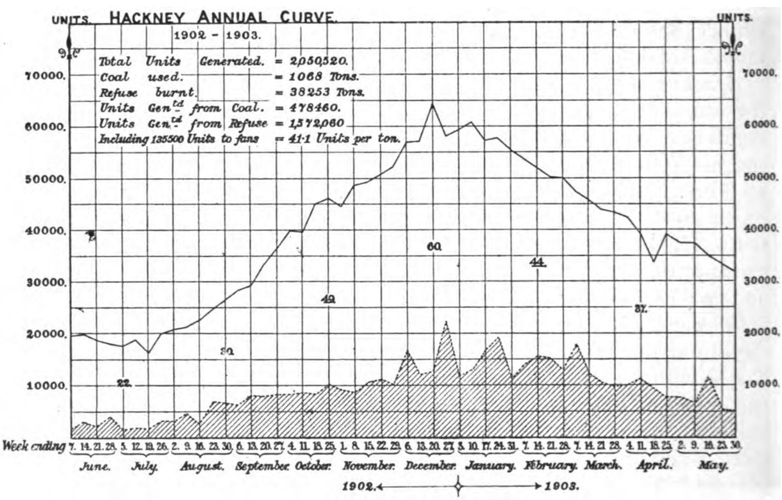


FIG. 18.

attempt to force the cells during the hours of the heavy load. The rate of burning per cell-hour varies very little. The cells are shut down early on Sunday morning, when the refuse which has been collected during the week is exhausted, and started again on Monday morning as soon as the new supply of refuse begins to come in. On November 6, 1902—a Thursday, which is closing day at Hackney—the whole output was carried on refuse steam (Fig. 20). The highest value on the peak lasting about two hours amounted to no less than 104·3 units per ton. The rate of burning is shown at the base of the diagram. Eight cells were burning from 12 to 8 a.m., twelve cells onwards to 10 p.m., and from 10 to 12 eight cells. Although the refuse was being burned steadily, the generating plant was shut down from 6. a.m. to 1 p.m. The three diagrams (Figs. 21, 22, and 23) for December 16th, 17th and 18th, 1902, show that the high value of 104 units per ton is not

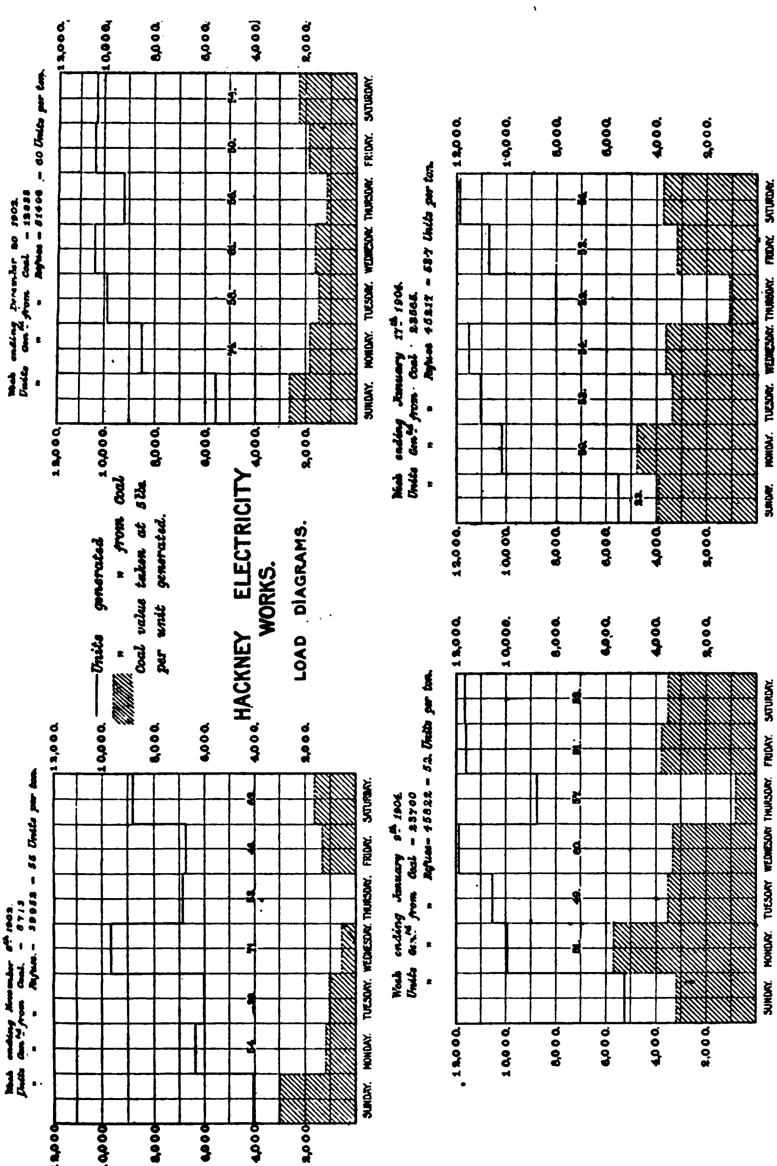

HACKNEY ELECTRICITY WORKS.
LOAD DIAGRAMS.
Units generated
" " from Coal
Coal value taken at 5 lbs. per unit generated.
SUNDAY. MONDAY. TUESDAY. WEDNESDAY. THURSDAY. FRIDAY. SATURDAY.
12,000. 10,000. 8,000. 6,000. 4,000. 2,000.

FIG. 19.

altogether exceptional, the average for the three days amounting to 102·1 units per ton. On each of these days the refuse was burnt uniformly throughout in 10 cells, and the generating plant was entirely shut down for four or five hours. The value obtained on December 17th of 105·8 units per ton on the peak load is the highest record which has come to my notice. I desire to repeat that these figures were obtained under ordinary working conditions, and without any one being aware that such fine results were being realised until the curves had been drawn out. Given a sufficiently large load such as that obtained at Shoreditch, there really seems to be no reason why 100 units per ton should not be reached throughout the day.

After observing the splendid value obtained on one of the Fulham

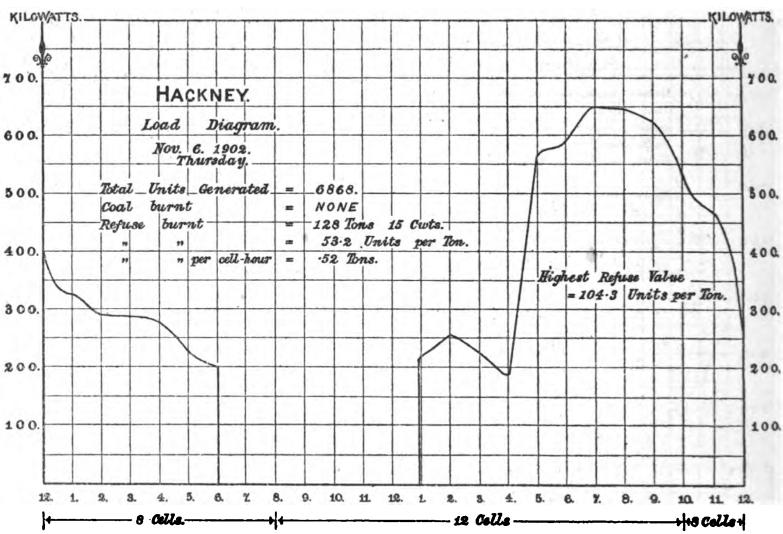


FIG. 20.

curves, I naturally anticipated better results would be found at Hackney, and, comparing the relative water consumption, if 90 units per ton is obtained at Fulham, 120 or even more ought not to be an impossible figure at Hackney. Air leakages are, I believe, not unknown at these works, and may perhaps account for reduced efficiency.

I have thought that a diagram showing the value of the large battery at Hackney might be useful. The diagram of December 18, 1902 (Fig. 23), gives the total load on the station, including feeder output. The solid line gives units generated, and the dotted line the station output. The battery charge and discharge is indicated by the shaded portions, on the top of the peak the average load taken is about 160 k.w. and the maximum 220 k.w.; roughly the discharge on the top of the peak represents 600 units, about half the nominal capacity of the battery. Had the full capacity of the battery

been utilised the load carried on the peak would have been 250 k.w., and a very small quantity of coal would have been required. As it is the diagram shows that about 40 per cent. of the coal which would be necessary without the battery has been saved.

A reference to Table IV. will show that for the year ending March, 1903 some 37 units per ton of refuse were generated after deducting the electricity used in the fan motors. As the average of steam-raising value is put at 1 lb. per lb. all the year round, and the water evaporated during the last financial year was only 0·54 lb. per lb., it seems fairly safe to assume that an average figure of 64 units per ton will be

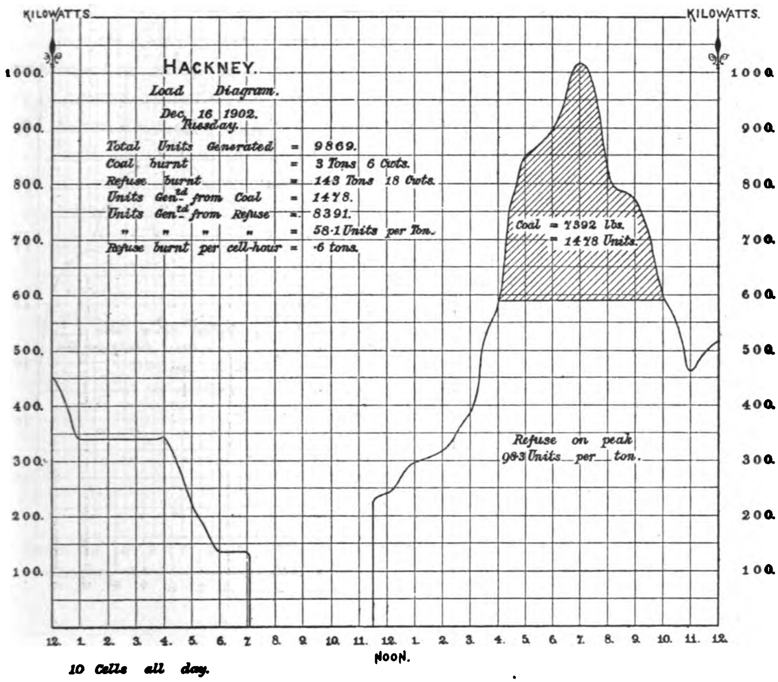


FIG. 21.

obtained ultimately. Personally, I shall be surprised if in the course of a few years this figure is not considerably exceeded. In this connection it is not a little interesting to note that a value of 105 units per ton at 30 lbs. of water per unit means an evaporation of 1·4 lbs. of water per lb. of refuse.

The destructor costs for the last financial year are high; the wages correspond curiously with those at Fulham, the clinker disposal formed a considerable item of expense, and as the destructor was running only 11 months during the year, the capital charges per ton were heavier than will be the case in future. The credit to the destructor for electricity is based on the coal cost, and is therefore absolutely fair.

As in the case of Fulham, the difference between the destructor revenue and total costs, namely, 35·4d. per ton destroyed, is met by a credit from the Health Department. Owing to the Hackney destructor having a less favourable market for its steam, it does not seem likely it will earn a profit as will probably be the case at Fulham.

Bermondsey.—The smallest of the London combined stations has successfully completed a year of working. The returns, however, have not yet been published, and at the time of writing are not available.

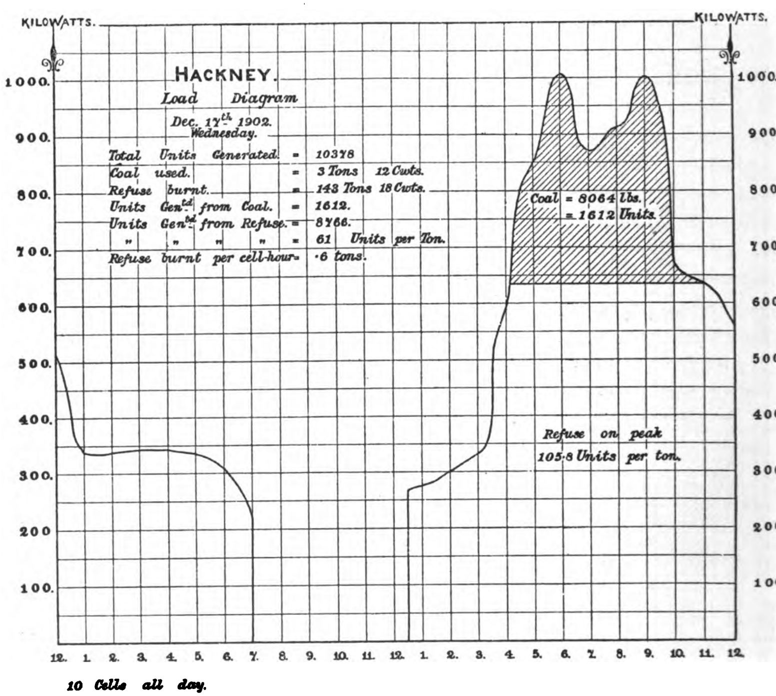


FIG. 22.

Mr. Vincent has, however, kindly placed at my disposal information which will prove of interest.

The plant consists of high-speed vertical engines with direct-current generators; no use is made of the exhaust steam. The battery is small, being of only 240 units capacity. The Hughes & Stirling destructor consists of three pairs of cells with three Babcock boilers. A combustion chamber is provided between each pair, which leads into a flue at the back of the cells and so to the boilers. This arrangement is not quite so favourable as that at Hackney. The blast is provided by electrically driven fans, which only take 6 per cent. of the total quantity of electricity generated. In addition to supplying steam to the electric generators a supply is given to the adjoining baths and

washhouses. The coal bill at the baths before the erection of the destructor was between £400 and £500 per annum; two stokers were also employed whose wages amounted to about £150 a year. The coal burnt at the electricity works was only 400 tons for the year, thus, altogether apart from the electricity works, a saving of about £600 less £380—the value of the coal—has been effected by the introduction of the destructor; it will thus be seen that the whole of the electricity generated may be credited to the refuse. After deducting the units used by fan motors, the very creditable figure of 29·8 units has been

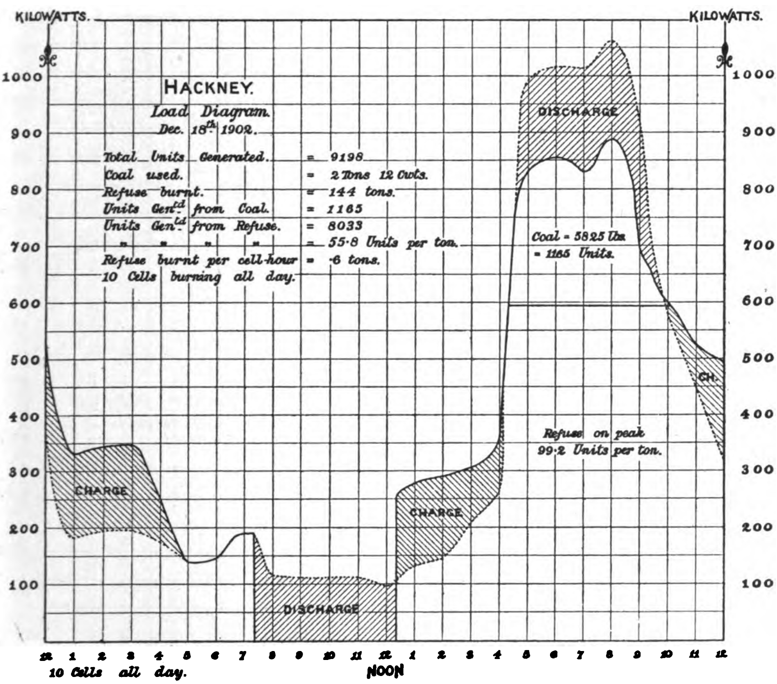
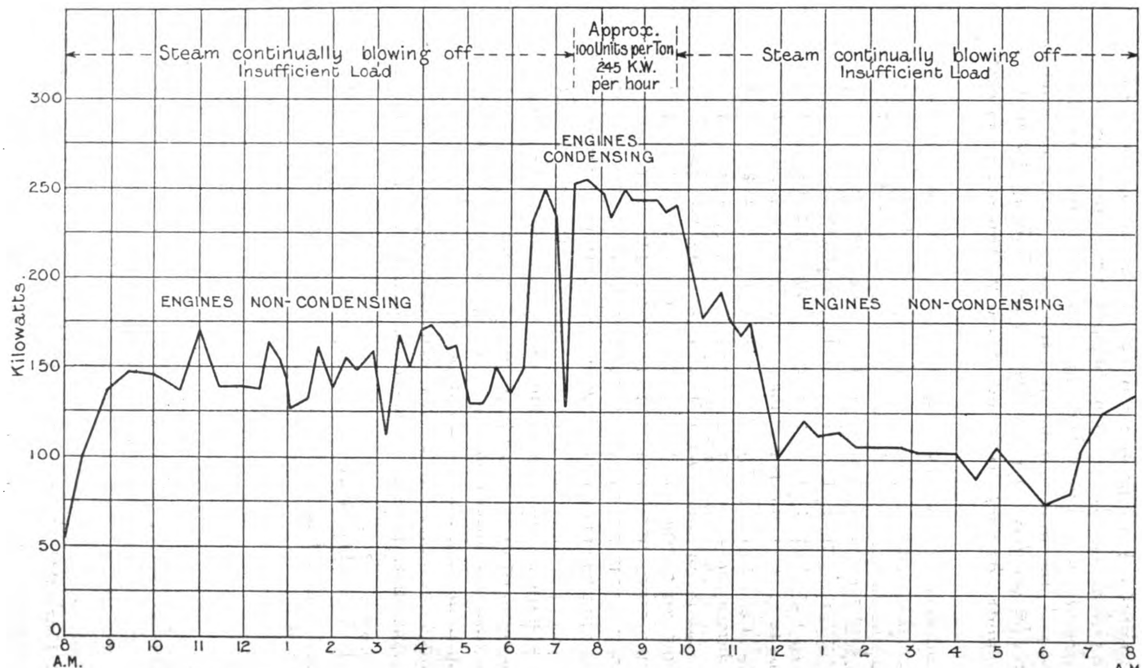


FIG. 23.

obtained per ton of refuse, a figure second only to Hackney. On the official test the fine value of 1·65 lbs. of water were obtained per lb. of refuse, and in view of the high calorific value of the Bermondsey refuse, Mr. Vincent anticipates that some 86 units per ton is the ultimate value of the refuse. The credit given to the destructor of 1d. per unit generated, or 1·09d. per unit sold, is undoubtedly high, but the works are unfavourably situated, the price of coal is high, and condensing is impossible. Compared, therefore, with the average for the six uncombined London undertakings of 0·93d. given in Table VIII., the figure is not so extravagant as would appear at first sight. In view of the high coal value, the Bermondsey destructor should earn a substantial profit when the full heat value of the refuse is utilised.



TOTAL REFUSE BURNED IN 24 HOURS 24 MINUTES = 63¼ TONS = AVERAGE RATE OF 2·6 TONS PER HOUR.

Note.—While the engines were running non-condensing—*i.e.*, when the load was very light—the boiler was continually blowing off. The electrical output per ton of refuse for 2¼ hours—*i.e.*, during maximum load—was just under 100 kilowatts, while the output per hour was 245 kilowatts.

Total evaporation from and at 212° F. = 1·917 lbs. of water per lb. of refuse
Feed water delivered to boiler per hour = 950·4 gallons.

FIG. 24.—Woolwich Destructor and Electricity Station. Chart giving Kilowatts Generated during a 24 Hours Test, carried out by The National Boiler and General Insurance Co.

Woolwich.—The combined electricity and destructor works here have so recently been added to the list of Metropolitan undertakings, that returns for a year of working are not available. I am able, however, in Fig. 24 to show the record of an excellent test taken early this year; with a water consumption of 40 lbs. per unit generated, the fine figure of 100 units per ton were generated for two hours continuously; with the same economical plant as at Hackney, this figure would have been 133 units per ton. If Mr. Mitchell can bring his water consumption down, he may yet beat all records. The destructor is a Meldrum Bros. "Simplex."

Provincial Undertakings.—I can only deal briefly with one or two of the smaller undertakings, as I find that my paper has already run into far more space than I had anticipated. I have, however, tabulated a large mass of information which has been very kindly provided by various Electrical and Borough Engineers.

Wimbledon.—From the figures given to me by Mr. Tomlinson Lee, it is plain that these works should not be included in the list of combined undertakings. The joint works adjoin the sewage works, and the whole of the steam available from the refuse is used for sewage pumping, and heating the hospital some 600 yards away from the works. The coal saved to the sewage works amounts to £900 a year, and Mr. Lee states that beyond what the destructor can do he provides coal-generated steam to the value of about £5 per annum in addition; this being so, the value of the steam in terms of possible units of electricity generated can only be estimated. On the basis of the present coal cost, namely, 0·9d. per unit sold, the units generated would amount to 300,000. From another source I understand that it has been estimated at 45 units per ton. Taking the former figure, 33 units per ton is a highly creditable performance, seeing that the refuse is burnt with pressed sewage sludge in the proportion of one ton of sludge to two of refuse. The large percentage of moisture and the very small percentage of combustible matter in sludge is well known. The furnaces are of the Beamen & Deas type, with Babcock boilers.

Grays.—Through the courtesy of Mr. Long and Mr. Arthur Preece I have been provided with very full information regarding these works, and although the quantity of refuse burnt is exceedingly small compared with the large works we have been considering, the results obtained are valuable. The generating plant is of the usual type, vertical high-speed engines with direct-current generators. The battery is a D.P. of 160 units capacity. The plant is run non-condensing. The destructor is a two-cell Meldrum Simplex, with a Lancashire boiler which is unusually short. An economiser is in use, and also a regenerator in conjunction with steam blast. I have plotted a curve (Fig. 25) for the year 1902 from figures given me by Mr. Preece. The coal consumption per unit generated as ascertained by Mr. Long when the destructor has not been at work was 9 lbs. in the winter and 10 lbs. in the summer. The average for the year works out at 9·5 lbs. The total coal used during the year was 348 tons, a large proportion of which was used in getting up steam to assist the destructor for a short time every day. The resulting 15 units per ton of refuse is poor, but it is clear that

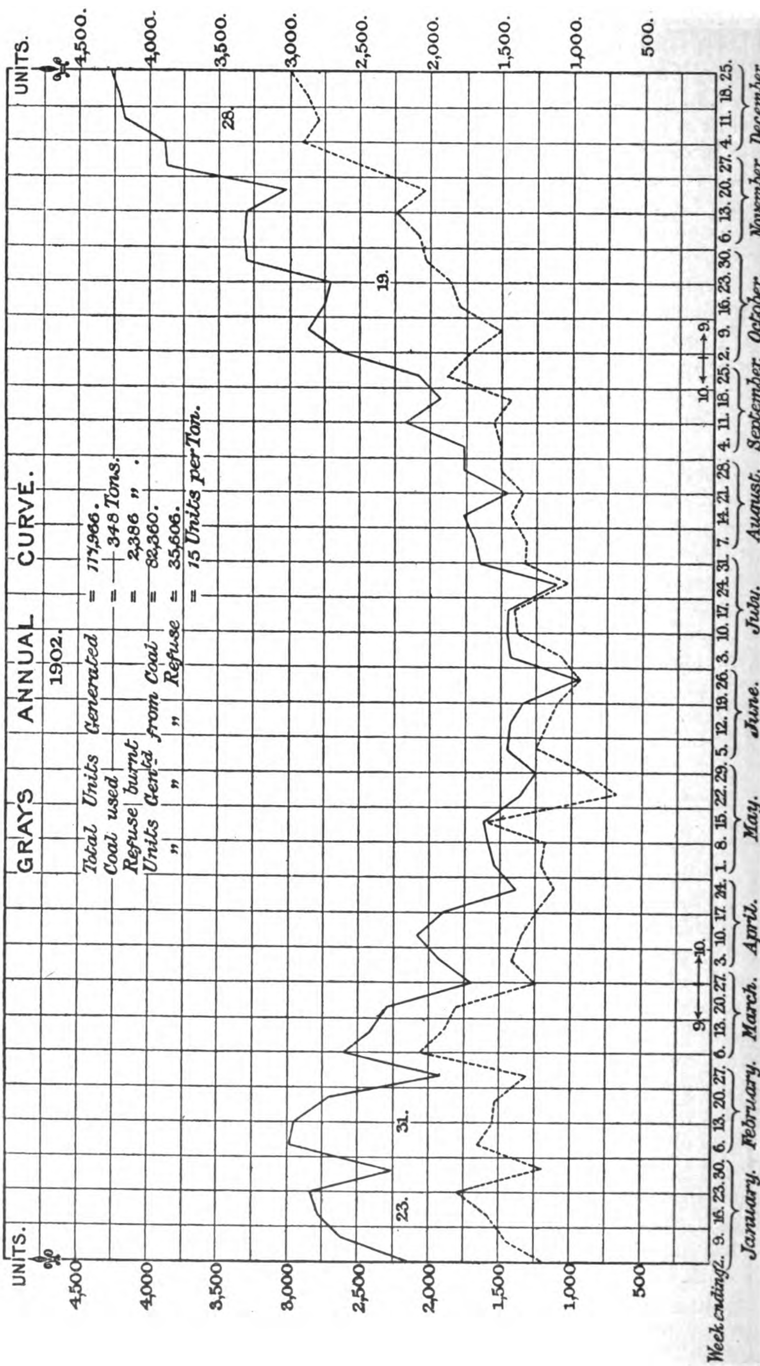

GRAYS ANNUAL CURVE.
1902.
Total Units Generated = 117,966.
Coal used = 348 Tons.
Refuse burnt = 2,386 " .
Units Gen'td from Coal = 82,360.
" " Refuse = 35,606.
= 15 Units per Ton.
UNITS.
4,500. 4,000. 3,500. 3,000. 2,500. 2,000. 1,500. 1,000. 500.
Week ending 2. 9. 16. 23. 30. 6. 13. 20. 27. 6. 13. 20. 27. 3. 10. 17. 24. 1. 8. 15. 22. 29. 5. 12. 19. 26. 3. 10. 17. 24. 31. 7. 14. 21. 28. 4. 11. 18. 25. 2. 9. 16. 23. 30. 6. 13. 20. 27. 4. 11. 18. 25.
January. February. March. April. May. June. July. August. September. October. November. December.
23. 31. 28. 19.
9 10. 10. 9.

FIG. 25.

ORKS.

	11	12	13	14	15
.te value of steam on of present coal cost.		Saving effected on Labour and Manageme		Total annual value of steam and saving by Combination. Cols. 7, 8, 12 and 13.	Cols. 7, 10, 12 and 13.
ıl.	Per ton of refuse burnt.	Labour electricity Works.	Management both Works.	Present.	Future, when full value of refuse is obtained.
	Pence.	£	£	£	£
:0	40	200	350	3,360	4,930
)0	32	200	None	2,115	5,580
)0	33·4	150	500	2,467	5,800
)0	25·3	...	...	2,362	4,090
	...	...	...	...	...
44	16·4	...	...	...	...
30	26·5	...	...	...	...
12	14·5	...	...	...	...
.	...	...	...	...	...
70	42·3	...	...	...	...
55	44	...	...	...	...
53	24·3	...	...	...	...
.	...	...	...	...	...
25	30	...	...	...	...
33	6·6	...	...	...	...
67	10·6	None	...	215	357

as the load increases a better value will be obtained. Since plotting the curve, returns for the year ending March, 1903 have reached me, and a substantial improvement is already manifest. Allowing for the reduced consumption of 9 lbs. of coal per unit, the average units per ton of refuse is increased to 21. The ultimate value with the plant working under more favourable conditions should reach 30 units per ton. I was for a long time puzzled at the "regular irregularity" of the curve, but the unusual character is explained by the fact that the street lighting is economically dispensed with at the time of full moon. In a small plant of this description burning between 2,000 and 3,000 tons per annum, the economical utilisation of the refuse becomes quite a special study, and the burning of coke breeze or coal in the destructor furnace to assist in the event of a shortage of refuse would probably prove more economical than getting up steam in an independent boiler. It is not possible to mix good fuel with the refuse, but with a little trouble a portion of the grate can be separately utilised. It is, I understand, now Mr. Long's practice to burn coke breeze in the furnace in this manner.

Gloucester.—I have been successful in securing an interesting curve from Gloucester. The type of destructor in use in this town is that constructed by Messrs. Heenan & Froude. The plant is a comparatively small one, consisting of two cells only, with one Babcock boiler. An economiser is used, and also a regenerator in conjunction with the fan blast, the fan being driven by electric motors. The engines are vertical high-speed condensing, the generators direct-current, and an E.P.S. battery of 440 units capacity is in use. In recording the returns for the year 1902-3, I have had difficulty in apportioning the units generated between coal and refuse ; Mr. Bache, the engineer, informs me that the usual rate of burning is one ton per hour, but on the peak the rate is increased to from 25 to 28 cwt. He states that 100 to 120 k.w. was maintained for one hour. From 25 to 40 units per ton are obtained in summer and from 60 to 80 in winter. When all the refuse is burnt usefully, it is expected that an average of 86 units per ton will be obtained ; at present the day load is small. In dividing the units generated for the year between the coal and refuse, I have had to assume that 15 lbs. of coal were burnt per unit generated. The refuse-generated units on this basis work out at one-third of the estimated total possible. Messrs. Heenan & Froude inform me that at present one-third of the refuse is burnt usefully. This figure of 15 lbs. per unit is high, but from the curves and from other information received, it is evident that a good value is being obtained from the refuse. In the curve (Fig. 26) for Thursday, Feb. 18, 1904, the coal used on the peak was 17 cwt. 2 qrs. The coal-fired boiler was in use for 4½ hours; one hour of this time was before the peak load came on, while the destructor was shut down for ashpit cleaning. It is, therefore, fair to assume that seven-ninths of the coal consumption took place on the peak. Averaging this, and assuming 9 lbs. of coal per unit generated, a value of 71 units per ton is obtained. As 20 tons only were burnt during the day, the high average value of 61 units per

ton was secured. These results must be taken as approximate only, in view of the uncertain coal value at these works.

Christchurch, N.Z.—One other plant I must mention briefly, as it has the distinction of being the only combined works outside the United Kingdom. The installation at Christchurch, New Zealand, has just completed its first year of working, and when the returns are published shortly, excellent results are promised. The electrical equipment is the usual high-speed direct-current type, but the battery is large, being half the size of that at Hackney, namely 600 units. As an advocate of large batteries in combined works, I look forward with special interest to the publication of particulars, for the total capacity of the generators installed is only 200 k.w. The destructor is of the Beamen & Deas type, consisting of two pairs of cells and two Babcock boilers,

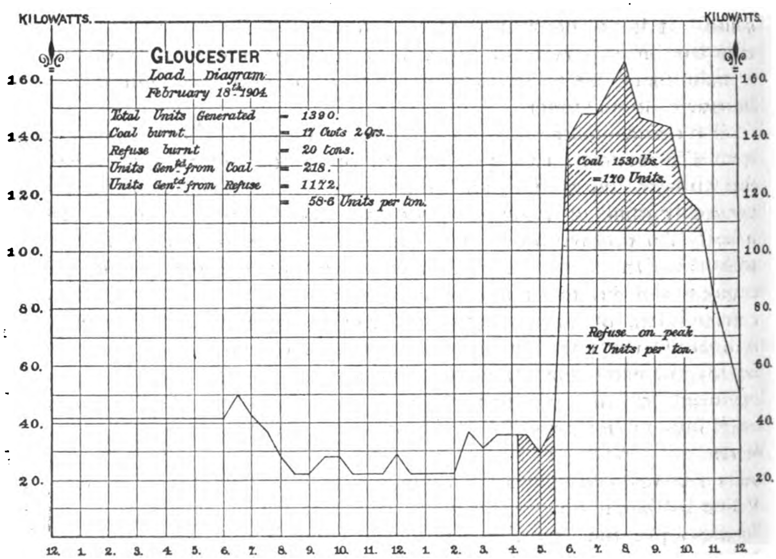


FIG. 26.

erected by Messrs. Meldrum Bros. in 1902. Twenty-five tons of refuse is destroyed between the hours of 8 a.m. and 10 p.m., giving ample steam for supplying the load during these hours and for charging the battery; the generators are shut down the rest of the day. The average evaporation is given at 2,000 lbs. of steam per ton of refuse.

Enough has already been said to show that under satisfactory conditions excellent results may be obtained from the combination of refuse destructors with electricity works. The installations at St. Helens and Shipley have been described by Mr. Highfield and Mr. Schofield respectively, and between 30 and 40 units per ton are obtained on the average. In a recent letter, Mr. Schofield informed me that he anticipates that some 50 units per ton will eventually be

realised. At Partick, 29·6 units per ton were realised during last year, and it is anticipated that 37 units will ultimately be obtained. Although I have not obtained much data from Garston, the figures supplied show that some 50 to 60 units per ton are frequently obtained, and particulars of a test lasting 18 hours with the generating plant run non-condensing show an average of 62 units per ton, the water consumption being 50 lbs. per unit generated.

In considering this question, the tendency is naturally to notice the more favourable results, but there is no doubt that in some stations the electrical returns are inadequate, and it is probable that the works would have been better kept separate—particularly the electricity works. I have recorded such reliable information as I have been able to gather in the various tables accompanying this paper.

In endeavouring to secure good steam-raising results, it is desirable to maintain the temperature of the flues as uniformly as possible, and any device therefore which will render the shutting down of the plant for cleaning purposes unnecessary wiil be valuable. Particularly is this so in small plants, and the apparatus devised by Mr. Sillery, of Wrexham, is a step in the right direction. By means of this apparatus the dust destructor at Wrexham has been kept running for a whole year without a break. The maintenance of the temperature in the flues is important, not only from a steam-raising point of view, but also is desirable as tending to reduce the expenditure on repairs. The constant expansion and contraction of the brickwork owing to alternate heating and cooling is destructive.

As points deserving of careful attention, I have already referred to air leakages and the method of producing the blast. Both of these may be sources of large percentages of waste. Another point to which I have referred and which it may be well to emphasise, is the possible loss of efficiency due to long and badly arranged flues. Mr. Price F. White, of Bangor, has been most kind in providing me with information concerning his plant, and in explanation of the poor results obtained calls my attention to the construction of the main flue. It is important to note that the destructor was added after the electricity works had been put down, and the main flue from the destructor to the boiler-house is long and built at a low level. The result is that the flue is very damp, and the greater part of the evaporation due to the destructor gases takes place *outside* the boiler. At times, Mr. White informs me, the flue is so full of water that the destructor has to be shut down.

A reference to Table VI., giving costs of disposal, will show the large proportion of total costs often caused in disposing of the clinker. This matter is one requiring serious consideration, as if a satisfactory sale for this destructor bye-product can be secured, a heavy annual cost will be turned into a substantial profit. As an illustration of this, I may point to the fact that at Fulham the cost of disposal of clinker amounted to £479. Mr. Fuller is making special efforts to dispose of his clinker profitably, and this year the loss will have disappeared and a profit have been secured instead. At Hackney, Mr. Robinson is not quite so successful, but he is now disposing of the clinker without cost.

This will result in a saving of £750, no mean proportion of the total costs of destruction last year.

The question of combined electricity works and dust destructors promises to assume very large proportions, and it is desirable to realise that it is only some five years since the pioneer works at Shoreditch were erected. Every town of over 10,000 inhabitants will probably in the course of a few years be provided with destructors, and if the heat value is sufficient to pay a large part of, and perhaps in some cases the whole, cost of destroying the refuse, the economy of combined works is a matter of high importance to ratepayers throughout the country. It is probable that many of the large existing destructors might be economically adapted for the generation of steam. Several instances have come under my notice, and I have recently been required to report in such a case. The conclusions come to are not a little interesting, and the economy of the proposed alteration for generating electricity is very considerable. It may not always happen that existing destructors are situated satisfactorily from an electrical point of view, but the transmission of the electricity generated to the electricity works or to a substation is not a problem presenting any serious difficulty.

The surprising results obtained in the London undertakings which I have had the advantage of introducing to your notice this evening, may well give rise to some curiosity as to the values which will be ultimately secured in large combined works. After a study of the Fulham and Hackney curves, an average in the neighbourhood of 100 units per ton would appear to be within reach, and the heat value is certainly contained in the refuse, if only it can be fully utilised.

So long ago as 1892, Professor George Forbes stated in a lecture before the Society of Arts, that if the refuse then collected in Paddington were properly burnt and used in the most economical way, it should provide enough electricity to light one 8 c.p. lamp for two hours every night of the year per head of the population, which is the estimated amount of light demanded by the exigencies of modern civilisation. It is a great pleasure to me to take upon myself the vindication of Professor Forbes as a prophet, for by the exercise of a small amount of scientific licence, I am able to demonstrate that in at least two Metropolitan boroughs this result will certainly be achieved within a few years. A 30-watt carbon lamp burning two hours per night throughout the year would absorb 22 units. A Nernst lamp giving the same light would burn, say, 11 units. The population of Fulham is 150,000, thus 1,650,000 units would be required; the ultimate value is estimated at 1,870,000 units. The population of Hackney is 230,000, the units required would amount to 2,530,000 units; the ultimate value of the Hackney refuse as estimated by Mr. Robinson is 2,436,000.

I will ask you to bear with me while I make a modest little calculation on my own responsibility. The annual collection of refuse in London amounts to approximately 1¼ million tons; taking so low an average as 40 units per ton as its potential capacity for generating electricity, no less a sum than £200,000 per annum—

taking the average coal cost of all the London undertakings—is being tipped on dust heaps, resolved into vapour and deliberately barged out to sea at heavy cost.

Conclusions.—As I am addressing engineers, I need summarise my conclusions but briefly.

1. The remarkable figures obtained in some undertakings as demonstrated in the series of curves I have had the privilege of laying before you, are evidence that great advances have been made in the problem of utilising waste heat from burning refuse in destructors since the pioneer works were started.

2. That these results still leave room for substantial improvement—(*a*) By closer attention to the details of construction of furnaces and flues, and the general arrangement of the plant; (*b*) by improving the methods of handling and burning the refuse; (*c*) by careful utilisation of the steam generated, and the adoption of an economical form of blast.

3. That given suitable conditions, there is without doubt a substantial gain to be effected by the combination of electricity and destructor works; but owing to the variation in the calorific value of refuse in different localities, and to other special local considerations, every proposed case of combination needs to be considered on its merits by persons qualified to make an independent and intelligent inquiry.

4. That the various and in some cases crude methods in vogue of adjusting accounts between the two works need to be systematised, and that it is highly desirable that the results obtained be separated and recorded with accuracy, so as to give, so far as possible, a true record of the financial position of both works.

I have much pleasure in recording my indebtedness to the various Engineers and Borough Engineers in provincial towns with combined works for their readiness in complying with my somewhat exacting requests for information. My thanks are in a special manner due to Mr. Russell, of Shoreditch, Mr. Tapper and Mr. Jameson, of Stepney, Mr. Fuller, of Fulham, Mr. Robinson, of Hackney, and Mr. Vincent, of Bermondsey, for their very great kindness in affording me every assistance in the collection of information from their records, without which the preparation of this paper would have been impossible, and also to my assistant, Mr. L. S. Fosbrooke, for his excellent work on the diagrams.

Mr. J. S. HIGHFIELD: Mr. President and gentlemen, I did not expect to have the honour to open the discussion on this important paper, but perhaps I can add a little to the general information contained in it by giving you some figures obtained by the destructor plant at St. Helens, Lancashire, which I had to work for some years. I might say at the outset that when I began to work that plant I had great hopes that the value of refuse as a steam-producing agency would be considerable, and I worked it with the idea of using it to the utmost. I have some recent figures from the engineer, Mr. Hollingworth, which are rather more up to date than my old ones; Mr. Highfield.

Mr. Highfield. and the present results, which they are getting after five or six years' experience, amount to practically thirty-six units per ton of refuse taken over the whole year. The electrical plant consists of direct-current generators driven by economical condensing engines, operating a combined lighting and traction load. There is, in addition, a great amount of motor driving in the place, as it is a manufacturing town, so that the load-factor is unusually high. The destructor is used to supply as nearly as possible a steady amount of steam throughout the twenty-four hours, all the peaks being taken by the coal-fired boilers. You will note that, from the curves attached to Mr. Adams' paper, this method of working has become general; thus the refuse is given every chance of turning out the maximum possible units, but it follows that the money value of each unit generated for this continuous part of the load is not nearly so great as the money value of the fuel used per unit over the peak part of the load. The system usually adopted of averaging the cost of fuel per unit and crediting the destructor with this average price gives an undue preference to the destructor plant. In an entirely coal-fired station, although the average weight of coal per unit works out at probably 4½ lbs., the weight of the coal used for the solid part of the load does not usually exceed 2 or 3 lbs. per unit with a high-class plant. The apparent money value of the destructor is therefore not quite so great as would at first sight appear. A further drawback is that usually the destructor is shut down over the week-end; this is a source of much trouble, as the coal-fired boilers have to be started up specially to handle this short piece of the load, resulting in a considerable waste of fuel. You will have noticed in nearly all these diagrams, especially the Fulham curve, the refuse starts rather badly, and as the day gets later the number of units produced per ton of refuse becomes more and more. I think the reason of this is due to the varying quality of the refuse. In the morning, great loads of refuse of all sorts come in. Usually the last lot to come in consists of market refuse, such as cabbages, fish, and so forth, which has a powerful odour. The first business of the men when they come in the morning is, naturally, to get rid of all this rather savoury material as quickly as possible, and it has not very great heat value; the fish and cabbages, and so on, do not make many units per ton, as the diagrams show. Later in the day the choicer varieties of fuel are burnt, and then more units per ton are made, as you would expect; in fact, some of the refuse that used to come up to us would be much better put inside the boiler than inside the furnace. I do not know whether one would agree altogether with the figures on which Mr. Adams based his conclusion that the whole of the electricity required in London could be made from refuse. In Montreal, 95,000,000 units are used per annum by 250,000 people. That is the figure we hope to get to, but I do not think such a number can ever be turned out by refuse. There is one almost hopeless drawback to the combination of destructor and electricity works. Those used to destructors think it is fanciful, but the engineers at the power-station know it to be a very rigid fact, that the amount of dust, smell, and so forth, produced from the destructor causes very great trouble in the engine-room. A destructor is not at all a nice neighbour

Mr. Highfield.

for an electricity works; and, as I said a long time ago when I read a paper in Glasgow about the same thing, the only way to make it at all tolerable is to build a very high wall between the destructor works and the electricity works in order to prevent dust and foul material of one sort and another leaking from the destructor into the electricity works. It is far preferable to put the destructor works as far as possible away from the electric works, and let the destructor work mortar-mills and similar rough engines. I think probably, if it is decided to use a destructor for supplying electric energy, the best way is to put up a small electric plant as cheaply as possible at the destructor works, and arrange the tramway feeding system (for I do not think the attempt at all worth making on a lighting load) so that the destructor can supply feeding units into the tramway system just as it finds itself able to do. If the main steam plant is large in comparison with the destructor plant, there is no difficulty about this. In this way the destructor can be used to the full extent, and if its steam supply should for the moment fail, there is no difficulty in the large plant taking up the small extra load.

Mr. Newton Russell.

Mr. C. NEWTON RUSSELL: I must offer my congratulations to Mr. Adams for the most interesting paper he has given us. He has my sympathy. The collection of such a mass of information as he has presented to us had taken up, I have no doubt, a great deal of time and trouble. I have very little to add to my previous statements on destructors in general, but there are one or two points on which I would like to speak. On page 258 Mr. Adams refers to the Shoreditch destructor, and says, "Dampers are provided between the destructor and boiler furnaces to cut off the former when not in use." Certainly those dampers were originally put in between the boiler furnace and the destructor furnace, but the heat proved so great that the cast-iron frames and dampers, composed of ⅜-inch thick plate, were burnt out in a few weeks; the construction of the cells would not allow us to replace them, so we have been working the furnaces under very disadvantageous circumstances. In column 21, Table IV., Mr. Adams gives the value of refuse in units per ton. I would like to say that, in my opinion, he has taken the wrong basis for the comparison. Although in another column he has given a few figures showing how many pounds of steam are required to generate a unit of electricity for different works, the whole of the figures are not there; but when you remember that the value of the refuse does not altogether depend upon its calorific value, but has to be coupled with the efficiency of the electrical generating plant which has to be worked with it, you will find a different complexion altogether can be put upon that column. For instance, taking Fulham as an example, I understand it requires 50 to 60 lbs. of steam to generate one unit of electricity. At Hackney, which is one of the most modern stations—you might take it as a model—they only require 27 to 30 lbs., or something of that kind, about one-half the steam that is required at Fulham to generate one unit. If you follow the figures through for other stations you will find that Stepney is the same, and Shoreditch gives about 50 lbs. of steam per unit, so that the number of units that can be generated per ton of

Mr. Newton Russell.

refuse burnt would in some cases be doubled. I would suggest that Mr. Adams should add another column showing the value of the refuse in lbs. of steam instead of units generated per ton. Then there is the question of the value of the land upon which destructors are built. Sufficient consideration, in my opinion, has not been given to this point, especially so far as London is concerned. The revenue-earning capacity of land used for destructor purposes as compared to the revenue-earning capacity of electrical lighting stations is only about one-tenth. In other words, given two pieces of land of the same area, and putting an electricity and generating station on one and a destructor on another, you will be able to earn £50,000 a year gross revenue with one and about £6,000, perhaps, on the other. So that when you come to think that in Shoreditch—and I suppose it would be the same in Stepney—the interest and the redemption of capital per ton of refuse destroyed works out at about a shilling per ton, it seems to me a mistake to put destructors in the middle of large towns. I am with Mr. Highfield in that respect; I think they ought to be outside the town, where the land is cheap. Mr. Adams also referred to the question of accounts. We have been very unfortunate at Shoreditch in the arrangement of the accounts, owing to the complicated disposal of the steam. The steam produced from the refuse boilers is mixed with the steam from the coal-fired boilers, and there are four purposes for which the steam is used: for the baths (the live steam), for pumping, and for boiling clothes in washhouses, and also for heating the free library. We have no possibility of finding out the exact amount of steam that is used for the boiling department; we have practically to make a guess at it, so that it is very difficult to know exactly the amount of steam that is used. We do know, however, that we receive much too small a sum to cover the expense. It may be of interest to members of the Institution to know that some time ago the Shoreditch Council gave me permission to conduct some experiments in connection with burning refuse with a high degree of blast, because from my experience there is no doubt whatever that the point at which the refuse is turned out from furnaces at present is too early; in other words, the clinker is not unburnable, there is still a very great amount of carbon in it, and if only the air pressure was increased, the residue can be reduced considerably. We put up a smelting furnace, practically the same sort of thing as iron is smelted in, with a pressure of 3 inches and upwards; and, starting the fire with a small amount of coke to get a start, we were able to run for many hours, smelting the refuse down by its own calorific value or with the carbon that was in it; the refuse melted itself and ran out just the same as slag out of an iron furnace. It was possible to run it into moulds, but it was so brittle that it was no good. The same experiment was tried in Berlin by Mr. Carl Wegner, but there the calorific value of the refuse was so small that he had to use a very large amount of coal with it, and the experiments were discontinued. But I firmly believe that if a very large smelting furnace were put up, something after the same style as they are in the north of England for smelting iron, the refuse could be smelted down. (I hoped to have had some samples here to-night, but I was unable to bring

them.) In that way the residue can be reduced down to 15 or 17 per cent. It looks like black glass when it is run out, and I have seen some of it after it has been run out made into glass paper, and used for other purposes. I do not think I have anything more to say, excepting that any information I can give with reference to our latest figures, as our accounts are on a new basis, the same as Hackney (which I think is a model arrangement, and which I would like to see adopted in all destructors, so that we could all compare accounts on the same basis)—any figures I am able to give I will forward to Mr. Adams, to be included in the paper. **Mr. Newton Russell.**

Mr. L. L. ROBINSON: Mr. Adams is an advocate of very large batteries. For the benefit of the destructor the battery must be large, but as the electricity undertaking has to bear storage, maintenance, and capital charges, complete usage of the destructor heat should be obtained by adopting low prices for day load electricity supply rather than by installing an enormous battery. With reference to our battery curve of December 19, 1902, I desire to point out that we like to keep a little in hand in our battery; we think a great deal more of the continuity of our electricity supply than of squeezing a few extra units per ton out of the refuse. **Mr. Robinson.**

I entirely concur with Mr. Adams that the accounts of each undertaking should be dealt with as a separate department, with its own capital, annual working costs, and income carefully set out. As a matter of fact, the tables prepared by Mr. Adams have little value as a comparison of cost of working of the various destructors, because no uniform system has been adopted for keeping the accounts of combined stations. Every one must deeply regret the clumsy way in which the accounts of many destructors have been kept, and the misleading statements which have obtained currency in consequence. At Hackney we have taken a firm step towards a strict and uniform system of keeping accounts. One stores is worked in connection with both undertakings, but most careful accounts are kept of all material issued to either department. The men's time is carefully analysed on time sheets, and every motor and lighting circuit in the destructor is supplied with electrical energy through an independent meter. The units generated are metered at the terminals of each dynamo, and all the coal used is very carefully weighed. As a check upon coal values, the coal required per unit generated is carefully measured every Sunday and records kept. These tests show an average of 3·6 lbs. of Aitken Navigation Rough Small coal per unit generated at 13s. 8d. per ton, having calorific value of 13,300 British thermal units per lb., and 4·3 lbs. of Broomhill Rough Small coal per unit generated at 10s. per ton, calorific value 11,600 British thermal units per lb. Broomhill and Aitken Navigation coals mixed in equal proportions were used until a few months ago, but now Broomhill Rough Small alone is used. An arbitrary coal value of 5 lbs. of coal per unit generated has been adopted for the purpose of adjusting accounts, but undoubtedly this figure is too generous to the destructor. The quantity of coal per unit generated is lower in the case of Hackney than in the case of any other works dealt with in the paper. This is due—(1) To the use of triple expansion

Mr. Robinson.

engines of 300 to 1,500 k.w. capacity; (2) Condensing; (3) Superheating; (4) Good mechanical stoking; and (5) All auxiliaries, except three feed-pumps, electrically driven. In connection with the accounts, the payment made by the Sanitary authority is important. I fail to see how any other system can be more equitable than that which provides for the payment to the destructor department of the balance of expenditure, after deducting the costs allowed by the electricity department for its steam and the sundry receipts. Any process of account keeping which could possibly reduce the apparent costs of refuse destruction at the expense of the electricity undertaking is to be deplored. The results would be an apparent increase of the electricity undertaking's costs, and consequently a real increase in the price charged. This would tend to hinder the progress and prosperity of a really commercial undertaking. I feel sure that the Shoreditch undertaking must have suffered in this direction from its association with the destructor (I mean, of course, its accountancy association), but I know that Mr. Newton Russell is doing his utmost to have his undertaking treated on the Hackney basis.

As to saving of labour. Where mechanical stokers are employed, the total number of men in the boiler-house is so small that no labour is saved by combination with the destructor. At Hackney two men are employed on the heavy shift, and one on each of the light shifts. It is absolutely necessary to employ these men so that the coal-fired boilers may be ready to assist the destructor at any moment. The peak of the electricity supply load can easily be met by the same number of men, whether the destructor is at work or not. The destructor foreman's services cannot be utilised in the boiler-house, as it is found that his time is fully occupied supervising the work of the stokers in order to secure efficient working of the destructor cells.

I think the author is too sanguine in regard to profitable clinker disposal. As the output of clinker increases and the destructors become further removed from building land, owing to the extensions of the town's boundaries, the clinker will become even more a drug on the market than it is to-day. The reason that clinker is so costly to deal with is that already the supply greatly exceeds the demand, and in London the cost of removing the clinker to localities where it can be utilised is so great. In my opinion no destructor is complete without a plant to work up the clinker into saleable articles; but here the municipality is at a great disadvantage, owing to the legal difficulties which would be encountered before one could create a wide market in which to sell.

On the general question of credits to the destructor I entirely dissent from the suggestion of the author that profit should be made by a local authority for permitting some of its ratepayers to pick over the refuse as it is turned out of the carts. The first essential in refuse destruction is that the refuse shall be disposed of in a sanitary manner. At Hackney we go so far as to burn our tins and dispose of them at 5s. per ton, with the full knowledge of the fact that we could dispose of unburnt tins at 16s. per ton, the reason being that we do not consider it right that any filthy articles which have once been in the ashbins should be

allowed to be distributed broadcast throughout the borough until they have been properly dealt with in a sanitary manner. Mr. Robinson.

Dust troubles are by no means a negligible quantity. No precautions seem sufficient to keep the flue dust out of the hot-wells and cooling towers. At Hackney we have had endless trouble with our feed-pump valves and condenser tubes owing to this nuisance.

Mr. Adams praises the Tables of Costs of the *Electrical Times*, but it is a pity that for his comparisons he does not adopt those methods in their entirety. He includes loan charges with costs per ton destroyed, and by that means Hackney's first year gets unfair treatment, for the reason that the loan charges cover a much longer period than the working period. Therefore the total costs are bound to compare unfavourably with those of Fulham. But take last year's records in both cases, and the Hackney costs will, I venture to say, be found to be lower than those of Fulham. Possibly Mr. Hawes, if he takes part in the discussion, will give us the benefit of the latest returns from the Metropolitan undertakings.

DISCUSSION AT MEETING OF JANUARY 12, 1905.

Mr. H. N. LEASK: I have listened with great interest to Mr. Adams' paper, especially as it is one given by an independent party; *i.e.*, one who is neither a maker of refuse destructors nor an engineer in charge of an electrical generating station. At the same time, I think there is an element of danger to the value of the paper in that in some cases the value of the refuse measured in Board of Trade units is arrived at by calculation. Mr. Leask.

On learning that a paper was to be read on combined stations I had rather expected and hoped that it would deal largely with the planning and designing of such, which I think is a question deserving of most serious attention.

If it were well considered, I think it would be easy to remove a great many of the objections that some electrical engineers have to refuse destructors, and turn these latter to much better account, especially with regard to their dividend-earning capacity.

When speaking of the planning and designing, I am not alone referring to the arrangement of the plant and the buildings, but more especially to the suitable combination of the boilers, flues, engines, etc., and provision of storage capacity for both heat and electrical energy.

Another point which is I think very often lost sight of is the benefit arising from the employment of forced draught. A forced draught system of some kind or another is employed by all destructor makers of note, and in some cases at least they can claim to obtain more complete combustion than is obtained in the majority of coal-fired furnaces.

There are not many electrical generating stations up and down the country that employ an artificial draught, and I only know of one combined station that does so, other than that mentioned in the paper.

I am of opinion that all combined stations should be fitted with a properly designed system of artificial draught, preferably forced

Mr. Leask. draught, and then the economy of the refuse destructor would become apparent, and there would also be economy in the operating of the coal-fired boilers. This would also result in a reduction of the smoke nuisance from coal-fired boilers, and would lead to an easy solution of the chimney question.

Some time ago Messrs. Heenan & Froude, Ltd., erected a destructor for the Corporation of Mansfield, which is now under the control of the electrical department, and I am informed by the electrical engineer to the Corporation, Mr. E. Holcombe Hewlett, that they have been running the whole of the station for the last three months on refuse alone. Their load is not large; at the present time they generate about 1,250 units per day.

Owing to the fact that there is only a single steam main from the destructor boilers to the steam main in the engine room, they are compelled to keep a banked fire in one of the coal-fired boilers. The steam pressure in this boiler is maintained about 20 or 30 lbs. below the pressure at which steam is used in the engines, and not more than 10 cwt. of coal per day is used for banking the fires. Including the lighting and warming of the furnaces, warming up of the steam mains, engines, auxiliary plant, and taking the refuse burnt from the moment they light the fires to the end of the run, the destructor produces over 50 units per ton of refuse destroyed. I think this is a very fair result, considering that the destructor is not being worked at all during thirteen hours of the day, and that steam for the purposes of generating electrical energy is only taken off for about seven or eight hours each day. It is also to be remembered that these 50 units are all that the station demands from a ton of refuse, and is not all that the refuse could give, steam blowing off very frequently at 200 lbs. pressure during the working.

This plant, when tested, gave at the time—month of May—an average of 88 units per ton of refuse when the engines were running condensing, and 72 units when the engines were running non-condensing. I have been informed that when they have their best loads at this station, over 120 units per ton of refuse have been obtained, and I am further informed that still higher results have been achieved, but I have no actual figures from the station to that effect. The evaporation from and at 212° F. at this station under test was 1·8 lbs. Under ordinary working conditions about 50 lbs. of steam are absorbed by the station in generating one unit at the switchboard.

At Gloucester they have a load which rises in half an hour from 20 or 30 k.w. to about 250 k.w. This load lasts for about one and a half hours and then gradually falls off. This has been successfully carried by a "Heenan" Twin Cell destructor, burning refuse at the rate of 1½ to 2 tons per hour. At this station under test the average number of units generated per ton of refuse destroyed was 90·3 during the first six hours of the test, and during the second half of the test the number of units was 79; the water evaporated from and at 212° F. during the first portion of the test was 1·47 lbs., and during the second portion of the test was 2·02 lbs. of water per lb. of refuse.

At Northampton there is a combined station, and the coal-fired

boilers are fitted with an induced draught plant, and there is a traction load. I am informed that when they are not running the destructor the amount of coal used per unit generated is 5·5 lbs.; but when the destructor is in operation they only use 2·5 lbs. of coal per unit generated, notwithstanding the fact that in this case the refuse is of particularly low calorific value. Mr. Leask.

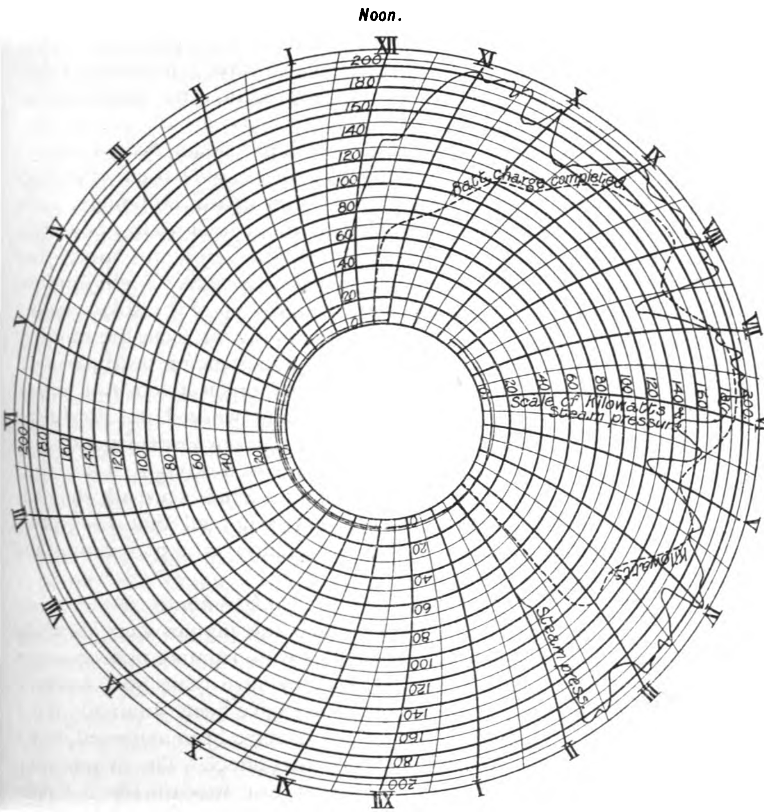


FIG. A.—Load and Steam Pressure Diagram, November 26, 1904 (Mansfield Corporation Electricity Works).

Fires started up midday.	Water evaporated, 6,520 gallons.
Refuse burnt, 25 tons, approximate.	Water per hour, 724 gallons.
Units generated, 1,280.	Units per ton refuse, 51·2.

Mr. W. R. COOPER: In the preliminary part of his paper the author has given the advantages rather than the disadvantages of destructors. One difficulty in combined works arises from the fact that the destructor has a full load as soon as it is built, but the electricity works starts with a very small load. The result is that, initially, perhaps the whole of the steam may be supplied by the destructor. As the electricity works becomes larger this stage passes into another, during which coal-firing has to be resorted to for parts of the load. Finally a stage Mr. Cooper.

Mr. Cooper.

is reached in which separate boilers have to be put down to meet the load. All works are liable to pass through these three stages, and eventually to find themselves with two sets of boilers. As the population does not increase as rapidly as the lighting load, the proportion of steam supplied by the destructor becomes less, so that one has to consider how far it is worth while to complicate the system, and very possibly to use the coal to less advantage. The complication is apt to hamper extensions, and although there may be a gain in the first instance in the amount of ground required, the proportionate gain diminishes as time goes on. Moreover, the requirements of a station, as regards site, are generally incompatible with those of a destructor. A destructor should be placed well away from the best parts of a town, but if an electricity works is so placed there is likely to be a material loss in distribution. A chimney shaft may be saved to some extent by the combination, but it cannot be said that a whole chimney is so saved, because a destructor chimney is very large in proportion to the power which it represents.

On referring to the various load curves that are given, it will be noticed that these are generally complicated by the two quantities—coal and refuse; and in order to determine the effect of one of these quantities, the effect of the other must be assumed. There are, however, two curves, viz., Figs. 20 and 24, which deal with refuse only, and these certainly give valuable information, though they must be looked upon as somewhat exceptional. Fig. 20 refers to Hackney, and gives the high figure of 104·3 units per ton; this is accounted for by the economy of the station, and it must also be borne in mind that stand-by losses of refuse do not exist, because the refuse is burnt whether required or not. Fig. 24 also gives a high figure, namely, 100 units per ton for part of the time; but in this case the calorific value of the refuse is unusually high.

In the other curves it appears that the value of the refuse is deduced by taking an average value for the coal, and arriving at the value for the refuse by difference. For example, in Fig 9, which refers to Fulham, an average of 8 lbs. of coal per unit is taken as a basis. How this is obtained the author does not state. The fact that a certain amount of coal is burnt in a particular time along with refuse gives, of course, no information as to the number of pounds required per unit. But assuming that 8 lbs. is correct as an average, there is no reason why this value should hold for all parts of the curve. Generation is more economical at high loads than at low loads. In other words, the figure for coal will be less at the peak and more at the low loads, and if a lower figure is taken for the peak, more units will be generated by the coal and the number of units per ton of refuse will be less. This reason also explains the apparent increased value of refuse as the load increases.

Apart from this, it must be remembered that the amount of fuel per hour is not directly proportional to the power. If, for example, a given load requires fuel to be burnt at a certain rate, the raising of this load, say, one tenth will not require so much as one tenth increase in the rate of burning fuel. In other words, it is the lower part of a load

curve which takes most fuel, and the upper part which takes least. Hence, the figure for units due to coal should be increased, which means that the figure for refuse should be lower. Mr. Cooper.

But, in any case, a comparison should not be drawn between the effect of refuse and coal in a combined plant, because the coal is often burnt in such a station under disadvantageous circumstances. What is really important is the best that can be done with refuse as compared with coal under the best conditions—that is, in a separate, well designed generating station. It is quite possible that coal may be burnt to such disadvantage in a combined works, that the saving due to steam from refuse does not exceed the loss from inefficient coal-firing together with increased capital charges, maintenance, and depreciation.

If one considers the steam-raising qualities of coal and refuse, it will be seen that the author's conclusions are not very well supported. On page 269, the author himself is inclined to look with scepticism upon calculations which depend upon refuse giving from 1 to 2 lbs. of steam per pound of refuse. The author would probably regard 1·5 lbs. as a high figure. Ordinary figures range from 1 to 1·5 lbs. and we might perhaps take 1·25 lbs. per pound as an average figure. This is equivalent to 2,800 lbs. of steam per ton of refuse. If such a figure as 100 units per ton is to be obtained, this means that the average water consumption for the whole station (including auxiliaries) per unit generated must be as low as 28 lbs., which is probably unknown in ordinary lighting stations. Taking the case of Fulham, 1·15 lbs. of steam per pound of refuse is mentioned, which is equivalent to 2,580 lbs. per ton. According to Fig. 17, 94 units were generated per ton, which is at the rate of 27·4 lbs. of steam per unit generated. In fact, the steam consumption per unit would be really even lower than this, because some of the steam is used for other purposes. Yet in the same curve it is assumed that the coal required per unit generated is as much as 8 lbs. Assuming that one pound of coal will give only 8 lbs. of steam, which is a moderate figure, then the steam per unit generated from coal is 64 lbs. as compared with 27·4 lbs. of steam for the refuse. In the case of Hackney, the value of the refuse is put at one pound of steam per pound of refuse, and the author says that it ought to be possible to generate 120 units per ton, or in other words, at the rate of 18·7 lbs. of steam per unit generated, whereas the author mentions that 30 lbs. of steam are required in the case of coal. Even if the refuse gave as much as 1·5 lbs. of steam per pound, the consumption of steam per unit would be only 28 lbs. Mention is also made of 5 lbs. of coal per unit at Hackney. This means that only 6 lbs. of water are evaporated per pound of coal; which seems very low, and therefore I feel there is some doubt as to one or other of these figures. If the steam consumption is so good, why is the performance of coal so poor in an up-to-date station?

The author's figures are very flattering to refuse, apparently at the expense of coal. This may be put in another way. Suppose 8 lbs. of coal are required per unit generated, as the author states at Fulham, and that it should be possible to generate 100 units per ton; then only 22·4 lbs. of refuse are equivalent to 8 lbs. of coal, or the calorific value

Mr. Cooper.

of coal is only 2·8 times that of refuse as burnt in a destructor. Again, if 22·4 lbs. of refuse are really equal to 8 lbs. of coal evaporating 64 lbs. of water, the value of the refuse is nearly 2·9 lbs. of steam per pound, which is quite unknown. Such figures might be possible enough if the refuse were burnt quite irrespective of sanitary requirements, but unfortunately steam-raising must obviously be considered as only a secondary object.

I think it is a pity the author has taken for his basis *units* generated per ton. Since the efficiency from boiler to generator terminals varies very much from station to station, such a basis renders any comparison of the different stations quite impossible as far as the destructor is concerned, and there is no reason to hope that good results will be obtained at one station merely because they were obtained at another. The utilisation of the steam in the two cases may be quite different. The only true basis of comparison for destructors is the heat available; but as this basis is more or less an impossible one, the next best thing is to take as a basis the quantity of steam raised per ton of refuse burnt. As to what is done with the steam afterwards, whether it is wasted or whether it is used to the best advantage, is a question of design and of the steam plant used.

The criterion in any case is, of course, the financial result. Unfortunately it is always difficult to arrive at a sound conclusion from such tables as the author gives, because the destructor and the electricity works are owned and worked by the same body. The combined undertaking is worked to benefit the destructor, not the electricity works, and the financial position is largely a matter of book-keeping. The only valid way of dealing with the matter is to consider the undertaking as a whole, and to see whether the saving in fuel to the electricity works compensates the complication, with probably a lower efficiency and a certain additional outlay, with depreciation, etc. I do not mean to say that I disbelieve in using steam from destructors, but I think nothing is gained by putting things in too rosy a light.

Mr. Watson.

Mr. G. WATSON: I think destructor makers ought to be particularly grateful to the author for bringing this subject so prominently before the Institution of Electrical Engineers. I have no doubt whatever that in matters of detail the paper is open to criticism, and I should also like to add that it was no doubt written for the purpose of being criticised and fully discussed. But I think there is a danger in this Institution—which ought if possible to be avoided—of two opposing camps being formed, one of the camps consisting of the enthusiasts, and the other of the gentlemen whose business it appears to be to pour cold water on the whole attempt. Mr. Highfield at the last meeting spoke of the refuse as if it consisted exclusively of rotten cabbages and stinking fish. It is very fine to see Mr. Highfield's scorn for those materials, but it should be remembered that there is also a certain amount of cinders, with rubbish and other things which make town refuse into a fairly good low-class fuel, which I entirely agree with Mr. Cooper is capable of giving from 1 to 1½ lbs. of steam for every lb. of the material. Then the fuel is not one of those fuels which have to be purchased. The local authority has that material on its hands,

Mr. Watson.

and it has to get rid of it; and if it does not burn it, it has to be got rid of in a more expensive manner. Not only does the refuse not have to be bought, but it is worth a certain amount to a local authority, which is in many cases equal to 2s. a ton, to get rid of it in a sanitary and proper manner. Therefore, it appears to me clear, seeing that the local authority has to burn the stuff, and that you are able to get from it 1 to 1½ lbs. of steam per lb. of refuse, it is exceedingly advisable for the local authority to use the whole of the steam and burn the refuse in the most profitable manner. The question is apt to be looked at merely from the point of view of the electricity station. That is too narrow a view. The main question for the community as a whole is what ought to be done with the refuse. I think everybody agrees that the refuse ought to be burned. Everybody agrees that the higher the temperature at which it is burned the more satisfactory the process, because you get an entire absence of nuisance, and you get more steam, and a harder clinker which is profitable in itself. I do not quite agree with Mr. Cooper when he says that the destructor should be as far from the town as possible; I think, on the contrary, that the modern destructor is capable of being placed in a convenient position in the centre of the town, without danger of nuisance arising; and the object of reducing cartage is quite as important to the destructor, as the object of avoiding long lengths of electrical mains is to the electricity station. Therefore the same considerations which lead to putting your electricity station in the centre of a town also lead you to put your destructor there. The fact that a destructor does not necessarily grow at the same rate as the electricity station does not appear to me to be a serious bar; it rather indicates that the destructor should be self-contained, while being placed as near as possible to the work which it has to do. Another objection raised to the combination, which I think perhaps has more point in it, is the question of the dust. Some gentlemen seem to be able to persuade themselves that dust will go through a 14-inch brick wall. My experience is that if you put a wall up between the boiler-house, which contains the destructor, and the power-house, there is absolutely no danger of dust doing any harm to the machinery. I think if any one will visit the Fulham station they will be at once convinced that the machinery does not suffer from that cause. There are points of detail in the paper—I do not know whether it is advisable to enter into points of detail—in regard to the destructors, at all events in the diagrams, which appear to me to lead to very serious considerations. Mr. Adams' only method of ascertaining the relative values of refuse and coal as fuel in most cases has been to take the amount of coal burned throughout a given period, and to deduct the value of that coal from the total raised, thereby getting the value of the refuse. Mr. Cooper criticised that on the ground that the coal appears in Mr. Adams' paper to be burned in a very inefficient manner, whereas the refuse is burned in an efficient manner. I venture to say that those are the actual facts. The refuse is burned at a regular rate of burning throughout the twenty-four hours, and it is burned in a very efficient manner. The coal, on the other hand, is burned during a short period of high load at full speed, as quickly and as well as possible; but dur-

Mr. Watson. ing the rest of the twenty-four hours it is simply burned on banked fires, and most of it, in my opinion, is thrown away. If you look at the diagrams—which I may say I have never seen put forward in quite the same way before—you will see that the refuse appears to be much more valuable during the time of high load than at other times. That is simply, I believe, because of the banked fires. I venture to urge upon you that there are three ways in which that waste may be avoided. One is to trust the destructor altogether and not to have banked fires. The destructor contains such a mass of hot brickwork that it is quite impossible for it to lose its heat suddenly. I think that before you lost your heat from the destructor you would almost have time to get up your steam in an auxiliary boiler: or in case of an emergency, and a sudden call for steam, you could always fire one or two destructor furnaces nearest to the boiler with coal until you got your boilers going. I quite admit that the coal would not be burned very efficiently if burned on the destructor grates; but you start with a heated furnace, and the amount of coal which you would lose would be nothing in comparison with the amount that you lose by keeping banked fires going in the boilers throughout the day; when you really have enough steam from the refuse all the time, if only you trust to the refuse to keep you going. Another suggestion that I would make is that for the purpose of getting up steam quickly and avoiding the necessity of banked fires, one or two boilers might be fired with petroleum refuse. In that way steam could be raised to deal with an emergency. Another way, which promises well, is the method of storing the hot water and steam by Mr. Druitt Halpin's thermal storage. I daresay many of the members present know far more than I do about the results obtained at the Kensington and Knightsbridge Works. At that electricity station there is no destructor in combination, but I think the advantages which have been found there are nothing to what will be found in a combined station, where you have the refuse available all day, and where the station is naturally big enough to absorb the whole of the power. In this way you have the means of storing up power in an available form to deal with an emergency, and so avoiding having these banked fires. No doubt as long as coal is plentiful in this country, and as long as electricity can command a large price, these low fuels will be somewhat neglected and scorned. But there is another low fuel which is to be found in many electricity works where there is no refuse, to which I would like to call attention, namely the clinker and cinder which leave the mechanical stokers. In large stations where fuel is burnt with mechanical stokers at a rapid rate, the clinker and cinder which leave the stokers contain from 5 to 7½ per cent. of fuel, if not more. If that cinder were burned in suitable destructors, enough steam could be derived from it to drive the whole of the auxiliary machinery, while at the same time the clinker would be reduced in bulk and weight, and would be saleable instead of having to be barged away at very great cost. In conclusion, I would only plead for a more scientific attitude on the part of electrical engineers in regard to "rotten cabbages and stinking fish." Electrical engineers were the first engineers, outside the destructor trade, to take an interest in this low

fuel. It is curious that it should be so, because at that time the stations were for the purpose of lighting and not for traction purposes, and were only used about three hours a day; whereas sewage works and gas works and other institutions wanted the steam for a much longer period; and naturally one would have thought they would be the institutions to take it up. I think the real reason is that the electrical engineers were more enterprising, more open-minded and more ready to take hold of the question; and it would be exceedingly disappointing if now, when the undertaking has proved a success, they should pour cold water on it and kill the business which they themselves started. Mr. Watson.

Mr. J. H. THWAITES: There are one or two remarks about the paper with reference to the Hackney plant which I should like to make. The total ultimate capacity of the plant seems to me to be placed very low indeed. The figure given is only 39,200 odd tons per year, whereas the plant is capable of burning about 180 tons a day; this would run up the yearly capacity to a much larger figure, and would, I think, increase the ultimate value to something like five or six thousand pounds. I fully agree with Mr. Watson's remarks concerning the attitude of electrical engineers towards combined destructors and electricity works. I also think that the destructors are worked on the wrong principle in this country. They should be worked, as far as I can see, upon a chemical basis. If, for instance, a proper recording analysis were taken of the gases, electrical engineers could more easily keep their plants working under the best possible conditions of combustion, and by that means would get better results not only from a sanitary but also from a steam-raising point of view. Mr. Thwaites.

A plant has been erected in Denmark, from which results have been obtained much above those expected; the whole of this plant is worked on these lines, and very interesting records have been obtained.

At the end of the paper I notice the author states that the only combined station outside the United Kingdom is one in New Zealand. I do not think this is the case, because Messrs. Hughes & Stirling put up, at Frederiksberg, Denmark, in the year 1903 a plant of about three times the capacity of that at Christchurch. The Frederiksberg plant is combined with electricity works, and has been working very successfully indeed for about one and a quarter years; it also supplies steam for disinfecting purposes, and heating, washing, and cooking and so on, to a large hospital.

The paper is very interesting, but it appears to me there is some want of further figures in regard to refuse destructors other than those supplied by the engineers of the stations, because, as most of the figures regarding coal costs and so forth appear to have come from the station engineers, no absolute figures are given which will allow of real comparison between the stations themselves.

Mr. H. L. P. BOOT: I should like to make a few remarks with reference to the use of air channel bars. I might say that for some years I have been in the habit of using these air channel bars as compared with the ordinary solid fire bars, and I find they increase the life of a bar by at least six times—that is to say, the bars last six times as Mr. Boot.

Mr. Boot. long. In refuse destructor work it is a well-known fact that the bars give a good deal of trouble by burning. Another point, where some of the plants are badly designed, and the reason they are not more efficient, is owing to the design of the flues to the chimney shaft. I have been surprised to see economisers and various other fuel savers put in such extraordinary positions. If they were put more in a direct line with the flames going to the shaft, they would get a very much better result. With reference to the question of dust in ordinary electricity works, I think perhaps Mr. Watson was a little bit hard on electrical engineers. Electrical engineers in general take very great pride in keeping their electricity works clean and free from smell and dust. Unfortunately, if you were to put up, no matter how thick, a brick wall between the boiler-house and the engine-room you must still have ventilation in your engine-room. It is a well-known fact that there is always a certain amount of very fine particles of dust around a refuse destructor which would blow into the engine-room through the skylights, if it did not come through the passages or the doors in the walls. When you take into consideration the enterprise shown by many electrical engineers in adopting refuse destructors, and endeavouring to get their committees and various people to adopt these destructors, I do not think it can be laid at their doors that they have been lacking in a desire to improve and cheapen the production of electricity. They only want more proof from the refuse destructor makers that the destructors are as good as they claim. With regard to the question of clinker disposal, I believe at Fulham they are in the habit of making a fairly large quantity of bricks from this clinker. I do not know whether Mr. Adams or Mr. Fuller could supply the meeting with the cost at which these bricks are turned out per thousand; it might be of considerable use in comparing it with the ordinary marketable commodity. The point rather in which I am interested is whether it is advisable to add destructors to *existing* electricity works where boilers and ordinary fuel is at present being burned. This apparently is not dealt with in the paper, and rather tends to show that if the expense has been gone to of putting down boilers and auxiliary plant, there is not much to be gained by adding the destructor afterwards. It will be remembered that when the question of thermal storage came up we were promised that we should have some figures given with regard to the savings effected. I have never yet seen any reliable figures, and if Mr. Adams could tell us whether the whole thermal storage question has been dropped at the works where it has been installed it would be very interesting! The only other point which I should like to mention is that I noticed in looking through the analysis of the accounts at Hackney the wages of the workmen for the destructor plant are £3,611 a year, but the wages charged for running the electricity works are only £1,391 a year. It seems to me that that is rather a discrepancy, because to run a works of the size of Hackney at £1,391 a year for workmen's wages seems to me too economical—rather penalising the destructor.

Mr. Broadbent. Mr. FRANK BROADBENT (*communicated*): Those of us who have had occasion to collect and compile data on a subject of this description

can fully appreciate the great amount of labour which has been involved in the preparation of this paper. I have little to add to what has already been said on the general question by previous speakers. Those who have studied the question know that there is considerable calorific value in towns' refuse, and that in some circumstances there are advantages in combining refuse destructors with electricity works. In my opinion, a tramway load is a more suitable one for a destructor than a lighting load, unless in the latter case a sufficiently large battery is used to permit of the refuse being burned at a regular rate, whilst at the same time doing useful work. The chief interest of the paper, however, lies in the diagrams which Mr. Adams has prepared, and I am inclined to question the accuracy of the conclusions which he deduces from these diagrams. Mr. Broadbent.

Referring to Fulham, from diagram Fig. 17 the author calculates that on the peak of the load one ton of refuse provides steam for 94 units. This figure is arrived at by crediting the coal used during this period with the average rate of evaporation taken throughout the year. When we speak about an average, we mean that it is neither the best nor the worst, but a mean; and the best must obviously be better than the average. The best results are unquestionably obtained on the peak of the load, and not during periods of light load or when the fires are banked. The average consumption of coal per unit is taken at 8 lb. per unit, and this corresponds with a consumption of water of 59 lb. per unit. On this basis the refuse must evaporate over 2·4 lb. of water actual per lb. of refuse; whereas on the test load, when the plant was newly installed, the evaporation was only 1·05 lb. per lb. of refuse. Further, during this test the water consumption was 43 lb. per unit, and, as this was prior to the use of the condenser, the figure on the peak load is probably now between 35 and 40 lb. If we assume that on the peak of the load each lb. of coal only evaporates the average quantity, namely 7·4 lb. of water, the coal consumed on the peak, namely 15,230 lb., would evaporate 112,500 lb. of water; which at 37½ lb. per unit gives us over 3,000 units produced from coal instead of 1,904 as calculated by the author, an increase of about 50 per cent. This leaves about 2,250 units to be credited to the refuse, reducing the figure of 94 units per ton down to something like 65. This calculation gives about 5·15 lb. of coal per unit, and even allowing a fair margin above this, say 5½ lb., the refuse value would work out to about 71·5 units per ton. Even this result is only arrived at by the author's assumption that the refuse destructor furnaces are not forced at all during the peak, and that the fires and steam and water gauges are in the same condition at the end of the five hours as at the beginning. It, in fact, makes no allowance for drawing upon the thermal storage of the destructor itself and of the boilers.

The same line of reasoning is adopted in the case of Hackney, and in Fig. 22 a duty of 105·8 units is claimed during a peak of about four hours' duration. In the destructor test the actual evaporation was 1·159 lb. of water per lb. of refuse, the units generated being 54·19, the engines running non-condensing. This works out at about 48 lb. of water per unit, the average at present being given as 30 lb. condensing.

Mr. Broadbent. Reckoning then only 30 lb. of water per unit, the figure of 105·8 units per ton means an actual evaporation of 1·77 lb. per lb. of refuse, which is an increase of over 50 per cent. on the maker's test of only a fortnight before. Here again, the author only credits the coal with its average evaporation, whereas he gives the refuse the credit of a considerably increased evaporation on the peak. This is certainly unfair to the coal, as in a station in which the average consumption is 5 lb. per unit, the consumption on the peak when everything is working at its best would probably not exceed 3½ to 4 lb. (Mr. C. N. Russell, in his reply to the discussion on his paper read in Chicago in May, 1904, says, "in the case of Hackney, a k.w.-hour could be produced with an expenditure of 20 pounds of steam." This makes Mr. Adams' figures worse still.)

In Fig. 20, still referring to Hackney, a curve is given in which no coal is burned at all, so that the above arguments would not hold. In this case the author's calculations are based upon a peak load of two hours' duration only; and again it is assumed that the average rate of burning was maintained uniformly throughout; that is to say, that during the peak of the load the fires were not pushed in the slightest, that at the end of the two hours' run the fires were clean and in the same condition as at the beginning, steam pressure was maintained throughout, that the level of water in the gauges was the same, and that the temperature in the flues and furnaces did not fall. There are far too many assumptions here to make a record of this sort of the slightest scientific value. If the trimmer dumped a few cwt. or so more than the average into the furnaces, during this two hours' run, the assumed figures would be very considerably modified. It is more than probable that just before the peak load came on the steam pressure had been got up, the fires clinkered and got into first-rate condition, and everything possible done to pull through the peak in order to avoid starting up the coal fires and boilers. At the end of the run probably both pressure and water were down, the fire practically dead, and everything cooled down as compared with the conditions at the beginning of this run. It is, I think, very important in publishing data of this sort not to exaggerate the results, but to take all conditions into consideration and discount the values where necessary, so as to make them absolutely reliable and beyond question. Considerable harm has in the past been done to the refuse destructor industry by over-sanguine statements as to the power-producing possibilities of refuse, and it would be unfortunate if, now that refuse destructors have made such headway, they should receive a further set-back owing to expectations being raised which are not certain of fulfilment. Had the author been content to look upon the figures he gives as exceptional results, it would perhaps not have been necessary to treat them quite so seriously; but when he states that there is no reason why these exceptional results should not be regularly maintained in practice, it is important, I think, to point out that, on the facts given, there is no justification for any such assumption.

Mr. Maxwell. Mr. H. B. MAXWELL (*communicated*): I notice that the figures given for Partick in Table IV., last column, are based on the first year of working, when the greater part of heat generated was by-passed to the chimney. I think, therefore, in justice to the contractors and

this Department, that I might be permitted to add some more recent figures relating to the Partick Refuse Destructor and Electricity Works. Mr. Maxwell.

Date, 1904.	Refuse Burnt. Daily Average.			Refuse Burnt. Total for Period.			Current Total Generated in B.T. Units.	B.T. Units Generated Per Ton of Refuse.
	Tons	Cwt.	Qrs.	Tons	Cwt.	Qrs.		
Whole of September	34	17	0	871	9	1	43,537	50
Week from 19th to 24th	38	6	2	229	19	0	12,428	54
Month ending 26th October	40	12	1	1,055	18	1	63,360	60·06
Month ending 26th November ...	42	1	2	1,136	5	3	71,584	63

During part of the above period the economiser has been out of use, and the steam piping has been partly uncovered, owing to extensions, while condensers have not yet been added to the engines.

Better results are anticipated when present alterations and extensions of electricity works are completed, and condenser running.

During the whole month of September, only one week's refuse was of first-class quality, the remainder being rather below the winter average.

Mr. W. C. P. TAPPER (*communicated*): The paper which Mr. Adams has read contains a very large amount of information on an important subject, compressed (at least so far as the Tables are concerned) into a small compass, and the thanks of the members are certainly due to Mr. Adams for the trouble he has taken in collecting the data given in the paper. Where so large a mass of figures are tabulated, a few discrepancies are sure, however, to creep in, and so far as Stepney is concerned, there are one or two I should like to correct. For instance:— Mr. Tapper.

Table I. The steam-pressure limits are stated to be 170–200. As a matter of fact, variations from 200 down to 130 are quite frequent.

Table II. I am not quite sure to which year Mr. Adams is referring in this Table, but in any case, at the date when we had three boilers our feed-water heaters had not been installed.

Table III. Mr. Adams states that the full heat value of the fuel was not developed. If by this he means that some of the heat was bypassed, I think he must have been misinformed, as the destructor has never been able to furnish steam for more than about 270 k.w. without the use of coal in addition to the refuse. This is on the assumption that one out of the six destructor boilers is laid by for cleaning.

Table V. Can the author state how it is that the cost of collection is so high at Fulham and Hackney as compared with other places?

Mr. Tapper. With regard to Table VIII., I consider the coal cost per unit given in columns 10 and 11 is entirely misleading in any combined works, for reasons to be stated later.

Table IX. In column 5 Mr. Adams states that the additional cost of the combination is three destructor boilers. This figure, for reasons to be given, should be *five*, with, of course, a corresponding alteration in the figures deduced from it. Also with regard to column 12, this amount is not actually saved.

Saving in Capital and Revenue.—Turning to some of the questions discussed in the paper, Mr. Adams refers at some length to the saving resulting from the combination, both on capital and revenue accounts. In some stations no doubt a considerable saving has been effected in area of site and in plant, but this undoubtedly aggravates the dust and nuisance trouble. In Stepney, owing to the two undertakings being entirely independent, no saving whatever has been effected. We have absolutely sufficient boilers to carry our full load quite independently of the destructor, and it would not be necessary to increase the staff, as our boilers are under steam almost throughout the 24 hours. I therefore venture to differ entirely from Mr. Adams when he states that two boilers are saved to the Electricity Works, together with £200 per annum in labour.

Coal Cost per Unit and Charge per Unit.—Mr. Adams states that he is of opinion that the coal cost per unit should be taken as a basis for calculating the credit to the destructor accounts. If by this he means the coal costs as set out in columns 10 and 11 of Table VIII., then I entirely disagree with him. If you will refer to Figs. 4 and 5, pp. 273 and 274, and compare the load-factors of those portions of the curve on dust steam and coal steam respectively, it will be obvious that a large proportion of the coal used is purely standby. Indeed the author admits this when he says, on page 293, referring to the coal used at the Grays Works: "The total coal used during the year was 348 tons, a large proportion of which was used in getting up steam to assist the destructor for a short time every day." I say that if that portion of the curve generated on dust steam had been generated on coal steam, the coal costs per unit would have been very considerably reduced; and this is borne out by the fact that now we at Stepney have to supplement the destructor on the day load—in other words, now that our boiler load-factor has gone up, our coal cost has dropped from 0·8d. to 0·6d. per unit mentioned in the paper at the present time. To be fair, therefore, in determining the value of the dust steam, the coal cost per unit when dealing with the *whole* load should be taken. These figures are not at present available in all cases, and even when obtained no small discount should be deducted to allow for the inferior quality of the steam supply.

Accounts.—On the question of accurate account keeping, I am quite at one with Mr. Adams, and I am not without hope that we in Stepney will be able to follow Hackney's excellent example and publish a destructor balance-sheet.

General Question.—With regard to the question generally, Mr. Adams certainly makes out a very good case for the combination of

dust destructor and electricity works. Some of the results obtained are a surprise to me; that anything approaching 100 units per ton of refuse has been attained is remarkable. Notwithstanding this, however, after over four years' experience of the results of combined working, I am decidedly of opinion that if other uses can be found for the steam (such as baths and washhouses, heating, disinfecting, etc.), then the combination is *not* desirable. It is quite impossible to exclude the fine dust from the engine-room and the buildings generally. The steam pressure is utterly unreliable, depending on the varying nature and dampness of the refuse from hour to hour. Priming and wetness of steam result from the rapid changes in pressure. Pipe joints give out, commutators suffer, and the voltage regulation requires unremitting attention. Mr. Tapper.

Mr. W. A. VIGNOLES (*communicated*): The only point in connection with this paper that I wish to draw attention to is Mr. Adams' method of ascertaining the number of units generated per ton of refuse in a combined station. He takes an average figure for the pounds of coal per kilowatt-hour, and from this works out the units per pound of refuse by calculating the number of units generated by the coal-fired boilers, and crediting the remainder to the destructor. This is, I think, an incorrect method of working out this problem, as the pounds of coal per unit and pounds of steam used per unit would vary according to the load on the dynamos. I think this may be the explanation of the excellent results obtained at the peak of the load in the various destructors; the load at that time being very large, the steam consumption is small, and consequently the steam coming from the destructor can supply a large number of units; and, for the same reason, I think the coal is not always credited with the full number of units for which it is responsible. Some system of checking the results obtained by Mr. Adams' ingenious method is desirable, as his results cannot be considered conclusive without confirmation. The only really reliable test is for Hackney, when the whole load was carried by the destructor. It is not possible to analyse Mr. Adams' figures in this matter, as although he gives figures for the coal consumption and the steam consumption in two or three cases, it is not clear if the steam consumption mentioned is the average over 24 hours, or if it is taken at any particular load. I do not think it will be possible to maintain the excellent results apparently obtained on the peak of the load at various works for 24 hours until the average becomes as large as the present peak load. Mr. Vignoles.

AUTHOR'S REPLY.

Mr. W. P. ADAMS: I propose, instead of dealing with each speaker separately, as is the usual custom, to deal with the various headings under which remarks have been made. In the first place, the selection and reservation of refuse, which Mr. Highfield suggested was the reason for obtaining good results on the peak in many of my curves, is impossible in large works like those in London. It might be possible, perhaps, with small works burning under 10,000 tons, but any reservation of good refuse in any of the London works would be seen Mr. Adams.

Mr. Adams. to be quite impossible by anybody who pays a visit to those large works. The dust nuisance has been mentioned by several speakers. Mr. Highfield stated that a destructor was not a nice neighbour. That, I believe, is so, and one can sympathise with the electrical engineer in charge of a combined works, because the dust and smell is more than he would experience in works run in the usual way. But, at the same time, I think it is rather overrated. For instance, Mr. Highfield himself, in his paper on the St. Helen's works, read at Glasgow in 1901, stated that "considerable care was taken to prevent the dust from the destructor getting into the power-station. It was almost impossible to do this quite successfully, but no serious harm was done by the small amount that did enter." These are Mr. Highfield's own words, and I take it that they are a very fair statement of the actual fact. Mr. Robinson also referred to this subject; but I would ask that the question be considered upon the basis of the extra cost of repairs to machinery and cleaning, because I think if it is looked at from that point of view it will be found to be a very negligible set-off against the fuel value of the refuse. I have been misunderstood with reference to some remarks I made in the course of my paper about storing refuse, and I should like to clear myself in that direction. My remarks have given rise to such strong expressions of opinion that I might almost have advised the storing of summer refuse for burning on the winter load. Of course that was not in my mind. The storing of refuse that I referred to was the daily storage for short periods, which is quite possible, and is actually carried out at the present day in many of the small and medium-sized works.

Mr. Russell, of Shoreditch, spoke of a method he has been testing for reducing the clinker to small proportions, which is a very important subject indeed. By studying the tables at the end of the paper, it will be found that the disposal of clinker is only second in importance to the question of obtaining the full fuel value from the refuse, so that it is an extremely important matter to reduce the quantity, if possible, and to improve its quality. By these means the difficulty of getting rid of the clinker will be largely overcome. Mr. Russell states that he has reduced his quantity of clinker to as little as 17 per cent. As the average amount of clinker obtained from refuse is about 33⅓ per cent., this is a very considerable gain, but particulars of the cost of the process are necessary before we can form an opinion of its commercial value. Mr. Robinson, of Hackney, suggests that there are legal difficulties in the way of the Local Authority working up and selling clinker. If this be so, I think every effort should be made to remove such legal difficulty, in view of the importance of the question. I cannot give Mr. Boot any particulars about the clinker bricks at Fulham, but in passing I might say that at Fulham during the last financial year the clinker fetched altogether about £500, and the costs of working it up came also, I understand, to about £500, so that they practically balance each other. Still, to get rid of the clinker without any extra cost is a very great gain.

With regard to the accounts between the works, I pointed out in my paper how important it was that proper accounts should be kept,

and I think every credit should attach to Mr. Robinson for the careful way in which the accounts at Hackney are kept; they are quite intelligible and fair. I quite agree with him when he says: "Any process of account keeping which could possibly reduce the apparent cost of refuse destruction is to be deplored; the result would be an apparent increase in the electricity undertakings costs, and consequently a real increase in the price charged." The tendency is generally quite the other way; the electricity works costs are often reduced in a combined works at the expense of the destructor, and the consumer gets cheap electricity, to the disadvantage of the ratepayer. You will see, then, how important it is that the accounts should be properly kept and adjusted. I do not think the Fulham method at all a fair one, because the true fuel value of the refuse can never be properly ascertained, and it is most important that it should be. With regard to the capital charges, Mr. Robinson said it was not fair that I should include loan charges in the costs. I do not quite see why, because if the capital expended on a destructor happens to be large in any one particular case, it is obviously with the desire to save working expenses; generally speaking, the extra expenditure is on labour-saving appliances, and it is only fair that the capital charges should be included when comparing the destructor costs of different combined works. Besides, the figures given in the paper are Mr. Robinson's own, so that I hardly feel it fair that it should be suggested that my figures do not represent the true state of affairs. Mr. Adams.

I think I have been misunderstood by Mr. Highfield in my vindication of Professor Forbes. It is quite obvious that Professor Forbes was only dealing with private lighting, and not with a power load and the numerous other uses for electricity which have sprung up in recent years. If I need to give any evidence of my attitude on the matter, I have only to point to the protest which I entered when describing the Shoreditch undertaking. There I pointed out that such a claim was made before the works were installed, and there is trouble still in Shoreditch in connection with that claim. Mr. Russell objected to my use of the expression "units per ton," as it does not give the steam-raising value of the destructor works. You will see that a good many different factors enter into this question, and it is impossible to devise a unit which would express the figure of merit of a combined electricity and destructor works. I am afraid, therefore, we shall have to be content with some such compromise as "units per ton"; but I would point out that in Table III. I give particulars of the evaporation which Mr. Russell asked for, and, what is still more important, in Tables VII. and IX. I give the actual cash value, so far as it is possible to ascertain it, of the refuse steam. The poor results obtained at some combined works need explanation, and as an instance I have only to refer to St. Helens. Mr. Highfield tells us that after about six years of working no better results than 36 units per ton are obtained all the year round, and the all-day load in the station must be largely beyond the capacity of the refuse. The coal cost at St. Helen's is 0·2d. per unit sold, which is almost a record value, and there are very few works in the kingdom where such a figure is obtained. With so favourable a coal cost one

Mr. Adams. would naturally expect a good value from the refuse. At 0·2d. per unit sold and coal at 6s. 9d. per ton the coal burnt per unit generated equals 5 lbs., and, assuming the good value of 7 lbs. of steam per lb. coal, the water consumption per unit generated is 35 lbs. Taking Mr. Highfield's value of 36 units per ton, the refuse value is only 0·55 lbs. steam per 1 lb. refuse, roughly one-third of the value obtained on test. This is surely a remarkably poor result for a Lancashire town where the fuel value of the refuse is above the average.

My diagrams have been closely criticised, notably by Mr. Cooper and Mr. Broadbent, who consider there are too many assumptions on my part. My paper is already too long, and had I attempted to give chapter and verse for every detail it would have been voluminous. I may say that in preparing my paper and diagrams I had the advantage over both these gentlemen of obtaining my information by personal investigations at the stations. Until I undertook this investigation the possibility of reaching 100 units a ton never seems to have been realised, but there is no question that it is obtainable, and is being obtained, and the figure will become more familiar in the future. It is quite obvious that, with an economical generating plant, it does not require a large amount of steam to produce such a figure. For instance, if refuse can be found to give 1½ lbs. of steam per lb. of refuse—and this is not an unusual figure—in a station where 35 lbs. of water are used per unit generated, the result will be 96 units per ton. I may point out in passing, too, that test figures are not always representative of what is best in any particular installation. The tests are nearly always made with cold feed, and often with cold furnaces and cold flues to start with; where a destructor has been set regularly to work and is working from week end to week end, then the conditions are distinctly better. Also it does not follow that when a given test is undertaken the best kind of refuse is obtained at that particular time. I might point to the Woolwich test as an additional illustration of 100 units a ton being reached. Diagram 24 is interesting on account of the high value of water per lb. of refuse which was obtained, namely, 1·917. This, I think, is a record figure for a test in London, and it shows what London refuse is capable of doing. During the peak period the generation was only 1·78 lbs. per lb., and at 40 lbs. of steam per unit generated that result was obtained.

I have some very interesting figures before me of coal consumption in electricity works, which appeared in a paper read by Mr. Giles* in the early part of 1904, on "Coal Consumption in Central Stations." Exception has been taken to the peak value which I have assumed for the Fulham works, namely 8 lbs. Referring to Mr. Giles's table, I find that 8 lbs. is not at all an unusual figure in works which are condensing, with a good load-factor, and with an output a good deal in excess of that at the works at Fulham and also Hackney. At Bootle I find it is 9 lbs.; at Halifax, 9·6 lbs.; at Bristol, 8·6 lbs.; at Leeds, 8·4 lbs.; and at Devonport, 9·6 lbs. Then, again, I consider that the figure originally taken by Mr. Robinson for Hackney, namely 5 lbs., is quite a fair one to the electricity works. The figures which Mr. Robinson gave us at

* *Electrician*, vol. 52, p. 530.

Mr. Adams.

the last meeting are quite unusual when one bears in mind that in Mr. Giles's table, to which I refer, there are only four works which have such a low coal consumption, viz. Bradford, with 12½ million units output and a load-factor of 20; Sheffield, 7½ million units with a load-factor of 21; Sheffield tramways, 9½ million units with a load-factor of 37; and Glasgow tramways, 15 million units with a load-factor of 47. I need hardly say that those outputs and load-factors are very greatly in excess of those obtained at Hackney.

The curves naturally form a somewhat interesting part of the paper, and I should like to add to the information which I have already given a few more particulars which I think will help to clear the air of some misunderstandings with regard to these curves. It is very difficult to get away from Fig. 20. There no coal at all was burned, and we get a peak value of 104 units per ton. I might say, while discussing the curves, that I do not claim that the values are right to a decimal place. There are so many things to be taken into consideration, as you will see further on, that it is almost impossible to get at the exact value, and one must be content with an approximation; after going most carefully into the matter, I think I have arrived at a very fair idea of the true value. Fig. 20 shows that 100 units per ton is not an impossibility at Hackney, but is actually obtained. If the top of the peak is cut off and brought down to a trifle under 600 kilowatts, the value is still 94 units per ton. As a matter of fact, Mr. Robinson states that he can nearly always depend upon an output of 600 k.w. from the destructor, and that taken by itself is sufficient to show that 100 units per ton is a fairly regular performance. There is some misunderstanding with regard to the value taken on the peak for coal. Mr. Watson was rather under a misapprehension regarding the coal value when he stated that I am not justified in taking the average value at the works for the peak. It must not be forgotten that in the case of the Hackney works the whole of the coal appears on the top of the peak; there is nothing taken off for banking, and therefore the average value is the peak value, and there is no getting away from it. In the case of the Fulham curves it is not quite so simple. With reference to Figs. 16 and 17, I can give a few more interesting facts. The coal value on the peak (Fig. 17) is taken at 8 lbs. per unit generated, and I have endeavoured to show that this is not an outrageous figure, and that the conditions at the works warrant such a figure being taken. Assuming it is not fair to take the average value, I would point out that the coal plotted includes a certain amount of coal for banking. Then, looking at the two other shifts which are plotted separately, no coal was used for banking in the first shift, and all the coal on the second shift, which is a very small quantity in this case, was used for banking; so that when one considers the quantity of coal burned in the earlier parts of the day with that burnt in the latter part, and also it is understood that coal burnt on the first and second shifts is burnt under better conditions of feed temperature and vacuum than that on the third shift, there cannot be much difference between the average value and the peak value. Then, if it is objected that I have taken too high a value for the coal, and that it ought to be 7 or even 6 lbs., I would point out that I

Mr. Adams. have taken the average rate of burning the refuse at 120 tons per diem. On the series of diagrams for 1903 and 1904 it will be seen that every one of them is marked 120 tons of refuse burnt. It just happens that in Fig. 16 only 115 tons of refuse were delivered that day; on the following day 114 tons were delivered. And, more, the refuse that was delivered on Saturday was being burnt up to nine o'clock on the following Sunday morning, so that I have a very substantial reduction to make from the quantity of refuse burnt, if I wish to do so. It will be seen at once from these figures that one can only arrive at an approximate value, and that 94 units a ton, taking a lower coal value, and making some allowance for a smaller quantity of refuse being burnt, will still probably be about right. I have been misunderstood with regard to the water consumption at Fulham. I have mentioned that, on a test, it was recently found that 59 lbs. of water were used per unit generated. That was a single test, and there is no reason why the water consumption should always be on such a scale. It is quite possible that the 8 lbs. of coal may generate as little as 40 or 45 lbs. of steam; it depends entirely on the evaporative efficiency of the boiler plant under different conditions, and I think it is extremely likely that if a test were made it would be found that the water value per lb. of coal was somewhat low at Fulham under running conditions, so accounting for the somewhat large amount of coal used per unit generated.

In conclusion, I should like to bring to your notice—if it is necessary to do so—in support of my point that 100 units a ton is a figure that is quite possible and likely to be heard a good deal more of in the future, that I have before me here a curve from Partick for December 16, 1904. This curve shows that nearly the whole value of the refuse has now been obtained. For several months past Mr. Maxwell has been obtaining over 60 units per ton. That in itself is a very good figure; but when I tell you that the water consumption is 55 lbs. of water per unit generated, you will see that there are possibilities of improvement even upon this. I may add that the 55 lbs. of water per unit generated is when running the plant non-condensing. Mr. Maxwell is shortly starting condensing, and expects to get better results; assuming that he gets such a good efficiency as 35 lbs. of water per unit generated, the refuse will give exactly 100 units per ton.

In conclusion, the results obtained five years ago are surely not to be considered as representing modern practice, and that 30 units per ton is an adequate return for the fuel value contained in the refuse. Mr. Broadbent is especially pessimistic and anxious to explain away the figures, but I am afraid facts will prove too hard for him, and in a few years he will be forced to admit that the "exceptional results" of to-day will then have become the standard of good practice. Mr. Tapper gives a terrible catalogue of evils to be met with in combined works. I would ask him one question—Why not try and overcome them? Several other gentlemen have spoken of difficulties and the disadvantages of combined works—is it possible that the difficulties are insuperable?

Finally, I hope that the facts which I have managed to put

together in my paper will lead to some little emulation amongst the managers of combined works in the matter of obtaining good steam value from the refuse. There is at present, I am glad to say, some emulation in the matter of destructor costs, just as there is with electricity works costs, but a little emulation in the matter of burning the refuse to the very best advantage would be a great gain. Mr. Adams.

The PRESIDENT: Gentlemen, I need hardly ask you, after the applause with which you have greeted the paper, to give your best thanks to Mr. Adams for having brought forward such an extremely interesting subject, and for the good discussion which it has provoked. There is one point which Mr. Watson mentioned in his remarks which I should like to emphasise. Mr. Watson said that there seems to be a danger of the Institution falling into two parties, the enthusiasts and the cold-water people. I think, however, that such a division serves the extremely useful purpose of securing a fair hearing on all sides, and that the interest in any discussion of a paper will be best maintained where we have an enthusiastic party and the cold-water party. I ask you now formally to record your thanks to Mr. Adams for his paper. The President.

The resolution was carried by acclamation.

Proceedings of the Four Hundred and Sixteenth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 12, 1905 — Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on December 15, 1904, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

Simon L. F. McLauchlan.
Walter Markby.
E. Albert Mitchell.
Jas. Edmund Sayers.
Herbert Thomas Sully.

From the class of Associates to that of Members—

Vivian B. D. Cooper.

From the class of Associates to that of Associate Members—

Joseph Boyce.
Edw. Vincent Clark.
W. de M. Landon.
Charles Stewart.
Joseph Taylor.
Frederick Walton.

From the class of Students to that of Associate Members—

Patrick G. O'Hara.
Harry Rust.

Messrs. W. W. Cook and W. Henderson were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Members.

Morton Beales.
James B. Henderson, D.Sc.
Alexander McClelland.
Albert Mitchell.

Associate Members.

William Allen.
John Anderson.
James B. Ballantine.
Philip H. Bertin.
Arthur D. Broadbent.
Paul Colbeck.
John Filmer Collard.
James Newall Dauncey.
Augustine G. Davis.
Boyd Dawson.
Frederick H. Dennis.
John William Donnet.

Audley Meryvn Duke.
Allan Edgar Eyears.
John Barnett Feltham.
Aubrey Fletcher.
Frank Purser Fletcher.
Robert Foster.
James Wilfrid Harris.
Charles Nelson Hefford.
Samuel G. Hobson.
Selwyn W. Humphrey.
Arthur W. Jones.
Lionel C. Knocker.
George Anslow Lister.
Cecil McCoull.
Wilfrid McWilliam.
William Alfred Neff.
Edward A. Nesbitt.
Reginald H. Pearson.
George Henry Phillips.
George P. Shallcross.
Percy Cunliffe Simpson.
Charles Edward Smith.
Robert W. Smith-Saville.
Mark Wray Staight.
William Stansfield.
William Henry Story.
Wilfred B. Thorpe.
Hanson Topham.
Geoffrey Trevithick.
John Harry Wild.
Jas. Henry A. Woods.
Alexander D. Wright.

Associates.

Charles V. Bellamy.
Louis Cassier.
Thos. Maltby Clague.
Alfred S. Fletcher.
Fred. Walton Green.
Wm. Fred. Taylor.
James W. Warnock.
Thomas C. Woolams.
Frank T. Wright.

Students.

John Tom Atkinson.
Albert Blanks.
Herbert Fred. Bowden.
Fred. C. Ross Brown.
Hubert Chas. Bullman.
John J. Cardwell.
Francis Farrar Carter.
Herbert Caswell.
Philip B. Clarke.
Stewart N. Clarkson.
Richard F. Collum.
Matthew H. Curry.
George Dearle.
Richard N. Eaton.
George Guy Ewer.
Edmund W. K. Foote.
Walter S. Forman.
William E. Foulkes-Jones.
Prosper B. Frost.
Reginald L. Godfrey.
William H. Grinsted.
William R. Harding.
Gurth Johnson.
Sidney D. Lancaster.
Stanley Mack.
James Morgan.
George Horatio Nelson.
George P. Nunneley.
Arthur John Pearson.
Geoffrey K. Podd.
Augustus R. P. Price.
Leon Maxime Rampal.
Hugh Stanier Reid.
Kenneth M. Roffe.
Guy Septimus Rose.
William G. Rowse.
George Henry Sargent
Frank W. Skinner.
Arthur C. Smith.
Howard Spooner.
Edward F. Steele.
Alick Lionel Tackley.
Edmund G. Townshend.
Bernard A. Tubini.
George E. S. Upsdell.
Edmund John Vaughan.
Cresswell J. Vickery.
Rupert Kenley Webster.
Harold Weston.
John Eason Wilks.
Thomas Owston Wilton.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Gauthier Villars, Constable & Co., C. Delagrave, British Westinghouse Co., and the Patent Office; to the *Building Fund* from Messrs. A. C. Anderson, H. G. Beeton, I. Braby, Major P. Cardew, R. A. Dawbarn, W. Duddell, H. J. Eck, *The Electrician*, V. W. Gill, F. A. Glover, W. Golledge, C. W. Hacking, R. Hammond, H. E. Harrison, Lt.-Col. H. S. Hassard, Prof. A. Hay, F. C. Heritage, T. E. Ingoldby, Capt. H. B. Jackson, G. F. R. Jacomb-Hood, H. W. Miller, A. W. Mindo, W. M. Mordey, E. D. Morgan, L. R. Morshead, F. H. Nicholson, W. G. T. Pope, S. R. Roget, B.A., J. Shaw, M. Solomon, and A. Stroh; and to the *Benevolent Fund* from Messrs. O. M. Andrews, Sir B. Baker, I. Braby, W. A. Buckland, G. Byng, M. Byng, W. C. Clinton, S. N. Clirehugh, H. W. Clothier, F. N. Clough, P. R. Cobb, W. J. Crampton, H. Dagnall, V. J. Delebeque, F. W. Dennis, A. Denny, H. C. Donovan, B. M. Drake, G. Draper, W. Duddell, J. Eck, F. Espir, E. Garcke, F. Gill, R. K. Gray, F. Gripper, N. Gunz, R. Hammond, H. E. Harrison, G. W. S. Hawes, Hawtayne & Zeden, K. Hedges, H. Hirst, S. H. Holden, E. S. Jacob, H. W. Kolle, G. J. Lemmens, Ede M. Malan, Sir H. C. Mance, C. H. Merz, H. W. Miller, A. W. Mindo, W. M. Mordey, S. Morse, F. H. Nicholson, Hon. C. A. Parsons, W. H. Patchell, C. C. Paterson, W. G. T. Pope, A. H. Preece, T. Rich, Sir D. Salomons, F. Samuelson, G. H. Sayer, W. H. Scott, A. T. Snell, C. P. Sparks, A. Stroh, Sir J. W. Swan, W. C. P. Tapper, C. Turnbull, T. C. T. Walrond, F. D. Watson, C. J. Wawn, J. Wetzler, J. G. Wilson, R. P. Wilson, and C. H. Wordingham, to whom the thanks of the meeting were duly accorded.

The meeting adjourned at 9.40 p.m.

The discussion on Mr. W. P. Adams' paper was concluded (see page 305), and the following paper was read :—

FUEL ECONOMY IN STEAM POWER PLANTS.

By WILLIAM H. BOOTH and JOHN B. C. KERSHAW, F.I.C.

(*Paper read January 12th*, 1905.)

SYNOPSIS:—Introduction. *Feed-water:* Analysis—Systematic control of supplies—Softening—Scale and grease troubles—Preheating—Thermal Storage. *Fuel:* Sampling—Testing—Systematic control of supplies—Suitability for boiler-furnace. *Air:* Preheating primary and secondary air supply. *Boilers and Furnaces:* Types of—Setting—Hand-firing *v.* mechanical stoking—Losses by radiation and air-leakage—Smoke prevention. *Steam:* Superheating—Condensation and radiation losses. *Waste Gases:* Heat losses in—Sampling—Testing—Systematic control of. *Draught and Regulation of Air Supplies:* Natural *v.* artificial draught.

INTRODUCTION.

In presenting this paper the authors are aware that economy in most or all electrical power plants, as usually understood, is at present unattainable.

The lighting station, the load of which at every moment of its working hours can, with care, be arranged to suit the economical load of the running plant, is nevertheless seriously handicapped by the very short length of time during which the total plant is at work. The possession of a really durable and efficient storage battery would raise the load-factor of the plant probably eightfold.

The traction power-house, the load-factor of which, for tramway work, varies approximately with the cube root of the number of cars run, cannot be worked economically, because, though all the power in the station must be run all the working hours, excepting of course necessary spares or stand-by plant; yet the load on the running plant varies from a maximum to a minimum every few minutes, and the mean load of the plant, except in the case of systems of several hundred cars, is but a small proportion of the total maximum load, and the steam engines at least must be wasteful. Here again economy in its fullest sense is a question of accumulators.

The authors assume that the fullest economy of first cost, and more especially of running cost, are recognised by all station engineers as only obtainable, in the present state of knowledge, by means of a battery. But they are also aware that the battery is so far from perfect that its adoption in any case must still be a matter of grave and careful deliberation, and that the greatest economy ideally possible is too controversial to enter into in this paper, save incidentally.

But there are numerous economies possible in every power plant, and an endeavour will be made to point these out in the hope of stimulating the search after possible and practicable economy, to suggest thoughts on which all station engineers may work—thoughts many of them suggested to the authors by engineers who have directed their own energies in certain directions more than in others. Economy, commercially considered, is not identical with economy of first cost, nor yet with economy in fuel or wages. Nor can commercial economy be defined even as that compromise which makes the best return on the outlay, for there are other considerations which are sometimes paramount.

It is, for example, justifiable to economise plant in a traction station down to such a point that, at occasional special loads, the cars shall run a small percentage slow. But the parallel happening in a lighting station is altogether undesirable and defeats its own ends, because the public who use current are not yet trained to switch off some lamps when current fails.

In all which follows, therefore, it must be understood that every means to economy that is put forward must be considered afresh for each particular case, for its adoption may be good or otherwise, according to the company in which it finds itself, and, though the authors are particularly concerned in fuel economy pure and simple, they are not unmindful of the discounts to be made on account of commercial considerations.

Steam power economy is to be secured by attention to fuel and its combustion, feed-water and its treatment and heating, furnace design and arrangement, steam treatment and its use in suitable engines.

The subject divides itself into the several heads as follows :—

I. Feed-water.	IV. Steam.
II. Fuel.	V. Waste Gases.
III. Boilers and Furnaces.	VI. Draught.

In such a paper statistics would only lead to controversies that might obscure the intention of the paper, which is rather to emphasise principles than to indicate beaten paths.

FEED-WATER.

Most electrical engineers are now ready to admit the importance of attention to the physical and chemical characteristics of their feed-water supply, and they recognise the impossibility of obtaining the highest efficiency from their boilers, if the water is passed into them without preliminary heating, carrying all the original suspended dirt and dissolved impurities.

Only very few natural waters are fit for use in steam boilers, without some preliminary treatment; chemical examination can alone reveal whether this treatment is required, and what must be its character. It is not the authors' purpose to go at length into the theory and practice of water-softening for steam-raising purposes, and they will only deal very briefly with a few of the more important points arising in a considera-

tion of the subject. The practical side of the subject was dealt with very adequately by Messrs. Stromeyer and Baron in a paper read before the Institution of Mechanical Engineers last session.

Analysis.—The examination of a water intended for steam-raising purposes should cover its acidity or alkalinity, presence of oily or fatty matter, total solids, temporary and permanent hardness. A trained chemist is required for this work. The examination of the water supply should be repeated at monthly intervals, since changes occur in the composition of all natural waters, according to the period of the year and the rainfall. Water taken from rivers or streams near the coast is subject to contamination by sea-water at the periods of flood-tide in the spring and autumn, and this danger must be recognised and guarded against.

When softening apparatus is installed, whether of the automatic or intermittent type, daily tests should be made of the water leaving the apparatus, since there is grave risk in many cases of excessive amounts of chemicals being used for softening. This excess produces priming and other evils in the boilers. The authors have seen at a north country works an ingenious automatic arrangement of twenty-four vessels for retaining this number of separate samples of the water during the day's work, each one filling in succession and requiring one hour for this purpose. The chemist at these works spends half an hour each morning in this sampling place (which is kept locked), and tests in this time selected samples of the previous twenty-four hours for alkalinity and hardness. Some check of this kind is imperative, since even the most reliable types of automatic softening apparatus are apt at times to get out of order, and variations in the composition of the water supply, as already noted, occur during the spring and autumn months of the year.

Softening Apparatus.—All waters containing more than 15 English degrees of total hardness (*i.e.*, an equivalent of 15 grains of calcium carbonate per gallon) should be subjected to treatment with chemicals before use in the boilers; and in large steam-generating plants it would probably pay to put in a softening apparatus when the water employed tests above 10 degrees of hardness.

The addition of "boiler compositions" to the feed-water as it enters the boilers is unscientific and bad. A boiler is designed for making steam, and not for carrying out half a dozen other operations, which sellers of "secret" compositions and unwise engineers sometimes attempt in it. External treatment of the water in an apparatus, which permits removal of the deposit before the water enters the boiler, is therefore an essential condition of scientific steam-boiler management. The authors do not propose to discuss the relative advantages of automatic and intermittent softening apparatus. Each type has its advantages and disadvantages, but scientific control is required with one, as with the other, to obtain the best results; and, in the absence of this, boiler troubles are certain sooner or later to ensue.

Scale and Grease Troubles.—A thin coating of scale upon the boiler plates is advantageous, since it protects the iron from pitting and corrosion. When this coating has increased to one-twentieth of an

inch in thickness no further advantage is gained, and the rate of heat transfer through the plates is diminished enormously as the scale increases in thickness. The heat-conducting power of scale is stated by various authorities to be between one-thirtieth and one-hundredth that of boiler plate, and should the scale contain oil, its conductivity for heat is still further reduced. The presence of oily matters in scale can be easily detected by heating a small portion of the scale upon a platinum spatula in a Bunsen flame; the scale, if it contains oil, chars and gives off a disagreeable odour under such conditions. Oil in the water-supply, and oily scales are a cause of pitting and corrosion; since animal and vegetable oils yield decomposition products of an acid character, at temperatures which may easily occur within the boiler. The examination of the water-supply must, therefore, include tests for oily and fatty matters, when water from the exhaust is employed. When mineral oils are used for lubrication this danger, of course, is not so great, since these oils do not yield fatty acids on decomposition; but even in this case it is the better plan to keep the oily matters out of the boilers, by efficient filtration. The addition of caustic soda to the feed-water as it enters the boilers, in order to saponify the fatty matter, is not to be recommended, since soapy water leads to priming and other troubles only a little less serious than those caused by oils.

Although the complete separation of oil and grease from feed-water s not a simple matter, it can be effected with well-designed apparatus intelligently worked, and references to articles and papers on this subject are given in the Appendix.

As a guard against pitting and corrosion, the authors recommend the use of zinc slabs and rods, bolted in suitable positions to the stays and angle plates of the boiler. The action of this metal is too well known to require comment; and even if its electro-chemical action does not come into play, these metal projections into the body of boiling water will help to promote regular steaming in the boiler.

Preheating and Thermal Storage.—The advantage of feeding the boilers with water approaching as nearly as possible to the boiling temperature is now fully recognised. Not only does this promote more regular steam generation and, therefore, the efficiency of the boiler plant, but it also accelerates the softening process, since the chemical reactions upon which this is based take place much more rapidly in hot solutions than in cold or warm ones, and the precipitates obtained are more easily settled out in the former case.

The only question regarding the value of preheating upon which engineers are now disposed to differ, relates to the use of live steam for this purpose. It is admitted that all the waste heat of a boiler plant, whether of the exit chimney gases or of the exhaust and condensing water, should be utilised, if possible, in the feed-water supply, and that the heat obtained in this way is clear gain. But, the advantages of using live steam, in addition, for heating the feed-water up to 212° F. or above that limit, as in the *Thermal Storage* system, is not by any means fully recognised, and there is still a disposition to regard this method of using live steam as uneconomical and waste-

ful. The authors do not agree with this view. The heat of the live steam is retained in the feed-water heater, and the advantages to be gained by having all the water heated to a temperature of 212° F. or higher, far exceed the slight loss of heat which occurs by condensation and radiation from the connecting pipes between the preheater and the boilers. Of course this loss must be minimised by placing the feed-water heaters close to the boilers.

The real cause of the increased efficiency which independent authorities (as, for instance, Mr. Miller and Col. Crompton) assert has been obtained from boilers fed with water at or above 212° F., is not yet settled. If water containing solid matter in suspension be heated in a glass vessel, it can be observed that convection currents are set up which continue until the boiling temperature is attained. These currents must necessarily absorb some portion of the heat energy of the fuel, and convert it into mechanical work. A possible explanation of part of the gain observed when feeding boilers with water at or above 212° F. is, therefore, that the loss of heat due to the performance of merely mechanical work is avoided, and the whole of the heat-energy of the fuel is devoted in the boiler to the conversion of the water into steam. In the feed-water heater as usually designed, convection heating does not take place to any great extent, the water being heated by falling through the steam in the form of mist or spray.

This explanation finds some support in a paper read by Mr. Hamilton before the Belfast Mechanical and Engineering Association in 1902, and quoted in a leading article in *The Engineer* of August 19, 1904. In this paper Mr. Hamilton pointed out that the hotter the water inside the boiler, the greater is the amount of heat which passes through the boiler-plate per unit of superficial area, the measure of relative efficiency being the gain in temperature when not steaming, and the water evaporated when steaming. The increased heating power when steaming represented a gain of about 100 per cent. in the normal heat-transmitting power of the boiler plates.

The subject is well worth more study than it has yet received, for the facts relating to the gain in boiler efficiencies when fed with water at or above 212° F. seem indisputable, and when a satisfactory explanation of the cause has been given, engineers will no doubt be ready to adopt preheating and thermal storage plant to a much greater extent than is now the case. The steam-boiler of the future is likely, in fact, to be specially designed for this double duty of preheating its feed-water and providing steam; and it is interesting to note that one or two boilers of this type have already been designed and patented.

Fuel Supply.

The importance of the fact that fuel represents 50 per cent. of the total works cost in the generation of electricity, and that from 10 to 20 per cent. of this outlay can be saved, is often ignored by engineers, when contemplating the low price at which they have placed their fuel contracts. It is held that because fuel is cheap,

therefore it may be wasted with impunity; and much of the inefficiency to be found in the boiler-houses of electricity works is to be traced to this fallacy.

But a 10 per cent. saving on the total works costs is worth effecting even when these costs are exceptionally low; and systematic sampling and testing of the fuel would in many cases be found to save money. The supplies of fuel whether cheap or dear, ought therefore to be subjected to regular sampling and testing. A check upon the colliery firm or coal merchant supplying the fuel is afforded by the laboratory examination, and this is useful not only in maintaining regularity in the quality of the fuel supplied, but also in affording a basis for the new contracts which are from time to time made. The recent letter of Mr. C. E. C. Shawfield, of Wolverhampton, which appeared in the *Electrician* of October 7, 1904, is proof that at last the truth of these statements is being recognised; and the only criticism the authors have to make on this letter is, that Mr. Shawfield does not appear to recognise the need of employing specially trained men for sampling and testing work.

Sampling.—The sampling of fuel requires care if the sample is to be representative of the bulk supplies, and much of the distrust in engineers' minds relative to the value of laboratory examination of fuels, has been due to lack of expert control of the sampling operation. A copy of sampling rules is therefore included in the Appendix of this paper, and it is recommended strictly to adhere to these when sampling fuel for analysis.

It may be considered a striking fact, but it is none the less true, that a sample of only one pound in weight, when properly taken, can be truly representative of fifty or a hundred tons of coal; and from such a one-pound sample a trained chemist can, by further grinding and reductions, obtain a sample of two grammes or less which shall be equally representative of the original bulk of fuel.

Whole shiploads of copper and iron pyrites containing several hundreds of tons are regularly sold on the results of the analyst's tests made with only half a gramme of ore, without any disputes occurring between buyer and seller, and it is certainly not impossible to obtain equally accurate sampling and testing in the case of fuel. But, as already said, scientific knowledge and control are required in the sampling operation, and lacking this no fuel test is worth the paper upon which it is written. The idea should therefore be given up that a few shovels of coal taken from any part of the fuel heap and placed in a box constitute a "*sample*," and some personal time and attention should be devoted to the rudimentary principles of sampling and to the training of men specially for this work.

A regular and constant check upon the fuel supplies can then be obtained, by having daily samples of the fuel taken. These samples can either be tested separately, or an average can be obtained at weekly intervals, by mixing all the daily samples, and reducing these to smaller bulk in the manner described in the Sampling Rules. When a coal-conveying plant is installed, an automatic sampling arrangement should be added to it.

Testing.—The testing of fuel for technical purposes should cover moisture, ash, coke, volatile matter and calorific value; the last being calculated from the approximate analysis and also ascertained by burning from ½ to 2 grammes of the fuel in one of the various forms of fuel calorimeters. Men without training in chemical analysis should not undertake this kind of work. Fuel calorimetry is difficult, and even trained chemists require practice and experience in it before trustworthy results can be obtained.

Placing Fuel Contracts.—It is often found that the fuel which yields the best laboratory test results is not that which produces the most steam per lb. of fuel when burnt under the boilers; and a tendency is manifest to make use of this fact as an argument for dispensing altogether with fuel analysis and calorimetric tests, when placing fuel contracts. This view is a superficial one, and is wanting in a true appreciation of the factors which govern the combustion of fuel under the boilers.

Every type of boiler-surface is naturally best fitted for burning a specific kind of fuel, and by empirical trials this fuel may of course be found. When fresh contracts are to be made two courses are open: either to obtain a fuel as nearly as possible equal in physical and thermal properties to that last contracted for—or, to obtain a different fuel and adapt furnaces and draught to the new fuel. Now the chemical and calorimetric examination is of the greatest value in either case. It simplifies the search for a fuel similar in all characteristics to that last used, since if the percentage of ash, volatile matter, coke, and fixed carbon be determined, the desired fuel can be selected without recourse to the troublesome and expensive steam-raising trials under the boilers. If on the other hand, another type of fuel be selected owing to its much lower price, the laboratory examination will be a guide to the changes required in furnace construction, methods of firing and draught, in order to get smokeless combustion and high efficiencies, when this fuel is burned under the boilers.

Too little attention has certainly been given in the past to this question of adapting the furnace construction and methods of firing to the fuel, and whatever boiler-makers chose to recommend has been much too readily accepted. Most of the electricity works where high-priced fuel is now being used, because it is asserted that steam cannot be kept up or that smoke is produced when burning cheaper fuel, are simply playing into the hands of the South Wales Colliery Companies.

The authors assert that with suitable furnace construction and scientific control, bituminous fuels can be burnt without causing smoke troubles and with high efficiency under every type of boiler, and that great savings in fuel costs could be effected in this direction. But the furnace construction must be adapted to the fuel, and it is absurd to suppose that furnaces designed for burning anthracite or semi-anthracite fuel, will satisfactorily burn fuels containing 30 per cent. or more of volatile matter. The percentage of volatile matter which a fuel gives off when heated is in fact a measure of the size of the chamber required for its complete combustion, and it is here that the

laboratory examination of fuel yields results of the greatest value. That such an examination is still regarded as unnecessary and of no help in the placing of fuel contracts, is clear proof of how much has still to be learned in the scientific control of steam plant.

AIR SUPPLY.

The advantages to be gained by a preheated air supply are very great, and it is surprising that hitherto so little attention has been given to this subject. In 1881 Hoadley, in the United States, published details of some unfavourable experimental trials relating to the use of preheated air, and possibly these unsuccessful results may have deterred others from experimenting along similar lines. But the gain in economy and efficiency by raising the combustion temperature inside the furnaces by the use of heated air is great, especially if heat otherwise wasted is applied to heat the incoming air.

The ideal system would be to reduce the exit flue gases to atmospheric temperature by means of economisers, air-heaters, and preliminary feed-water heaters—chimney-draught being dispensed with and the draught produced by fans. The preliminary feed-water heaters would be placed last in such a system, and the whole of the remaining heat would be removed from the exit gases by spraying the water through them, or by use of a tower filled with tiles or flints.

Little or no unconsumed carbon would be produced under the conditions obtaining with a very high temperature in the combustion chamber of the furnaces, and the sulphur dioxide and CO_2 absorbed by the feed-water would be neutralised in the water-softening operation. It must be remembered here that the sulphur would finally appear in the boiler as sodium sulphate, but the authors suggest that it (the sulphur) might be recovered in some way, and made a source of revenue to the feed-water purification plant. The air-heater would be of the type used in the regenerative gas furnace—lengthened contact with a considerable surface of heated brickwork being necessary to heat air, which is a poor conductor of heat and demands time for its temperature to be raised.

As regards the thermal advantages of heating the air used in the combustion process, it may be pointed out that from 20 to 25 lbs. of air are usually required per lb. of fuel burned, and if this air could be raised to 300° F. before entering the furnace, the final temperature after combustion would be increased practically by a like amount. A gain of 300° F. in the furnace temperature is not to be despised.

According to Stromeyer's experiments * the usually accepted formula for the relation between the rate of heat transmission (a) and temperature (t) is incorrect; and in place of the former varying as the square of the temperature difference, or $a \backsim (t_2 - t_1)^2$, the ratio is expressed by the formula, $a \backsim (t_2 - t_1)$. Even though the ratio be not as the square of the temperature difference, the formula shows that a much greater efficiency will be obtained from a high than from a low initial furnace temperature.

* *Memorandum to the Manchester Steam-users' Association*, 1902.

The most important causes of low initial furnace temperature are, excessive air supply to the furnaces, and too sudden contact of the half-burnt gases, with the water-cooled tubes or plates. Larger combustion chambers and refractory furnace linings are the proper remedy for the latter evil, and gas-testing is the check and remedy for the former.

Furnace temperatures might, in many plants, be raised 500° or 1,000° F. by attention to these two points alone, but even then the limit of what is attainable in this direction would not be reached. By the use of a heated air-supply in conjunction with closed-in ash-pit and induced or forced draught, much higher temperatures can be attained in the boiler-furnace; and should Rowan's suggestion* for working such furnaces under two or three atmospheres pressure be adopted, further increases of 715° F. and 493° F. could be attained. The possibility of obtaining far higher evaporative efficiencies from boiler plants, than any yet attained, by intensifying the combustion along these lines, is therefore strikingly manifest, and it is surprising that hitherto so few attempts have yet been made to put these suggestions to a practical trial in new plants or extensions.

The authors have, however, recently inspected one plant where mechanical draft, closed ash-pits, and a heated air-supply have been installed. This station has recently been designed and erected under the charge of Mr. S. E. Fedden, for the Sheffield (England) Corporation at Neepsend, and is at present equipped with 2 × 1,500 k.w. turbo-generators of the Parsons type. It has not yet commenced full work; but excellent results are said to have been obtained with the same system of induced draft and heated air-supply at the older generating station of the Sheffield Corporation, and it is believed that these results will be surpassed in the new station. A full description of this plant has already been published.†

The limit of improved efficiency possible in this direction is that imposed by the ability of the steel used for boiler construction to withstand very high temperatures under steam pressures of 150 or 200 lbs.; but no doubt the resisting qualities of steel plates in this respect could be greatly improved, and the more general use of refractory linings to the boiler-furnaces would also assist in meeting this difficulty.

Boilers and Furnaces.

The choice of a boiler will depend on the circumstances of each particular case.

There are three general types of boilers :—

(*a*) The large shell type such as the Lancashire and Marine.

(*b*) The large water-tube.

(*c*) The small water-tube.

For long-continued steady work such as electric tramway work the type (*a*) is good. For lighting work and large installations type (*b*) is good. Type (*c*) is suitable for the peaks of loads of short duration, and can be heavily forced without danger.

* *Cassier's Magazine*, vol. 25, p. 131.

† *Electrical Review*, vol. 55, p. 99.

In every case there are certain stand-by losses to be reduced by attention to air-tight brickwork and heat insulation. The rate of loss can be found by closing the dampers and observing the rate of pressure or temperature reduction of a standing boiler. The losses thus observed may be greater than those from a boiler at work, for they include air-leakage loss, which may be less with a working boiler than with one at rest. That a boiler contains much heat in a large mass of water is no reason why its stand-by loss should be great. Small stand-by loss is rather a question of external heat radiation and of convection by air currents. Hence the importance of tight dampers. The type of boiler to be employed is less a matter of heat storage than of rapidity of steaming from colder conditions. Once the temperature of full pressure has been attained, the boiler containing much water will answer as rapidly as the flash boiler to an increased urgency of fire. The conditions of an electric light station may be held to point to the use of light water-tube boilers of the express type fired by liquid fuel. This policy will be the more suitable as the load peak becomes of greater height and shorter duration. The expense of fuel will be counterbalanced by the staff reduction which a purely mechanical plant renders possible.

The economy to be secured by employing cheap and uneconomical engines for getting over brief peaks of the order of those in a lighting station must not be allowed too great weight in view of the fact that a wasteful engine makes a greater demand on the boilers, and too big a saving in engine cost will demand too great an expense in extra boiler power or too large an increase of fuel combustion if the boilers are unduly forced to supply the extra steam. But it may well form a point to be considered whether a boiler that can be heavily forced for short periods is not most suitable for high peak loads. On this point engineers should distinguish between an installation for home and for abroad. Abroad in many places, one need often only count upon forcing boilers for the few minutes either side of the maximum peak. The same forcing cannot be done for hours at a time, as might be required any winter in a big English city during a fog which may last a week. It behoves the designer therefore to proportion boiler plant cautiously for lighting work, and somewhat more boldly for mere power work. But he should certainly consider the possibilities of superior fuel used for short periods only.

Where coal is the fuel the provision of a second supply will present difficulties that may be more serious than the provision of more boilers; but with a system of liquid fuel, the conveyance of the fuel to the furnaces is a simple matter, and it may well pay to employ expensive liquid fuel during heavy loads of short duration, if capital outlay is thereby reduced together with rent and attendance.

This seems for Great Britain at least the proper use of liquid fuel, and this view is forced on us by a consideration of the relatively small output of liquid as compared with solid fuel. Any more general employment of liquid fuels would deplete the market and raise prices.

The Lancashire Boiler.—This type of boiler has the advantage of larger water contents, and will respond to a sudden demand for more

steam with less loss of pressure than is possible with boilers containing less water. In every type of boiler the water is at the temperature of the steam, and only awaits the addition of its latent heat to become steam. Obviously, therefore, the urging of the fire to greater activity will promote an increased production of steam proportionate to the increase of heat units generated by the fire, no matter what the weight of water in the boilers.

The Lancashire or Shell type of boiler has the advantage of small heat-radiating surfaces, especially where several boilers are set in a battery. With most water-tube boilers there are at least two side walls to every two boilers. There are only two side walls to any number of

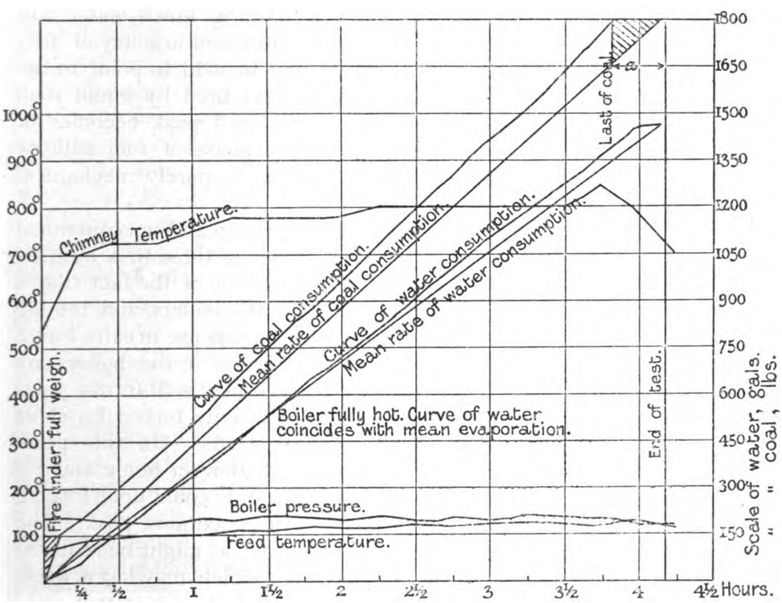


FIG. I.—Boiler-test Curves showing heat storage in brick casing of Solignac Boiler.

Lancashire boilers, and heat radiation is confined to the front end and top. The chief disadvantage of the Lancashire type of boiler is the furnace. It is narrow in width, has a low crown of water-cooled plate inimical to good combustion, and it cannot be lined with firebrick without considerably reducing its dimensions, nor so far do we know of any very successful lining having been done in this country except in dust firing.

The Shell Boiler has a large water surface, and gives off steam quietly and fairly dry. It costs very little to maintain, and, being certain and reliable, can be employed with a less proportion of spares than other boilers. In large installations the advantage of larger water contents is reduced because the steam output of a dozen or twenty water-tube

boilers may average out to a steady curve. But for small stations the shell boiler offers better prospect of steady pressure.

The Water Tube Boiler.—Though this boiler does not possess the steady steaming qualities of the larger shell boiler, this fault decreases as the number of boilers increases. It is more apt to prime, and it has greater liability of stand-by losses and larger radiation losses and maintenance. Being externally fired, it possesses great furnace potentialities which are easily secured. The water-tube boiler can take full advantage of the external furnace provided that the process of combustion is allowed to be carried out in conformity with the known facts of physics and chemistry.

The Small Tube Boiler.—This boiler is merely another step forward in the process of splitting up the heating surface and reducing water contents. Carried to still greater lengths, it produces the flash boiler.

As the water contents are small, steam can be got up quickly from cold water, but even here, as shown on Fig. 1, considerable heat is absorbed by the brick lining of the casing, so that the full output of steam does not begin until the boiler has been some time steaming. Hence the bend at the beginning and end of the coal and water lines in the diagram, showing heat absorbed and given off by the brickwork. These small boilers appear to offer themselves for supplying emergency steam. Fired by liquid fuel, some of these boilers, such as the Solignac, could instantly be put in full work if filled up with fully heated feed-water from a thermal store tank. The full fire might be started almost as soon as the water had been turned into the empty boiler.

FURNACES.

The economy possible from many appliances lies entirely in the possibility of using a cheaper fuel. Unscientifically designed furnaces are unfortunately of lowest first cost. There are certain essentials to the perfect combustion of bituminous coal which, if wanting, render necessary more expensive Welsh coal. The three essentials for the proper use of bituminous coal are a furnace so arranged that the gases given off the green fuel and air to burn those gases shall travel over the length of the fire together, under a draught velocity of not less than 30 feet per second. The mixed gases must have a free unencumbered space beyond the furnace in which to complete combustion, and this combustion will be complete if the third essential of temperature be present.

In shell boilers of the Lancashire type without cross tubes, with roomy brick flues and a 200-ft. chimney, a careful hand stoker will burn bituminous fuel without smoke. But this boiler has a water-cooled furnace, and is at just about the critical point as regards smoke production. With a less skilled man or a less active draught smoke will be produced. The presence of water cross-pipes beyond the bridge will render smoke still more easy to produce. Observations of some thousands of boilers, with and without cross-pipes, have demonstrated to the authors the great influence of this detail on smoke production.

The ordinary furnace of the usual types of water-tube boiler is

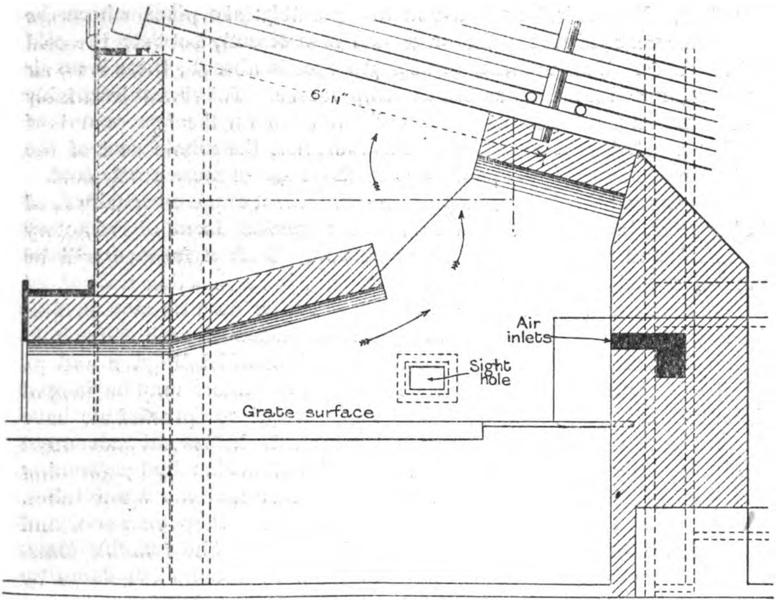


FIG. 2.—Refractory Furnace of Babcock Boiler (Kensington and Notting Hill).

Form—Correct. Environment—Good.

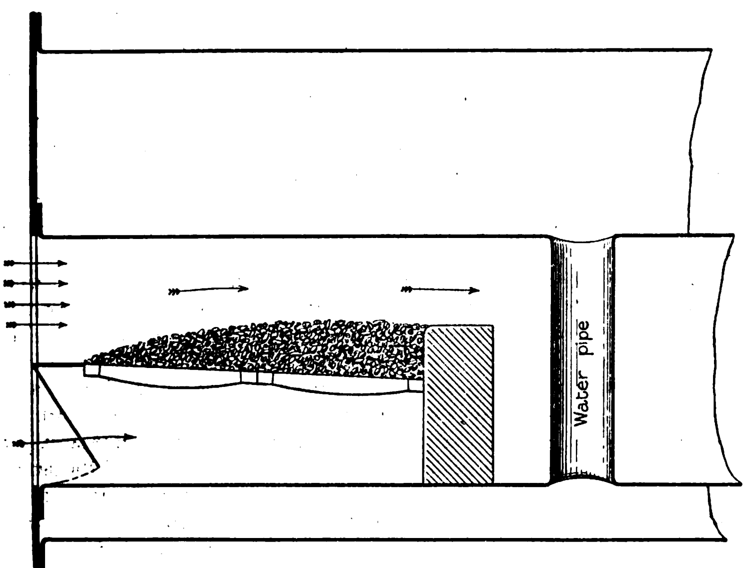


FIG. 3.—Ordinary Furnace of Lancashire Boiler.

Form—Correct. Environment—Bad.

hopeless. No perfect combustion can possibly take place where the gases rise vertically from the grate and pass directly between the cold tubes above. The essential sweeping effect is absent; there is no air mixture, and there is no sufficient temperature. Yet almost invariably this common and inefficient setting is accepted for the few pounds of saving effected in first cost, with the result that the whole cost of the boiler in a year or two is paid away in the shape of more costly coal.

The water-tube boiler, in common with most or all externally fired boilers, can be supplied with a furnace of perfect form, of refractory material, and with suitable air admission. Such a furnace will be smokeless and economical. The water-tube boiler has thus undoubted potential advantages over the shell boiler which has a furnace, perfect it may be in form, but covered with a water-cooled arch difficult to line with refractory temperature-conserving material (Figs. 2 and 3). That air mixture and time are recognised to be factors may be judged from the fact that, in the Belleville boiler, jets of compressed air have been blown into the furnace to produce rapid horizontal movement and mixture of the gases, and to compel combustion by lengthening the path of the gases between the fire surface and the water tubes. The effect of the air-jets is to diminish smoke, but they are a poor and uncertain substitute for proper form of furnace. The familiar class-room experiment of extinguishing a candle by cooling the flame by means of the heat-radiating power of a cone of wire, helps one to realise the conditions under which bituminous coal is ordinarily burned.

As between Welsh coal and bituminous, the thermal units of the latter per unit of cost are usually superior to those of the former, and it is easy to understand why large power-stations prefer to pay an occasional £100 in smoke fines rather than to abandon one coal for another of twice the cost, though it is less easy to understand why correctly formed furnaces are not employed. In existing stations, with floor lines fixed, basements occupied, and pipes difficult to alter, the change to good furnace conditions may involve considerable difficulty, but, in laying out new plant, it should be possible to afford the necessary head-room to give good furnace arrangements. The appearance of ninety-nine out of any hundred boiler-houses tends to the impression that the designer has acted on *laissez-faire* principles, or has been denied the small additional cost that would be added to the lowest tender which, irrespective of future trouble and expense, rules his employers' judgment.

In running a station, coal is one of the principal items of cost, and the ability to employ cheap coal justifies a reasonable expenditure on furnaces to enable such cheap coal to be used. Only, however, by suitable furnaces can the minimum of cost be secured, and the great economy usual as between Welsh and bituminous coal, is only to be had where furnace forms, draught, and lining are correct. Possible only with care in internally fired boilers, this correctness is easy to obtain with externally fired boilers, but is rarely attempted. Smoke is looked upon as inevitable, and the engineer may make all the smoke he wants if he can show that he has used ordinary plant.

Heating Surface Efficiency.—Particularly in water-tube boilers, provision must be made to ensure that all heating surface is effective. Gases from the furnace will flow by the path of least resistance to the chimney, and while this path must not be unduly baffled, the area of the path must be sufficiently small to compel the travelling gas to search out every part of the heating surface.

It is perhaps easier to apply suitable tile baffles where the path of the gases is parallel with the tubes than when it is across them. Since the gases shrink in volume as they traverse the heating surface, the cross-sectional area of this space should be progressively reduced if necessary, to prevent short-circuiting. Where final temperature is high after passing a reasonable area of heating surface, the explanation may be sought partially in this direction.

Radiation and Air Leakage Loss.—Loss of heat by radiation has long been recognised, and it will suffice here merely to indicate its reduction to a minimum by careful covering.

Loss by infiltration of air has only lately been generally recognised as a serious matter. It was possibly first pointed out as serious by one of the authors over twenty years ago, and was ridiculed as hair-splitting. It is serious as a direct loss, as a reducer of temperature head in the external flues and in the economiser, and as a reducer of draught by cooling and choking the chimney. Glazed brickwork—recommended by the authors many years ago—might often be better employed to case in boilers than to decorate engine-rooms. In the absence of glazed brick, all boiler-casing walls should be of hard pressed brick in cement, or the walls should be thickly painted until air-proof, or covered in thin sheet metal all the way to the chimney. Heat insulation of pipes depends for its effect upon the entrapping of films of air, by some poor-conducting cellular or laminated material. Animal wool is probably the best heat insulator, could it withstand modern temperatures. Of the many coverings sold, any maker can produce figures of tests which prove his particular material to be the best, if gauged per unit of thickness, or by price, or upon some other basis. All of them are better than bare pipes. Pipe flanges must be afforded equal protection with pipe bodies, and heat insulation is of sufficient importance to warrant careful attention.

Stoking.—Few men can properly stoke a furnace so long as 6 feet. The chief thing to aim at in stoking is even thickness, careful filling of hollow places, and complete covering of the bars, often *so neglected at the front corners near the door.* More steam can usually be made on the spreading than on the coking system; but in the spreading system either one side only should be fired at once, or one half of the length, so that the excess of air through half the grate may mix hot with the gases from the other half. Bituminous coal does not burn, except as a gas, until about a third of its weight has been gasified. When stoked, it chills the fire much more than would happen with so many cold stones; and the remarkable deadening of the fire is not due to this slight cause, for only about 11,000 heat units are employed in raising a charge of ¼ cwt. to 1,000° F. of temperature, or say ⅔ lb. of coal. The cooling is due chiefly to absorption of latent heat by the production of

gas. A piece of coal is obviously carbon plus some solid variety or varieties of hydrocarbon unknown to the chemist, with a mean proximate empirical formula C_7H_5. Chemists only know the various heat distillates, and, as in all operations involving change of state from solid to liquid, and liquid to gaseous, the change of solid coal into gaseous compounds demands heat. In burning bituminous coal on a grate, much of the calorific capacity of the fuel is absorbed as latent heat by the newly distilled gas, which renders it up again when burned later. Coke or anthracite, producing little or no gas, burns at or near the grate, and produces a clear bright fire. Bituminous coal cannot be thus burned. It must have time, and space, and temperature, and the least time and space are demanded when the temperature is highest.

Mechanical stoking cannot abolish these facts, nor can it possibly be a panacea for the smoke evil. Mechanical stoking does permit the use of inferior or rather smaller coal, and it tends to render more uniform the régime of the furnace. It does not render grate cleaning unnecessary, and it is not, therefore, a perfectly continuous process with the ordinary moving bars. In a coking stoker which feeds fuel to one end of a grate, the action as regards smoke production and burning is perfect, for the coked fuel tumbles into a pit behind the grate, and burns out there as in a gas producer. The grate is short, and the coal never burns very thin upon the bars in such a stoker as the Vicars—Fig. 4—which aims to keep down air excess by this means. In the chain grate without a back pit, the fuel burns out on the grate, and large volumes of excess air get in at the back end. This renders economy impossible, unless means are taken to regulate the admission of air to different sections of the grate in accordance with the demands of the fuel. Air admission above the fire is regulated by grids in the furnace door. The opening for air may be 4 square inches per square foot of grate. Such grids are regulated by hand, or by clockwork or similar devices. In mechanical stoking the air-supply gets in more or less incidentally through the fuel hopper, and through casual openings, and often in far too large amounts at the rear of the grate.

Mechanical stoking does not eliminate the need for correct furnace forms, or for sufficient temperature and proper mixture of air and gas. All inventors and purveyors of furnace accessories, such as grate bars, air admission devices, stokers, etc., appear to become possessed of one idea, and this they push for every case, overlooking the fact that the combustion of bituminous coal is a complex chemical process which can only be conducted where the chemical equivalents are present to combine, and the further essentials of combustion is ensured. Theoretically, perhaps little air beyond the chemical equivalent would be required, were the furnace continued to so great a length that ultimately every molecule of combustible came into contact with its molecule of oxygen. This assumes a heat-conserving furnace.

Practical considerations render it necessary to reduce the extent of the furnace, and to add an excess of air so as to ensure oxygen coming into contact with all the combustible elements of the fuel within the zone of sufficient temperature. It should be possible to do this with 25 to 33 per cent. of air in excess, and certainly with less than 50 per cent. excess.

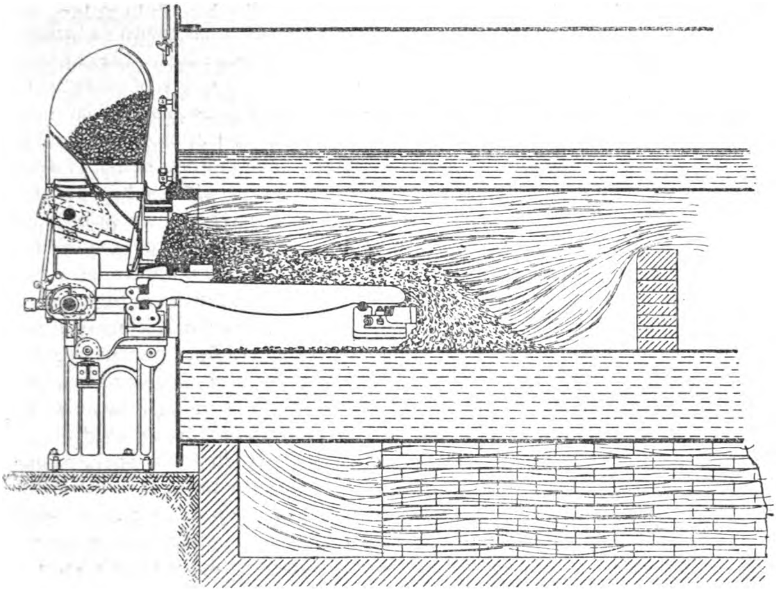


FIG. 4.—Vicars Stoker, with short grate and gas producer pit.

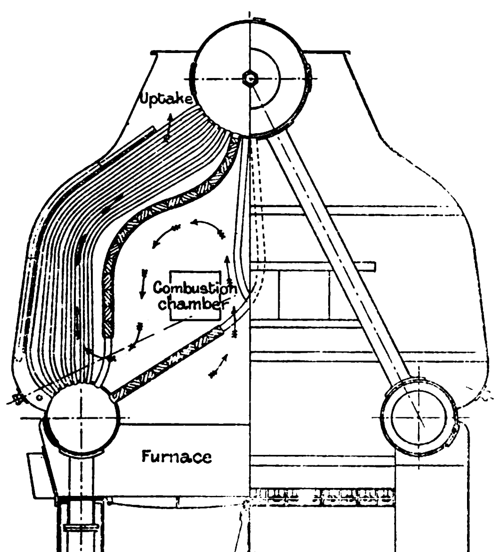


FIG. 5.—Weir Water-tube Boiler, with refractory furnace and combustion chamber.

The flame of bituminous coal will be longer, as the heat is kept low, so that a suitable furnace has a cumulative effect. By conserving temperature flame is shortened, and by shortening flame the necessity for long-protected furnace surfaces is reduced. In Fig. 2 is a furnace which has been shown effectually to burn bituminous coal without smoke; but, so far as the authors are aware, this furnace has only been used with ordinary chain grates, and there is no ordinary chain grate that does not pass an unnecessary and grossly excessive volume of air. Until this furnace is tried with a less wasteful form of grate, no absolute opinion can be passed upon its proportions, though its principles are correct.

The manufacture of a maximum of carbonic acid gas per unit of fuel consumed, or rather per unit of air used, is what should be the aim of the stoker. High CO_2 means high furnace temperature, high heat transmission, small weight of waste gases, and general furnace economy. *Per se,* black smoke may not indicate serious waste of fuel; but it is evidence that the boiler surfaces are covered with non-conducting soot, and black smoke is an incentive to buy more costly coal of a less smoky order.

The arrangement of Fig. 2 is smokeless under the conditions named.

Fig. 5, which shows the Weir water-tube boiler, is an example of the same principle, scarcely different in the way it is carried out. Coal is burned in the Λ-shaped furnace, the walls on all four sides of which are of fire brick, and combustion is continued in a similarly protected chamber, and is perfect with ordinary hand-firing before the heated gases pass among the tubes.

Fig. 6 shows the locomotive furnace similarly arranged to secure correct form.

The chain grate stoker is perfect as a self-cleaning contrivance, but admits excessive air. Hence the need of choking boxes, as employed by Mr. E. B. Coxe, Fig. 7.

These difficulties with travelling grates are responsible for the sprinkling form of stoker, which, when in good order, keeps the grate evenly covered, as the coal tends to fill the hollow places. But the winnowing action of the draught carries off the dust to become a public nuisance. Such stokers require initially providing for, in the design of a chimney with a settling chamber to gather the dust.

The economy of mechanical stokers lies in their ability to raise sufficient steam with cheaper coal than hand-firing demands, and in the reduction of the stoking staff. But mechanical stokers will not be an economy unless their hoppers are filled mechanically, and they ought to be in charge of a skilled mechanic to look to upkeep and maintenance, and not be expected to run with unskilled attention.

The ideal mechanical stoker is of the inclined grate type, Fig. 8, in which the inevitable thinning of the fire of the coking furnace is made good by the aid of a moving grate assisted by gravity. In this type, only possible in its full extent with external furnaces, the fuel is helped to fill gaps and close the lower end of the fire by gravity, and at the foot of the inclined grate is some arrangement which enables ash and dirt to be removed.

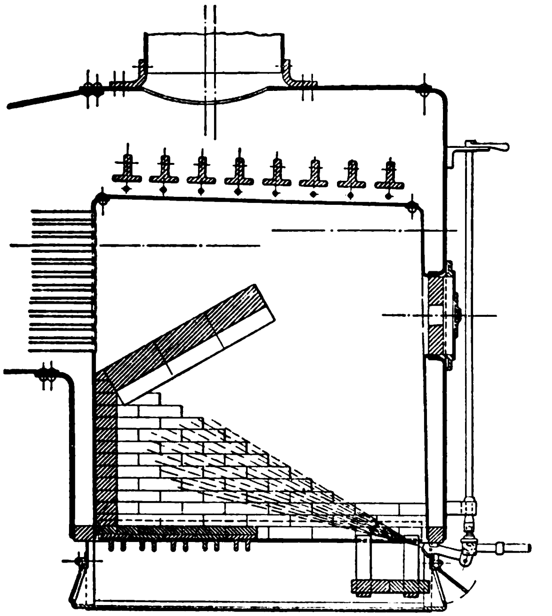


FIG. 6.—Locomotive Boiler, brick-lined for liquid fuel.

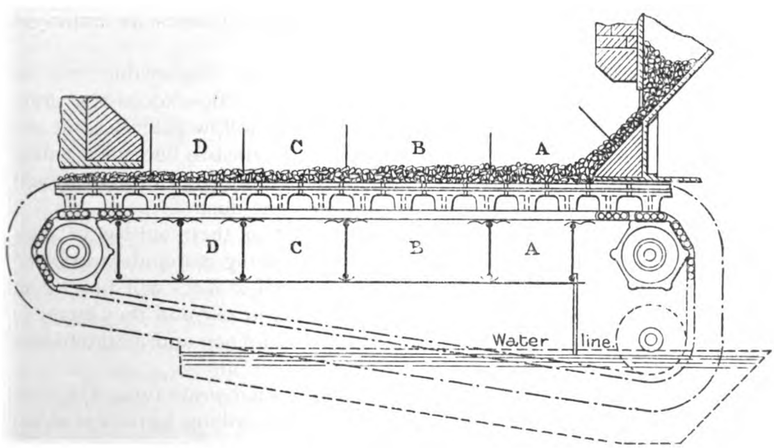


FIG. 7.—Chain Grate Stoker, with air-regulating chambers below.

Rocking grates which profess to cut out the ash and clinker from the under side of the fuel bed are of doubtful use, for clinker is tough and adhesive when hot. It becomes brittle only when chilled, and users of such bars should be prepared at least to have each half of the grate workable independently. The system may then be used to clean a grate quickly, after the clinker has been chilled by removal of the fire to the other half of the grate.

Smoke Prevention.—This can often be managed by excessive air admission, for the reason probably that though air in excess has a chilling effect, its ample provision enables all the gases to burn before they have lost much temperature. But smoke cannot be prevented with economy unless sound conditions are present as already detailed and the crux of the whole matter is summed up in saying that a maximum percentage of CO_2 must be present in the furnace gases.

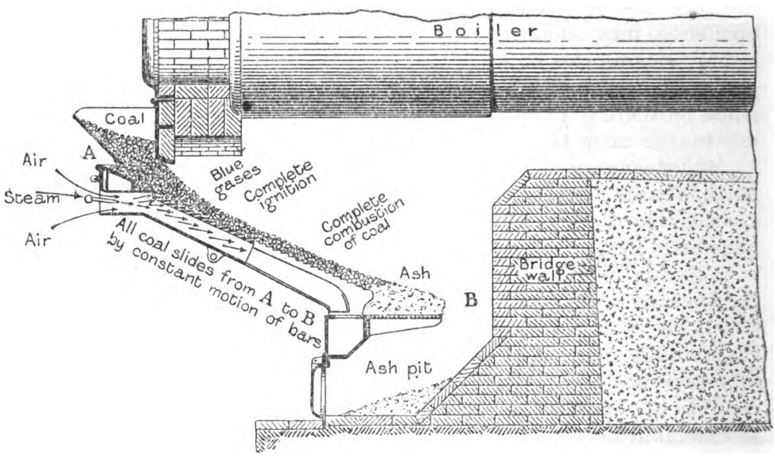


FIG. 8.—Inclined Grate, with shaking and gravity effects.

Could the furnace products be rejected at atmospheric temperature, an excess of air would be of no account. But the heating surfaces commercially available render it impossible to cool rejected gases below, say, 400° F. whatever the supply of air, and obviously the loss of heat is greater where air is used in excess. This point is emphasised because there is frequently a serious confusion of thought in relation to heat and temperature, and the weight of the gases possessed of such heat or temperature. Cold flue gases, unless of small weight per unit of fuel, do not represent economy, but excess of air. Some people think otherwise.

STEAM.

The object of burning fuel under steam boilers is to produce steam. Steam at atmospheric pressure contains about 967 units of latent heat and 160 units of sensible heat above feed temperature. Only by

means of the latent heat can work be done. These facts make it of prime importance that when steam is used it should contain all possible latent heat. It should be steam, in fact, and not hot water.

Steam loses heat from the moment it leaves the boiler. The surfaces which contain it must therefore be of minimum area. This involves pipes of minimum length and is one of the various reasons for avoiding ring mains, the fancied need for which is obviated by using good materials well put together for steam-piping. Short pipes make it possible to carry a given weight of steam with less drop of pressure, or otherwise to carry the same steam at the same pressure fall through smaller pipes.

The loss of heat through pipes varies with the head of temperature. The weight of steam that will flow through a pipe varies with the pressure absolute very nearly. The pressure of steam rises faster than the temperature, and high-pressure steam (realised to be economical) will lose less heat than low-pressure steam, if the pipes are as much smaller as they should be for the high pressure. It is a mistake to employ steam-pipes so large that there shall be no appreciable fall of pressure. Some fall, 3 to 5 lbs., per square inch may be allowed in long pipes, for there is a point where the saving due to high pressure will be lost by the extra radiation from the larger pipes.

Wet steam travels with more friction than dry steam. Superheated steam, despite its greater head of temperature, loses heat less rapidly by contact than does wet steam; it flows with less friction and loses less at joints. Less of it is also required, and pipes may be smaller.

There is no thermo-dynamic gain from superheat. Steam leaves the boiler at boiler temperature; it starts expanding at boiler temperature. The work done on the piston during admission comes directly from the fire, or is done by expansion of saturated steam in the boiler. The superheater is merely an incident in the steam pipe. The sole function of superheat is to ensure that steam, saturated with heat, and at maximum density per cubic foot for its pressure, shall be present in the cylinder to work expansively when steam is cut off from the boiler. Superheat is thus intended to heat the cylinder as hot as the boiler, and, by avoiding the presence of water in the cylinder, to minimise the exchanges of heat between the cylinder metal and the steam. Superheated steam supplies heat directly to the cooled surfaces which require it. The jacket, on the other hand, endeavours to maintain the outside of the working liner of the cylinder as hot as the boiler, and, while it may do some good in certain cases, it is always liable to do little else than evaporate water of condensation during the exhaust period.

The steam engine being a heat engine, it is obvious that the best working fluid is that which contains a maximum of heat in a unit of volume. This is what saturated steam does contain, and the object of superheat is to supply saturated steam to fill the cylinder at the time of cut off.

Superheated steam is still often erroneously claimed to owe its superiority to the fact that less of it will fill a given space at a given pressure than is the case with saturated steam. This argument ignores the rapid drop of pressure which occurs when superheated steam

expands. It ignores the fact that in a given bulk there is less heat and therefore less capacity for doing work. Therefore, while superheat is essential to economy, it is so only because it is the one means we possess of filling a cylinder with dry saturated steam.

To produce superheated steam has proved more difficult than might be anticipated, owing to the variable conditions of the furnace. To prevent superheater tubes burning away, steam is often made to rush through them at a high frictional velocity, whereby, its pressure being reduced 12, 15 and even 25 lbs., much of the gain of superheating is sacrificed. In separately fired superheaters no tube will stand a temperature above, say, 1,400° F. Few will stand more than 1,000° to 1,200° F. But a furnace worked with any regard for fuel economy has a temperature of waste gases of 2,500° or 3,000° F. All separately fired superheaters that do not possess a system of water control and temperature reduction, are made safe against the high furnace temperature by an enormous inlet of cold air through the grate or door, or through special air openings. As the superheater must inevitably reject its gases at, say, 700° F., the surplus air is all rejected at that high temperature, and a large proportion of the advantage of superheat is lost. The separately fired superheater should abstract the surplus furnace heat in a rational manner, and this is best done by means of a shield of water between the furnace and the superheater. The advantage of the water protection shield in advance of the superheater tubes is that the feed-water for all the boilers of the set may be passed through the tubes and raised to the boiler temperature. The advantages of thermal storage are thus secured, for these are chiefly confined to the high rate of evaporation at which a boiler can be forced when fed with fully heated feed-water. Not only is priming reduced or prevented, but the whole working of the boiler is rendered more efficient.

To secure superheat economically, it is first necessary that the steam way through the superheater tubes should have an area equivalent to 25 to 50 per cent in excess of the boiler steam-pipe. The ordinary small tube superheater, if given so much equivalent area, would not then cause the serious loss of pressure it now causes, but it would burn away more rapidly than it now does. In Germany, where, probably, small tube superheaters have a larger allowance of area, this burning out does occur rapidly, but it is said to pay to burn out tubes rather than to sacrifice so much pressure. There is no need to do either where water control of superheat is adopted, as proved by six years of steady work without deterioration. But a first-class water controlled superheater, though it may last indefinitely, will cost double or treble as much as an ordinary uncontrolled superheater. In return for this, it will supply feed through the water control pipes at full boiler temperature, thus increasing the efficiency of the boiler considerably as well as the output.

In the manufacture of steam fit to be used in an engine, economy demands its production in distinct stages. The water, duly purified, is first raised to the temperature of condensation by the exhaust steam. This may be, say, 100° F. It is next raised to a temperature of, say, 250°, 300° or upwards, by means of the residual heat of the waste gases.

If still below boiler temperature, the feed-water may be raised to that temperature by the water control apparatus of the superheater, and, in separately fired superheaters, by the water screen which protects the superheater pipes from too direct fire action. The boiler then comes into action and adds heat to the feed-water. All this heat ought to become latent. None of it ought to go towards raising feed temperature. Indeed, in the absence of the last-named stage of feed heating, a live steam heater should form this stage.

The boiler should produce steam which contains the maximum possible amount of heat per cubic foot, and is capable of performing work in the smallest possible cylinder. Unfortunately for economy, cylinders are made of material which exchanges heat with the steam, particularly if wet, and, despite all precautions, steam enters the cylinder loaded with water and cannot lose heat without liquefaction taking place. If superheated, its water contents are evaporated and its volume is increased. It can travel faster because its molecular kinetic energy is greater. It loses nothing at drains, and though it loses temperature it does not become wet so long as any superheat continues. It furnishes heat to the interior walls of the cylinder, and reduces those exchanges of heat which increase steam consumption by 20 to 50 per cent. Little or no superheat need remain at the point of cut-off, and there is then contained in the cylinder the greatest amount of heat possible at the given pressure. For the first cylinder of a compound engine steam should be superheated. The superheating of the steam between the cylinders is a matter of dispute. It ought to be economical, and, if it fails to be so, the cause is perhaps connected with the absence of drainage of the receiver. Engineers who have intermediate receivers capable of reheating, would perform a service if they would make commercial tests under the two conditions during a series of alternate weeks.

Superheating should therefore be considered as one of the final stages of dry steam production, the actual final stage being the cooling of the steam to saturation temperature, by means of the heat absorptive effect of the cylinder walls. Superheating is necessary to economy, because cylinders are not adiabatic. With superheated steam, cylinders are nearer to adiabatic conditions.

The economy from a given expenditure upon superheaters is to be found by suitably equating first cost charges, cost of stoppages to renew tubes, and maintenance generally with the economy of fuel that superheat will secure. There are only two classes of superheaters, the cheap apparatus without control, and the apparatus with water control of which class there is only one representative. Comparison would be therefore out of place by the authors.

THE WASTE GASES.

The temperature and chemical composition of the waste gases passing from every large boiler-plant ought to be regularly ascertained; the work being carried out by a man specially trained in technical gas analysis. The absence of smoke is no guarantee that combustion is being carried out in an economical manner, and only by constant

testing of the exit gases can one be certain that the fuel is being burnt without undue excess of air. The significance of air excess becomes manifest when one considers the following figures :—

With gases containing only 4 per cent. of CO_2 and a chimney temperature of 400° F., 4,380 B.Th. units are lost for every pound of fuel burnt, *i.e.*, 32·4 per cent. of the total heat value of the fuel. It may be objected that this percentage of CO_2 is much lower than that present in the flue gases of electricity works; but the authors believe that this percentage is not exceeded in many works, where no special attention has yet been given to this question, and where no CO_2 testing apparatus has yet been installed.

In nearly every case where automatic or other CO_2 testing apparatus has been purchased and used, the authors know that all the earlier tests have been under 5 per cent. of CO_2, proving that under the every-day conditions of work obtaining in these plants, unduly large volumes of air were being admitted.

Where no economiser is installed, the loss of heating power is even greater, for the gases then pass to the chimney at a temperature of from 600° to 700° F., and the heat losses rise to 6,570 and 7,665 B.Th. units, or to between 48 and 56 per cent. of the total heat value of the fuel.

Taking now the case where the boiler plant is under scientific control, and by constant testing the percentage of CO_2 in the exit gases has been raised to 12 per cent., the loss of heat with chimney gases at 400° F. is reduced to 1,540 B.Th. units, or to 11·4 per cent. of the total heat value of the fuel.

In the one case, only between 44 and 68 per cent. of the heat can possibly be utilised; while in the other, 88·5 per cent. of the thermal value of the fuel may be converted into useful work—either in the boilers or in the feed-water apparatus. Engineers would therefore be wise to give most careful attention to this question of excess air supply to the boiler-furnaces, and to make proper provision for the constant sampling and testing of the exit gases.

Having drawn attention to the great importance of this subject, the authors do not propose to describe all the various apparatus and methods to be used in sampling and testing the exit gases. A few general remarks on the subject may, however, prove of value as a guide to practice in this matter.

The examination of the exit gases should cover the following points :—

1. Colour and appearance.
2. Draught and temperature.
3. Chemical composition.

Daily tests should be made for these on each boiler of the plant, and in the main flue, and at the base of the chimney. To facilitate this work, special sampling and observation holes must be constructed at the various required points in the brickwork of flues and boiler settings, by use of 30-inch lengths of wrought-iron pipe, 1¾ inches inside diameter. The pipes should have a flange screwed on the top, and should be set in the brickwork with cement. When not in use for

testing purposes, they must be closed with an iron bolt or with cotton waste. When new boilers are installed, these sampling holes should be left when setting the boiler.

Iron pipes are not to be recommended for sampling flue gases, since such pipes corrode and become choked. Many low tests of CO_2 are, in the authors' opinion, due to the use of iron pipes for sampling purposes. Three-feet lengths of hard potash glass tubing $\frac{3}{8}$ inch in external diameter should be employed. This glass stands a very high temperature without softening or fusing, and does not combine with oxygen at a red heat.

These glass sampling tubes must be removed from the flues when not in use for sampling purposes, and cleaned with a tube brush from adhering dust and soot. Fixed filters for retaining soot and dust are also bad in practice, since, if small, they speedily get stopped up, and, if large, they hold a large volume of gas, and prevent the tested sample from being truly representative. They also increase the dangers of air-leakage, and are to be condemned on that account. Lengthy lines of iron connecting pipes are also not to be recommended. The sample of gas from boiler flues ought in fact to be collected *as near as possible* to the flue itself, and only glass and rubber should be employed for connecting the sampling apparatus to the flue. Many of the CO_2 test results now being obtained are, in fact, of dubious value, owing to the neglect of these elementary precautions in sampling.

With regard to temperature determinations in the flues and at the chimney, these may be made either with a water pyrometer—a high temperature mercury thermometer—or with one of the various forms of electrical resistance pyrometer.

For ascertaining the heat inside the combustion chamber of the furnace, one of the photometric methods should be employed. The theoretical temperature attainable in the furnace by the combustion of coal lies between 3,500° and 3,700° F.; the maximum attained under practical conditions is about 2,800° F. The temperature of the exit gases should not exceed 400° F. when chimney draught is employed; when artificial draught apparatus is installed the exit gases may be reduced to atmospheric temperature, if this is practically possible.

Draught tests can be made with any form of water gauge reading to $\frac{1}{16}$th of an inch. A portable U-tube gauge, that can be carried about, may be employed for testing the draught in individual boilers, but it is well to have a fixed draught gauge at the chimney base and on the main flue. Here again a glass tube is to be preferred for connecting the gauge to the flue. This tube should be removed at short intervals and thoroughly cleaned and washed.

If automatic recording apparatus be installed for taking the CO_2 temperature, or draught tests, glass tubes should be employed whenever possible for the necessary connections to the flues, and all the precautions already named must be taken to keep the tubes clean and free from deposits. The apparatus must be placed in all cases as close as possible to the flues from which the samples of gases are collected; otherwise the volume of stale gases in the connecting pipes and air leakage at the joints will cause error. When water-jet air-

pumps are employed to draw the sample gases through the apparatus, a separate water tank with a filtering attachment should be used for supplying the pump, otherwise stoppage of the apparatus is likely to occur at inconvenient times, by dirt, etc., collecting in the jet of the pumps. The results obtained by all recording instruments require checking occasionally by independent tests with other apparatus.

Draught, etc.

A good draught is only an aid to economy when other conditions are good. Too good a draught must not be employed with a thin fire, for it will cause too large an influx of air. Fuel of a large size will stand a powerful draught, but also demands a thick fire. With a good draught it may occur that the closing of the damper will not only check the rate of combustion of the fuel, but will cause smoke by reducing the indraught of air through the door grids. Boilers are thus best partly regulated by dampers which close the ash-pits, rather than altogether by dampers in the flues. The ash-pit damper properly applied and used will prove an aid to economy.

The production of a chimney draught with a temperature of waste gas, say 300° F. above atmospheric temperature, points to a consumption of heat about one-ninth of the total heat produced in the furnace. This fact points to the minimum air supply, which implies maximum furnace temperature and best efficiency of heating surface. Under normal conditions, however, from 20 to 30 per cent. of the total heat of coal goes up the chimney. It is claimed that a fan draught can be produced with about 1 per cent. of the total engine power, and that the use of a fan must therefore effect a great fuel saving. A fan will not save fuel unless it enables this to be burned more efficiently as above. A fan draught will give economy if, by its aid, a flue feed-water heater can be installed to reduce the excessive temperature of the flue gases. To use full effect feed-heaters, a high and expensive chimney is needed. Fan draught must therefore be considered in the light of enabling an economy in chimney construction to be effected, and full use made of feed-heating apparatus. It is also useful as enabling boilers to be forced to rapid steam production, and presents itself as a safeguard against sudden load in lighting stations and as a means of surmounting the load peak without excessive boiler plant. Fan draught is thus useful as a means of reducing capital expenditure on chimneys and boilers, by promoting the rate of combustion of fuel at higher rates per unit of grate area. It enables the thickness of the fire to be regulated in better accordance with the fuel size and generally promotes elasticity. It is also useful in enabling the hot gases to be compelled to pass over all the heating surface, and baffles may be introduced that would otherwise be perhaps too great a hindrance. Its economy in other cases appears to demand the fullest practicable reduction of the calories in the exhaust gases by feed-heaters, which in large stations may well be in two stages, and perhaps by air-heaters for furnace supply.

When, however, it is attempted to utilise the last remnants of waste

heat let it be remembered that we are, so to speak, working near the lower end of the curve of expansion, and that for each item picked up with one hand we are paying out with the other. All attempted economies, therefore, may cease to be commercial economies before they cease to reduce fuel expenditure per kilowatt.

CONCLUSION.

Finally, may the authors point out that the best results can only be attained if scientific methods of control are employed in relation to feed-water fuel, waste gases, and draught. Automatic apparatus for testing flue gases or any other product can never take the place of the trained chemist; for an automatic apparatus demands not less but more skill in its control and management than ordinary apparatus, if the records obtained by it are to be of permanent value. The results now being obtained by CO_2 recorders in many electricity works are, in the authors' opinion, of doubtful value, owing to the lack of trained expert assistance in their management.

Automatic apparatus of all kinds is, we admit, of great service, but not in the direction popularly supposed. It enables one skilled and efficient man greatly to increase the number and extent of his duties, and practically to treble or quadruple his efficiency; but it does not, and cannot, give accurate and valuable records when placed in the hands of untrained men.

It is remarkable that engineers, after their long experience of automatic stoking appliances, should imagine that automatic testing apparatus will achieve the impossible. In each case, the best results can only be attained when the automatic apparatus is under expert supervision and control; and for testing apparatus, the expert control required is that of a specially trained and skilled chemist. In the working of a large modern boiler plant the chemist is, in fact, as much in place as in a chemical works, and the recognition of this truth will undoubtedly promote the economical working of such plants.

The following words, written over 60 years ago by the pioneer in the movement to obtain more scientific use of coal and fuel in steam raising (we refer to that remarkable man, C. Wye Williams), are still applicable to the position in this year, 1905, and there are few present who would guess that they were a quotation from a book entitled "The Combustion of Coal," published in the year 1841 in Liverpool, and dedicated to the Proprietors of the City of Dublin Steam Packet Company:—

"*It may be asked how it has happened that hitherto the smokeless combustion of fuel has not been effected. I answer, because the* chemistry *of combustion has been neglected, not in the laboratory but* in practice; *and because the construction of our furnaces has been too much left to those who know little of the chemical properties of the materials which are consumed in them.*"

APPENDIX.

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RULES FOR SAMPLING FUEL.

As each barrow load or fresh portion of fuel is taken from the pile or store heap, a count is kept of the number used, and *the whole contents of each tenth or twentieth barrow or portion* are placed on one side, in a cool place, under cover. Care must be taken that the barrow or portion selected for the sample does not contain an unfair proportion of lumps or smalls.

At the end of the day, or period for which the sampling is to be carried on, the heap of fuel obtained for sampling purposes, as described above, is transferred to a sampling plate, and the larger lumps are all crushed down to walnut size. Should no sampling plates be available, four of the iron plates used for covering man-holes and boiler-flues may be utilised to obtain a hard, clean surface on the floor of the boiler-house, and the crushing down of the sample may be carried out on these plates with any heavy and flat lump of iron at hand. The heap of fuel, after this first crushing, is thoroughly mixed by turning over and over with a shovel. The heap is then flattened down, two lines are made across it at right angles with the edge of the shovel, and two of the four opposite sections are selected to form the reduced sample. The lumps in this are again crushed, the sample is again mixed, and the quartering operation repeated, until about 8 or 10 lbs. of fuel only remain, with no lumps that will not pass through a ¼ in. sieve. Two 1 lb. tins, with ordinary or patent lids, are filled from this remaining heap of fuel, after thoroughly mixing the same with the hands or with a small shovel. One of these tins is to be sent per parcels post to the fuel expert for analysis; the other is to be kept for reference in case of dispute.

Discussion at Meeting of January 26, 1905.

Mr. Kershaw.

Mr. J. B. C. Kershaw: I must preface the experiments in connection with our paper with a few words of explanation and apology. Experiments relating to flame and combustion are always somewhat dangerous when carried out in places not specially fitted for such demonstrations, and I have been compelled, therefore, by consideration for safety, to plan the experiments upon a scale that may seem small and insignificant. I trust that you will make some allowance, therefore, for the conditions under which I am working.

The first experiments I propose to show will illustrate the great differences in fuel and the difficulty of burning bituminous fuels without smoke. I will heat in a small platinum crucible successive quantities consisting of 1 gramme each of anthracite, South Wales steam coal and bituminous coal, until all the gases are driven off. The points to be noticed in connection with these operations are (1) The time taken until the commencement of volatilisation; (2) The volume of gas; and (3) The total time required. In drawing attention to these points I desire particularly to emphasise their significance when burning coal under boilers. The following table affords a comparison of results of laboratory tests with the same classes of coal.

A. Sample used.	Per cent. Volatile Matter.	Per cent. Ash.	Time of Heating.
A. Anthracite	3·64	1·70	No gas evolution
B. South Wales Steam Coal	12·00	12·40	15 sec. to commencement 60 sec. to complete
C. Bituminous Coal ...	33·50	4·25	12 sec. to commencement 80 sec. to complete

In the case of South Wales steam coal, with 12 per cent. volatile matter the evolution of gas generally ends with a small explosion, due to the enclosure of gas in the outer crust of coke before evolution is finished; that is, the fuel is a "caking" one. The hydrocarbous gases are split up at high temperatures into their constituents, and the carbon separates as graphite. You may see the graphite on the interior of the crucible and on the cotton wool.

The second series of experiments I have to show you bear upon the cooling effects exerted on flames and gaseous mixtures, by boiler plates and cold surfaces generally. I have already shown that when coal is heated, the gas which comes off is practically the same as the gas from the street mains, and therefore in most of these experiments I shall simplify matters by using coal gas from the Westminster supply. I will show:—

Mr. Kershaw.

(1) The effect of placing a copper spiral (*a*) when cold, (*b*) when heated around a candle flame; the shadows from the smoke of the flame are to be seen clearly on the white screen.

(2) The effect of placing a wire gauze in the blue flame of a Bunsen burner.

(3) The result of exposing paper wrappers on a brass tube, to the full heat of the blue flame of the Bunsen burner.

(4) That water may be boiled in paper cups; on removing the cup from the flame the steam may be plainly seen.

Of the above experiments, Nos. 3 and 4 show how low the temperature of the boiler plates when clean really is; that is, far below the ignition point of ordinary paper.

The third of the series of experiments relates to the influence of oil and scale deposits on plates through which heat is transmitted. Many of my audience would doubtless often like to know the exact heat conductivity of a scale they may take out of their boilers, as compared with that of the boiler plate. So far as I am aware, no very accurate experiments have been made on this subject, and most of the statements as to the low conductivity of scales are based on the diminished steam production of boilers when heavily scaled. The small apparatus I have here is a scale-testing apparatus, designed for testing the relative heat conductivity of boiler plates and scales, and the results obtained are, I consider, sufficiently accurate to be of value for technical purposes. (A demonstration was given with this.)

I have also here for exhibition samples of $CaCO_3$ from a Boby feed-water heater, and two samples of badly scaled water-tubes kindly lent by Babcock & Willcox Co.

My fourth series of experiments relates to the testing of the waste gases. (The improved Honigman apparatus for sampling the waste gas and for testing the CO_2 and O present in these was exhibited, and its use demonstrated here.) The method is based on the use of "tabloid chemicals," solutions of sodium hydrate and pyrogallic acid being made just as required for use, in the burette, which serves both for measuring and absorbing these gases. A water bath is used to obtain the essential condition of success, namely, a uniform temperature before and after absorption of the CO_2 and O. (Charts obtained, with three types of recording CO_2 apparatus, namely, "The Ados," "The Krell-Schultze," and "The Uehling," were also exhibited.)

Colonel Crompton.

Colonel R. E. B. CROMPTON, C.B: I must apologise for the rather hurried remarks I have to make, as I am compelled to leave early. I must commence by thanking the authors; their paper is interesting to me, and I think ought to be interesting to all of us. I think that electrical engineers do not really appreciate how greatly their work is affected by the matters brought before us in this paper, and I do not think many of those who are closely connected with power and lighting schemes know how far their education is defective on these points. I have for a long time past had to thank Mr. Booth for his valuable hints. Mr. Booth has been a writer on this subject in season and out of season; in his writings he has continually put before us our shortcomings in knowledge of combustion questions, and I believe that his preaching

Colonel Crompton.

has had good effect. At all events I for one have benefited to a considerable extent by his writings, and I hope that others will lay the very valuable teaching in this paper to heart.

If I now make some remarks which are to some extent critical, or which appear to differ from the authors, I can only say that it is natural that those who study these questions from independent points of view are sure to hold views which to some extent are divergent. But I will say this, that at no time in the history of the electrical power generating and distributing industry during the last four or five years have there become apparent any such potentialities of future economies and development of one branch of our science as are here treated of by the authors. I think, therefore, that a paper on this subject, coming at this time, ought to be not only criticised by us on its merits, but those of us who are studying the subject ought to add their own views and ideas, in order to help others to go on and do still better than we are doing at present. I shall not, therefore, go very closely through the paper in detail, but just touch on one or two of the important questions which the authors have adverted to, and to which I think they have not given sufficient prominence—that is to say, from my point of view.

In the first place, there is this question of the temperature of flames and the cooling effect of the boiler surfaces which have been so neatly illustrated by the experiments just shown. This is a subject on which I most cordially agree with the authors; they are undoubtedly right, and it cannot be too often repeated that this is so. The methods which were introduced into this country when we first put in water-tube boilers, of putting the furnaces very close to the cooling surface of the tubes, are now known to be barbarous methods, and a great deal of the early discredit which was brought against water-tube boilers was really due to mistakes in the brickwork setting, and in the calculations of the combustion space needed for soft coal, and which the authors have described so fully and clearly in their paper that I need not say anything further on the matter. But there is no doubt that huge mistakes are made by those who technically instruct us in our technical journals when they talk of the prevention of smoke. I believe I am correct in saying that a large number of so-called practical engineers will tell you that you cannot prevent smoke without at the same time causing economic losses; that is to say, that their sole idea of smoke prevention is by the passing of excess of air through the furnace, and therefore diluting the products and making the boiler less efficient. Hence arises a great deal of the difficulty that has been found in persuading engineers to study the matter on account of the prevalent idea that you cannot consume or prevent smoke without losses. So long as it was a mere question of excess of air, this objection was to a certain extent well founded, but I beg you to observe that the authors' method of giving refractory linings to furnaces so that the flames are not cooled before proper and perfect combustion has taken place is the true remedy; it is the right way to prevent smoke, it does not require excess of air, and it does not cause dilution losses; on the contrary, wherever you find that this system has been properly studied in the boiler setting, there you find not only smoke prevention but the highest

Colonel Crompton.

economical results. You find the works where they employ it are not fined for smoke nuisance, and at the same time have record figures for coal economy. I speak on this point with considerable confidence, having at Calcutta introduced water-tube boilers against the advice of all the so-called practical engineers at Calcutta, who said that with the Bengal smoky coal we should be a perpetual nuisance and be rightly fined, and should not be able to produce the large amount of power we required without a complete alteration of our boilers and by going back to the other kind of boilers. When I say at present the Calcutta Electrical Works are the largest power works in the city, and supply a very large amount of light and power, and are the only smokeless works there, and that this is entirely due to following the principles put forward by the authors, I think that is enough to say on the matter. It is the same in London. There are power-houses alongside one another where, in one, frequently heavy fines are incurred on account of continuous smoke, so that they have been obliged to discontinue the burning of bituminous coals, and have to burn anthracite and coals of that kind at heavily increased cost, whilst at other works alongside which are properly fitted with correctly-designed combustion chambers, the ordinary coal is burned smokelessly and economically, and they are not subject to any prosecution for smoke nuisance.

The next point I desire to touch upon is that great puzzle to us all, thermal storage. I see that Mr. Halpin is here, and I have no doubt he will have something to say on this important matter. I only wish to say, as a user of thermal storage, that it has come to stay. It is going to be one of the greatest advantages that electrical engineers who have a peak load have introduced into their stations. Not only does it greatly reduce the quantity of boiler plant required, but it also produces economy of a kind which is altogether inexplicable at the present time. It is probably due to causes which have hitherto escaped the attention of physicists. In my discussions with Mr. Halpin as to the causes of the remarkable increase of boiler output which we undoubtedly obtain from the use of thermal storage we have not yet been able to say with any certainty how it is that when we add storage reservoirs to a boiler, and draw from them at the times of heavy load, the increase in boiler output is not confined to the 25 per cent. that we would be led to expect by calculation, but we get sometimes an output at least 150 per cent. for two hours in excess of the maximum output which we obtain from the same boilers if the feed water enters the boiler direct from the economisers at about 250 degrees Fahr. I cannot, therefore, pretend to tell you the cause of this remarkable phenomenon, but there is no doubt that it exists. Many tests have been made, and the measurements repeated so many times, that there can be no possible doubt that thermal storage is a proved fact, and is certain to be of great benefit to power-station engineers where a peak-load has to be dealt with. I therefore do not agree with the authors that before we use thermal storage we must wait until we have discovered the causes of these phenomena. It is sufficient to us engineers that we have proved the useful effect, and this ought to be sufficient for us; but in addition to the increased output, the effect of thermal storage in increasing the boiler economy of a

station is very remarkable. One power-station that I am connected with, which had previously obtained very good boiler economy, has had this increased nearly 25 per cent. since thermal storage has been added. The cause of this increase in economy is not so inexplicable as the increase of output, as we can evidently account for part of it by the fact that since we added thermal storage we are able to utilise many of the heat units wasted in the brickwork of our furnaces. Colonel Crompton.

Turning to the authors' opinions on Lancashire boilers, I differ from Mr. Booth, for I fancy I here see his hand. He evidently still hankers after this form of boiler. I remember some years ago a series of articles written by him in the Engineering Journals pointing out how ignorant we electrical engineers were of boiler engineering, his main reason being that we used water-tube boilers. I hope he has now changed his mind sufficiently to see that we had reason for what we did. From the very first some of us saw that the water-tube boilers gave us an opportunity of properly designing our furnaces and combustion chambers in the manner which is now so properly advocated by the authors; but there is more in this than the perfecting of the combustion. With the water-tube boiler it is far easier to arrange that the superheating part should be placed in a hotter part of the furnace than is possible with the Lancashire dry back or similar class of boilers. I do not think that it would be ever possible to fit a perfect furnace to the Lancashire type of boiler even if the furnace was constructed in front of the boiler, so that the process of combustion could be perfected in the furnace and the heated products only passed forward through the flues of the boiler, as you would then introduce difficulties with the boiler front plate which would be subjected to very high temperatures, and from this would arise all kinds of troubles connected with the front plate, which are so well known to boiler engineers that I need not mention them.

The authors have touched on the question of furnaces, and mention the chain-grate furnaces of the Juckes type, which have been recently developed by Messrs. Babcock & Willcox and others as being practically on a level with the inclined Rooney type of grates which are used in America. Personally I do not think the inclined type of grate, which I have to some extent studied, and which moves its fuel forward partly by gravity and partly by vibratory motion of the bars, is ever likely to give the same steady and perfect combustion of fuel as the chain or other form of grate which has the surface of its bars practically on a level. This latter form of grate appears to me the one which is best adapted to propel the fuel from the front to the back end, so that during its passage across the grate the whole of the combustible matter can be effectively burnt out, leaving only incombustible matter to pass under the dumping bar.

I agree with the authors as to the economical advantages of obtaining continuous records of the combustion of the waste gases. I have used this apparatus, and I think it is a pity that we are not able at the present time to obtain this apparatus from English manufacturers, but are obliged to buy it in Germany.

I now come to my last, and perhaps most important point, which I

Colonel Crompton.

think has been insufficiently dealt with in this paper, that is to say, the economy which we may expect from using steam in our steam engines superheated to a far greater extent than the authors, apparently in common with most of our teachers hitherto, have told us is necessary. I think the authors are still imbued with the notion that we get the bulk of the advantages of superheated steam from the mere fact that when we use it we ensure that nothing but practically dry steam enters the cylinders. I believe, and I think I have many strong opinions on my side, including Professor Elihu Thomson, with whom I had the pleasure of discussing it during our recent American visit, that this matter has been hitherto misunderstood by the teachers of steam engineering. I think that we get thermo-dynamic efficiency by superheating the steam throughout the whole range up to a total temperature of 1,000 degrees Fahr. Professor Elihu Thomson has made experiments, and has shown that with steam heated up to this temperature economical results can be obtained which cannot be accounted for by the ordinary $\theta\ \phi$ diagram. I regret to see that the authors in their otherwise good and correct paper have not given credit to Professor Callender for the work which he and Professor Nicholson did for us by reading their paper on this subject before the Civil Engineers in 1898. In that paper they pointed out that, quite apart from the thermo-dynamic advantages of superheated steam, it had enormous advantages in reducing leakage past such sliding surfaces as piston rings, valves, or slide valves. The economy arising from this is so important that it improves the economy of most engines, not by an increase of 15 per cent., but by increases up to 50 per cent. or 100 per cent., so that I hope all this audience will look out Callender and Nicholson's paper and read it into this paper. These gentlemen show that initial condensation in the cylinder, hitherto the bugbear of the engineer, was not initial condensation. Most of the losses put down to it were really losses due to the leakage of saturated or partly condensed steam past valve and piston valve surfaces, the water contained in these leakage losses being afterwards re-evaporated just at the time when it is not required, namely, on the exhaust side of the pistons. Again, the authors appear to consider that one of the difficulties arising from the use of superheated steam is connected with lubrication. This is another popular fallacy which cannot be too strongly contradicted in public. I find that what were supposed to be difficulties in lubrication were really due to irregularities in superheat; at one time steam containing water passed over the lubricated surfaces and washed the lubricant from those surfaces. This would be immediately followed by over-superheated steam, carrying insufficient lubricant with it to properly cover these washed surfaces. If your regularity of superheat is sufficient you do not wash away the lubricant, and a small supply of it will then suffice. I believe, therefore, that we are on the eve of building up a new and greatly improved practice in steam engineering, and that when we have followed the new ideas that I have above indicated to their ultimate conclusion, we shall find that the economy of our old friend steam will reach very closely indeed to that of the internal combustion engine, and will possess certain advantages

which we can never hope to obtain from the internal combustion engine. I believe that the time is coming when we shall use steam engines taking in the steam at 1,000 degrees temperature and at 1,000 lbs. pressure, and expanding this steam through several cylinders, possibly re-heating it in the course of its passage from one cylinder to the other; and that when we do this, and use the right kind of boiler and the right kind of furnace, we shall be able very probably to cut down our present best figure of 2·9 lbs. of fuel of 12,000 British thermal units to 1·5 lbs., and such a figure will be very hard to beat by any form of internal combustion engine. Colonel Crompton.

Mr. H. L. P. Boot: I should like to say just a few words with regard to the separation of oil and grease from the condensed water referred to on p. 332 of the paper. The authors state, "There is considerable difficulty in the separation of this grease and oil." My own experience is that, provided the grease separator is put in the proper place and placed at the right level, and the steam is taken up higher before it gets into the condenser, there is very little trouble with the separation of the grease. In fact, in my own plant I have put in a grease separator, and I can use the water in the hot well without experiencing any trouble in the boilers. It is true I have provided additional filters of the Edmiston type, but more for the purpose of a stand-by when we want to clean out the hot well, or when the grease separator is not in use. The water could be used without any filter at all. This separator requires no care and no attention. The only objection, which is more or less serious where water is expensive, is the fact that you lose 5 per cent., or rather, from 4 to 6 per cent., of the water in addition to the ordinary losses, and this has to be made up in *new* feed supply. With regard to the remarks on feed-water heating, I have been experimenting for some time with a system of feed-water heating by passing the water from the hot well, after it leaves the economiser, into a system of trays, arranged in the steam space in the boiler. This has the effect of heating the water up to the same temperature as the water in the boiler before it mixes with same, and also has the advantage of throwing down any of the sulphates or other salts in the trays around the steam space, instead of them collecting at the bottom of the boiler and furnace crowns, or in the tubes if they be water-tube boilers. I remember when I was adopting this it was condemned by one or two friends of mine because they thought it would cause wet steam. I can confidently say, after giving it some four years' testing, that this is not the case, that it answers its purpose well. The only trouble one has with it is that you have to provide for contraction and expansion in the trays in a very careful manner. On p. 336 there are some remarks with regard to hot air. It has often occurred to me, and I do not know whether any of the engineers present have ever tried the effect of lining the boiler with an air lining at the back of the firebrick, that is to say, between the ordinary brick and the firebrick, passing cold air from the atmosphere through this lining, where it gets heated before it arrives in the grate of the boiler. I have made one or two trials of this, but, of course, it is rather a difficult thing to arrange, and I have not been able to get any figures as to the economy gained thereby. Mr. Boot.

Mr. Boot. The authors go very minutely into the question of the various tests that have been carried out at electricity works. Although I quite agree with Mr. Kershaw that when anything is found wrong it should be fully investigated, and that whatever particular portion of the plant is faulty should be ascertained, I do not think it altogether practicable to carry out all the tests he suggests in his paper continuously, because it would require so many more men for the purpose. Experience of electricity works generally rather goes to show that, if anything, they are under-staffed, and have not sufficient men to conduct these tests, except on special occasions, when it usually means overtime. However, it is the practice at my own works, and, I believe, of all engineers, to take the ordinary log readings and obtain from those the lbs. of coal used per unit generated, and as soon as the engineer finds it advancing instead of going down, as we all wish to see, he takes steps to test the plant, and to find out where and why the losses are occurring; but for every-day tests the lbs. of coal per unit generated are sufficient. I was surprised to hear Colonel Crompton speak so hardly of the Lancashire boilers. I have been using water-tube and Lancashire boilers, and I must say that the engineer's lot is a happy one where he has a *combination* of the two. I have found the Lancashire boilers of the utmost value in the daytime in meeting the various conditions of loads caused by fogs and clouds. Unsteady steam-pressure since installing Lancashire boilers is unknown. Water-tube boilers are excellent for rapid steam-raising, but it appears to me that in electricity works, where you have a variation of load every hour very often, and sometimes more frequently than that, you want always a sufficient storage of steam ready at a moment's notice. There is one thing I should like to ask the authors, namely, whether they have found in their experiments that the placing of a superheater in the down-take of Lancashire boilers has set up serious vibration? The usual fire-brickwork construction without a superheater is to put a mid-feather wall between the two flues, so that the gases cannot cannonade against one another. But if you take the feather wall away and fix a superheater in its place, I have found certain difficulties, and the only way to overcome vibration is to remove some of the tubes of the superheater and replace the original mid-feather wall. With regard to the refractory furnace mentioned on p. 340, it has often been a surprise to me why the makers of water-tube boilers, knowing what an excellent article they have, still go on selling the old-fashioned, so to speak, water-tube boiler designed on barbarous "furnace" principles, so far as the combustion of fuel is concerned. Is it because the cost of repair of the refractory furnace is heavy, or is it because there would be a certain amount of time to heat the fire-brickwork, which, during the time it is being heated, undoubtedly would make smoke? The authors speak of the question of always using a cheap class of fuel. I would like to mention that that depends entirely on the location of the town. If you have a railway rate of something like 12s. a ton on every ton of coal, no matter if it is rubbish or the best value, you would soon find that the cheapest class of fuel to use is absolutely the highest steam-raising fuel you can get in the market, no matter what price, within reason, almost you have

to pay for it. With regard to superheated steam, I have been conducting some careful tests on the consumption with steam-engines, under identical conditions, first with the saturated steam and then with the superheated steam to 150 degrees superheat, and I have never yet been able to get above 18 per cent. saving in steam; of course that figure in itself is a good reason for adopting superheated steam. Mr. Boot.

Mr. G. DALE: For many years I have had charge of the fuel department of Messrs. Crosfield's Warrington Works, and for several years also the same department of the Liverpool Works. The results obtained by the application of methods very similar to those indicated in the lecture afford positive proof that a very appreciable gain in fuel economy may be obtained; in fact, a gain far greater than is generally supposed possible. If I mistake not, Messrs. Crosfield's is the North Country works to which the authors refer on p. 331 of the paper, where they show the water-sampling arrangements. They are quite right in noting that some device based on this principle is absolutely indispensable in dealing with water on a very large scale. It may be dispensed with, perhaps, in dealing with a very small quantity, as in a works with one or two boilers only; but when treating five thousand gallons per hour and upwards, I think an arrangement worked on some such principle as that referred to on p. 331 is very necessary. I do not know whether the authors intend to convey the impression that a monthly analysis of the feed-water taken from a river is sufficient. We find in using water from the Mersey, where the hardness is sometimes as low as 16, and at other times as high as 26, with an occasional abnormal hardness of 50 or 60 degrees, that a daily analysis is necessary. Of course I do not refer to an elaborate analysis, but to a chemical examination which can be conducted in a very short time, and from which it may be ascertained whether a temporary modification in the treatment of the water is necessary. River water is frequently undergoing changes in its composition. Rains, varying amounts of sewage, and tides are factors to be dealt with when using water from such a source. As to the advisability of passing exhaust steam containing mineral oils into feed-water, it seems to me that if exhaust steam containing only a reasonable amount of mineral oil be passed into the feed-water, it should not act injuriously, provided the water be kept by suitable treatment on the alkaline side. In the case of fatty oils, great care should be taken to exclude them from feed-water. With reference to the percentage of carbonic acid in the boiler exit gases, I quite agree with the authors that in a large number of works the carbonic acid is not much higher than 4 or 5 per cent. I can speak from experience on this matter, as I have had the opportunity of testing the boiler exit gases at a great many works in England and Scotland. With hand-firing an element of irregularity is introduced, and the composition of the flue gases is continually varying. With a first-rate type of mechanical stoker, such as the Vicars referred to on p. 344, a practically uniform figure can be obtained. When using slack of medium quality, the carbonic acid should reach 13 per cent. in regular working. It is not, as a rule, I think, advisable to attempt to greatly exceed this figure, as beyond this there is a danger of finding Mr. Dale.

Mr. Dale.

excessive quantities of carbonic oxide. This remark does not refer to boiler furnaces in which hot air is used, as under such a condition a larger percentage of carbonic acid may be obtained. The object should be to secure the highest possible percentage of carbonic acid consistent with the elimination of the carbonic oxide. The automatic apparatus for gas analysis has been referred to. Undoubtedly this is useful under certain conditions, but requires checking periodically by a direct analysis to make sure that the indications are correct. The great difficulty of this apparatus appears to be its inability to give information respecting the carbonic oxide. For example, 15 per cent. of CO_2 might be indicated, but if 1 per cent. of CO were present, the economy would be more apparent than real, as 12 per cent. of CO_2 with entire absence of CO would be a much more economical figure. The remarks on sampling and analysis of fuel I entirely agree with. The proximate analysis, that is, estimation of ash, water, fixed carbon, and volatile matter, is all that is required for technical purposes, and from this the calorific value can be calculated. In some cases it may be desirable in connection with that to conduct an ordinary calorimeter test. On the subject of superheaters, I may remark that in the Warrington Works we have found great advantage from an arrangement which I think is not very well known in this country, or, at any rate, is not extensively used. Most of our Lancashire boilers in the Warrington Works are 30 ft. by 9 ft., and instead of covering the top, as is usual with this type of boiler, with non-conducting material, we utilise it as heating surface, and over this we fix two horizontal tubes, each about 30 ft. long and 3 ft. in diameter. Between these tubes we construct a mid-feather, so as to cause the hot gases to traverse the whole length of the tubes. Of course it is only the gases which under ordinary conditions would be leaving the boiler that are led up into this superheater chamber. The temperature of these gases will not, as a rule, exceed 400° C., and we gain about 70 to 80 degrees of superheat. This may appear a small figure, but it is uniform and continuous, and in most of our processes is quite sufficient for our purposes. We have used this system for several years, and have found it very satisfactory. If any members of this Society who are specially interested in this subject would care to visit our Warrington Works, we should be pleased to show them our methods of water purification, gas analysis, and other processes which are connected with this subject of fuel economy.

Mr. Druitt Halpin.

Mr. Druitt Halpin: I quite agree with what Colonel Crompton has said respecting this paper. It is most valuable, and gives a great deal of information which we are much in need of. There are only one or two parts I do not agree with, and there are one or two parts I do not understand, which is probably due to my own fault. I quite agree with what the authors say on the first page about batteries; certainly it seems a most brilliant field for an inventor of electrical genius to devise a good battery, and I hope it will not be long before it is done. The present battery, its enormous price and its severe depreciation, and the very poor return it gives—not more than 75 per cent. of what is put into it—is capable of much improvement, and a good battery would be exceedingly useful. On p. 330 the authors refer to the

Mr. Druitt Halpin.

question of feed-water and the carrying of the dirt directly into the boilers. Of course that is a very unprofitable thing to do. If dirt is present, the best thing to do is to get rid of it before it does the mischief which it will naturally do inside the boilers. When dirt gets inside the boilers and establishes itself inside the economisers or in water-tube boilers, the temperatures present bake that dirt on and make it very much more difficult to deal with. The ideal condition, if you have to get the dirt inside, is to deal with it in some way that it does not bake on, and is soft enough to be easily removed. The authors also referred to the necessity for frequent analysis of the water. A previous speaker has mentioned analysing it once a day, and when it ranges from 15 to 60 degrees in hardness I should say that is a very necessary precaution. Mr. Stromeyer, of the Manchester Steam Users' Association, in his reports has drawn the attention of boiler owners strongly to this fact, particularly in dry weather, that the salts become more concentrated, and consequently do very much more damage. With regard to the question of lubrication, dealt with on p. 332, of course vegetable and animal oils have practically ceased to exist now for the high temperatures that are being used in steam cylinders, and the mischief with mineral oils is less, although a good deal of mischief can be done even with them. The quantity used, however, is also much less. In connection with the use of zinc the authors recommend the projection of the zinc into the body of boiling water in order to promote regular steaming in the boiler. I do not know whether that is a misprint, but I fail to understand it. If there was good contact with the zinc when it began its work it might temporarily form a kind of Wye Williams heat peg, and in that case there would be a greater rate of transmission. I do not see how "help to promote regular steaming" is to be explained. On page 333 I think there is another misprint. Speaking of the motion of the water in boilers, they say: "A possible explanation of part of the gain observed when feeding boilers with water at or above 212 degrees F., is, therefore, that the loss of heat due to the performance of merely mechanical work is avoided." Surely there they mean the "conversion" of heat into mechanical work. They do not profess to be able to annihilate heat. They also refer on that same page to a paper read by Mr. Hamilton in Belfast, which gave a great deal of useful information on heat transmission through boiler plates at high temperatures. In connection with that particular matter, they do not give credit to whom credit is due for one of the original investigations, the late Sir Frederick Bramwell and Dr. Anderson. They made experiments in this way. They had a jacketed vessel with a steam pipe, A, and a pipe B, to drain it. WL was the water level, and the pan was open to the atmosphere, so that the temperature of the water was always practically constant. They made experiments with steam at 5, 10, 15, and 20 lbs. pressure, and corre-

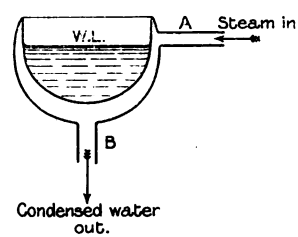

FIG. A.

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sponding differences of temperature. What they found was this: that when they worked below 212° F. below the point where convection currents become very marked, they got a line of A, B (see diagram).

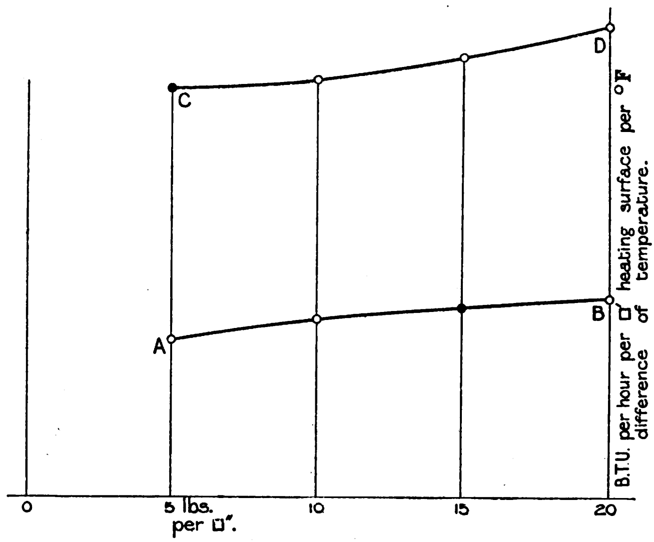

Fig. B.

Here the transmission was given in units per hour per square foot per degree difference of temperature. As the temperature went up so did the rate of transmission go up also. They then tried at about 220 degrees F., and they got a curve, CD (see diagram). These curves were not even parallel. When testing above 220° F. the transmission was very greatly increased. The experiments were made very carefully and they were very valuable. On p. 334 the authors speak about sampling coal, and I quite agree with all they say. Many years ago I discussed this matter more than once very fully with Mr. Mair-Rumley, who had a good deal to do with those matters, and we quite agreed that buying coal without a guarantee of the heat units it contained was a very unprofitable business, and that this certainly should be avoided if possible. I decidedly think coal ought to be bought by the heat units it contains. We have taken one step in civilisation in that respect; instead of looking at a heap of coal and judging by the eye as best we can what it may weigh, and say we will give so much for that heap, we go through the formality of weighing the coal, so that we know at least the weight even if we do not know what is in it. The next step is to know what is in it.

The authors then go on to the question of coal, and refer to the best Welsh coal, and I am with them in all they say there. I certainly object to paying a fancy price for best Welsh coal, because what you are buying is not proportional in heating power to the cost of ordinary coal. It is

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very much like the old trouble in days gone by with regard to best Yorkshire iron. Ordinary iron stood roughly 20 tons per □″, and you paid £6 or £7 per ton for it. Best Yorkshire iron stood 23 or 24 tons, and they made you pay £20 for it. I used to say that one would be quite willing to pay for the extra strength if it was sold at some reasonable proportion. It seems to me this is analogous to that case. The best Welsh coal has probably 14,500 in it, and you pay 18s. or 20s. for it delivered in London, whereas, if you take coal with 13,500, of which there are large quantities sold in London, that coal is nothing like the price proportionally. If it is burned in the way the authors say, which is the only rational way under the circumstances, experience shows the highest possible effect can be got out of it, and it can be burned without the slightest danger of smoke. They go the right way to work, and instead of talking about the nonsense of smoke consumption, which is physically impossible, they point out the right way to look at it, as smoke once produced can never be consumed at any reasonable cost. It can only be disguised by enormous air dilution. On p. 337 the authors referred to temperatures. These questions were very fully treated by Rankin, Clarke, and others. They make comparisons between chimneys and fans. The great example we have to look to in that particular matter is in coalpits, because there is nowhere where coal is so cheap as at the bottom of pits. Up to forty or fifty years ago the greater part of the ventilation of pits was done with open furnaces at the bottom of the upcast shaft. I am not a mining engineer, and I do not know whether that system is much used now, but I do know there are a large and increasing number of fans made, and that the work is done mechanically, and much more effectually, and much more cheaply, and, what is of greater importance, with much more certainty. When they were depending on the furnace for draught, if they had any down-draught they were very likely to get into trouble, whereas now they are masters of it, and they have no trouble at all. Each case must be dealt with on its own merits, and it is a question of the large capital in a chimney, the cost of erection and the simple capital charges, against the smaller capital charges for fans and engines or motors with very heavy wear and tear, plus the power of driving them. With regard to the very high temperatures in furnaces, Mr. Stromeyer has again done exceedingly good service in drawing attention to the fact that by overdoing this question of temperature it is possible to bring about exceedingly undesirable results. On p. 338 the authors speak of dampers. Air leakage can be avoided in an exceedingly cheap and simple way by an arrangement designed by Mr. Schönheyder, and described in the last number of the *Municipal Electrical Engineers' Paper.* Instead of having a great gash in the brickwork 2 ft. to 2 ft. 6 in. long up to 6 in. or 7 in. wide, they have a hole ⅝ in. in diameter, and perfectly nominal expense in brickwork. I quite agree with what the authors say about the folly of adopting cheap engines to go over the peak of the load, because if you adopt cheap engines they take a great deal of steam, or you spend a great deal more in the boilers and have all the stand-by losses in the boilers in that way. They compare water-tube boilers with Lancashire boilers. Of course, water-tube boilers, when

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driven in the ordinary way at anything like a decent rate of evaporation, produce an enormous amount of priming, which is caused to a great extent by very narrow necks, through which the whole of the water and steam is driven several times. There is a powerful object lesson on that point in some late numbers of the *Engineer* newspaper. In a big power-station in America they had a great number of water-tube boilers and a large number of vertical engines, and they were continually getting the cylinder covers off without unloosing the bolts; but they changed it by a suitable system of baffle plates, and I believe the thing is quite right now. With regard to the drawings given, particularly Fig. 2, of the furnace, I have very often seen those furnaces at work, and as far as combustion goes and a general all-round efficiency, I must say I have never seen anything like them. They use a very low class coal and are perfectly without smoke, and within my own knowledge they are getting evaporative results which are exceedingly hard to beat, or even to equal, and that quite apart from anything else. Of course, one great trouble with those boilers is the enormous radiating surfaces they have in the brickwork setting and the enormous quantities of air thus allowed to leak in, which has a most pernicious effect. On p. 343 the authors make a remark on which I wish to say one word that I hope they will not misunderstand. They say that possibly twenty years ago they were the first to draw attention to this. I am sure they have done very valuable missionary work in drawing attention to it, but I do not think they were the first. I will show you an experiment that was shown to me several times by the late Mr. McTear, a very well known engineer and chemist in Westminster. It relates to the teaching he received as a student in Glasgow University in the fifties. At that time the late Professor Rankin and Lord Kelvin were at the head of these matters. (Fig. C.) *This* is an ordinary brick, and on this brick they put a funnel, A; then they put a cone on the other side, and *here* they put a candle, C, and by blowing in through A they blow the candle out. The Germans have greatly elaborated the subject. The most complete experiments ever carried out on the matter were published by the Commission, who examined and tested a great variety of boilers at the Frankfort Electrical Exhibition. They analysed the gas at the bridge and at the other end of the flues going out at the damper, and, of course, the observed differences must have been due to the air which came through the brickwork. They showed the whole of those results in a very complete way. There were a number of boilers, and a curve of efficiency was plotted from the results. I am not quoting accurate figures, but they had an excess of 25 per cent. of air. In another case they had an excess of 210 per cent. A fair curve resulted, and the whole curve of efficiency came down in exact proportion to the surplus air they were letting in, and getting heated up at the expense of the boilers. This is translated and

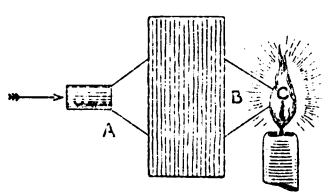

FIG. C.

fully illustrated in the last number of the journal of the Institution of Municipal Electrical Engineers. Mr. Druitt Halpin.

DISCUSSION AT MEETING OF FEBRUARY 9, 1905.

Mr. DRUITT HALPIN: This is probably one of the most serious questions we have to deal with, and it is of course particularly so in the case of water-tube boilers, where the wall area is relatively so large compared with the brickwork in Cornish boilers and Lancashire boilers. The experiments made in Frankfort to which I referred at the last meeting threw the greatest possible light on the subject. They have all been translated, and can be seen by those who are interested in them. Radiation in pipes is a thing with which great care must be taken, particularly in flanges, because I am of opinion that whether flanges are one foot apart or ten feet apart, the flange will always radiate the same amount of heat as the length of the pipe between the two flanges. You have the bolts and bolt heads and an enormous quantity of surface of the most efficient kind for radiation. On p. 344 the authors refer to mechanical stoking. I certainly believe that with many of the chain grates there is a great deal to be desired, because the motion of the grates is uniform; and they admit enormous quantities of air at the back, which necessarily is only doing harm. I have seen a drawing of one which I think will get over that difficulty, and make that type of grate very much more efficient. On p. 345 the authors show a section of the Weir boiler, which is one of the multi-tubular type, and which is provided with a good deal of brickwork to enable proper combustion to take place before the products have been utilised. But I should like to put on the board another boiler of that type which they have not noticed, which has a much simpler arrangement for completing the combustion. It is the Fryer boiler. The tubes are straight; the furnace is completely enclosed in brickwork; there are two furnaces back to back, one at each end. The gas comes out in the opening between them, and goes through amongst the tubes into the casing, producing perfect combustion before any attempt is made to utilise the products of combustion. In one of the earliest of these types of boilers an inventor in 1827 or 1828 had outside the casing two downcomer tubes, ten or twelve inches in diameter, and in order to assist the water circulation he put a shaft in through the bottom, and on this shaft a screw propeller was carried. He put a coal sprinkler lower down on the shaft, like the automatic sprinklers of the present day, and a fan lower down still; he thus got the whole thing on one shaft. He was a little before his time, because he made it in 1827. In Fig. 7 a system is shown of troughs underneath the chain grate with the hope of excluding superfluous air, but I very much fear that it would not be possible to work a system satisfactorily with that arrangement, because there would be enormous leakage through it. On p. 355 the authors refer to a controllable superheater. I think a controllable superheater is a very good thing, if it can be controlled, and it may save trouble. Mr. Booth was good enough to send me a pamphlet showing this arrangement, which I have carefully examined. The superheater consists in principle of a couple of concentric pipes; but I am

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afraid the complication of all the joints that are shown would be very hard to deal with and to maintain. I think there would also be another objection to that superheater, namely, the depositing of lime in the very small spaces that are available. The authors may say in reply to that, and I quite agree, that all water should be purified before it goes into the boiler and is subject to heat, but that cannot always be properly done. I saw a case not very long ago where they had an exceedingly ample provision for softening water, which was supposed to be worked by carefully trained people, and all the quantities were logged; and in that particular place, amongst a quantity of other boilers, they had one water-tube boiler. They worked this water-tube boiler for, I think it was, twenty-four days, day and night, not very hard, and then they stopped; and afterwards they told me they had three happy weeks in cleaning it out. If that happens in an ordinary water-tube boiler it might happen to a very much more serious extent I fear in the case of annular boiler pipes. I quite agree with what the authors say on p. 350 about the production of steam; I think it is just the inverse of its utilisation. Experience has taught us long ago, although it was not always believed, that the best effects can only be got out of steam during its utilisation by using it in very easy stages. Forty years ago no one believed in a compound engine. If you were going to expand ten times, they said, you might just as well do it in one cylinder, cut off at one-tenth, and utilise the steam there, rather than, as they said, "worrying" the steam through three or four cylinders. Of course, we know that the ranges of temperatures produced by that process are very beneficial and give very economical effects. In the same way, as we go down in the heat curve I am perfectly convinced we must go up, and go up in stages by feed-heaters and other arrangements, and go up slowly. I have only one more thing to say, namely, that I cannot agree with the figures the authors give on p. 353 with regard to the maximum available temperatures. The authors have not given what is in the coal, but I took the coal from the figures that they gave on the preceding page. On that page they gave the coal at something like 13,600 units, and working the figures out in that way I find the temperatures are very much higher. The authors give the figures as 3,500 to 3,700 with coal at 13,600. As I work those temperatures out they come to 4,400; and, with a higher grade coal of 14,500, the best Welsh coal, I got a maximum of 4,580, with a theoretically sufficient quantity of air; with 50 per cent. excess of air I got 3,215; and with 100 per cent. excess of air 2,440.

Mr. Rosenthal.

Mr. J. H. ROSENTHAL: I have looked through this paper with a good deal of interest, and I do not take the dismal view that Colonel Crompton did of the knowledge of central station engineers on the subject of it, because out of a very extended experience I know that most of them are well acquainted with the subjects that the paper deals with. Dealing first of all with thermal storage, it would appear as if this is surrounded with some mystery; but there is really nothing at all mysterious about it. The results that are brought out prominently as being obtained at the Kensington station only show what one would theoretically expect thermal storage to

Mr. Rosenthal.

achieve. Naturally it is better to feed a boiler with very hot water than it is to feed it with cold water, and naturally a boiler will do more work, but it is quite a mistake to suppose that the less the range of temperature in the water that is inside a tube the greater the amount of heat units that can be transmitted through a certain amount of surface. Then, again, dealing with the question of fuel supply, the paper indicates that specially trained men are necessary to analyse coal, flue gases, and water. That is not so. To analyse flue gases, coal, and water is a very simple matter; and I am quite certain that any central station engineer with a little application could easily acquaint himself with all that is necessary, and do it just as well as any expert. At any rate, any one who desires to be fully acquainted with it has our laboratory at his disposal. Then Mr. Boot spoke of the barbarous ignorance, or something to that effect, of water-tube boiler makers in designing furnaces. If boiler makers are barbarously ignorant they are waiting to be civilised. At any rate they have got so far as this, that if they design a plant the way they can design it they can give about 85 per cent. of all there is in the coal back in the steam, and it wants a good deal of civilisation to get beyond that. So that the ignorance of boiler makers is not so very dreadful. Then it is stated in the paper that mechanical stokers are not a cure for the smoke evil. I have had twenty-five years' experience of that particular subject, and I can say most positively that mechanical stokers are absolutely the only cure for smoke with smoky fuel; and no amount of furnace designing other than properly designed mechanical stokers will prevent smoke, unless a boiler is practically doing no work, or running at the rate of 6 lbs. per square foot of grate, like they used to run boilers of the shell type twenty years ago. In central stations, at any rate, it is not possible to burn a bituminous fuel smokelessly without mechanical stokers. Then the chain grate has come in for a great deal of abuse. It is supposed to lose a great deal by excess of air. Excess of air is, of course, always a trouble in furnace management, but it is a bigger trouble in hand firing than it is with any mechanical stoker. Probably the authors of the paper were not aware that there are at the present time devices in regular use in connection with chain-grate stokers which enable a boiler plant to be run with 10 to 12 per cent. of CO_2. The question of air blown into the furnace for mixing the gases is also dwelt upon. That was a common practice in the Navy in Belleville boilers, and it is not at all a bad practice. But the action of the air blown into the furnace is purely mechanical. If the furnaces of those boilers were sufficiently large, the air would be of no benefit whatever, and would be quite unnecessary. The air is a good thing to retard the gases in the furnace before they touch the heating surface of the boiler when the furnaces are small. Then the authors say that there is no thermo-dynamic gain from superheat. I think a previous speaker has already stated that that is entirely wrong. Superheating to-day is too common for such a notion to take permanent ground. I could cite hundreds of instances of plants worked without superheaters, and of plants worked with superheaters, and it is found that the gain is in the coal bill; and I have not the slightest doubt there are many central

Mr. Rosenthal.

station engineers here to-night who can bear me out. One of the most recent instances that I know of is at Hampstead, where separately fired superheaters have been put in. Separately fired superheaters are not as advantageous in respect of fuel economy as superheaters that are integral with the boilers, but they are far better than no superheaters at all. The best proof of what I am saying is that at this station the minimum saving in actual expenditure of fuel per unit generated is 10 per cent. There are many other points in this paper that I could dwell upon at length, but I have only dealt with the main points, so that nobody should go away with the erroneous impression that everything at the present day in boiler management is as bad as the paper would lead one to suppose that it is.

Mr. Holland.

Mr. H. N. HOLLAND: The authors made some remarks on p. 332 with regard to thermal storage, which have already been discussed a good deal. At the same time they have also made some references to batteries, which I do not think are altogether fair. This battery question is a question that we central station engineers have got to face; and I maintain that in modern plants, where we have polyphase generators and distribution from sub-stations with direct current, batteries are practically an absolute necessity. The burning out of any synchronous motor or dynamo will frequently throw all the synchronous machinery out of step; and it means a complete shutting down. We have got to get these sub-stations away. If there is no battery we must start up generally with induction motors, and then start the synchronous sets on the D.C. side and synchronise, and then have to build up several motor generators on a peak load and a dead net work. I have known some cases where it has taken over half an hour, but if we can only have sufficient battery power we can get away in about five minutes; if the battery is big enough we need not have the lights out at all. The authors have referred on p. 335 to the question of fuel testing. Mr. Rosenthal has already dealt with that subject, and has stated that chemists are not required in order to get calorimetric tests for coal. I should like to endorse that statement. There are plenty of central station engineers who can use a calorimeter; but at the same time you must not depend on a calorimetric test only; you must take a test under the boiler as well. These tests have been referred to in the paper as being very expensive. I maintain if they are carried out as they should be done they need not be expensive at all. The system we adopt is this. We have a complete set of plant, such as pumps, CO_2 recorders, and other apparatus, which is all ready to be coupled up to any boiler, and which can be done at a few hours' notice. We have a certain number of men who have been trained to this class of work in our boiler-cleaning gang; they have been taught their several duties in the way of measuring coal, in the taking of the tanks and the temperatures, and so on. The tests of the boilers are not expensive under those conditions, and they supplement what is being done in the laboratory by means of the calorimeter. The authors recommend express boilers for fog loads. Express boilers will not work with the ordinary mechanical stoking; they need hand firing and special firing, and they need a higher paid and more skilled man than the average stoker in a

Mr. Holland.

central station. It has been my experience that you can deal with fog loads with ordinary water-tube boilers and mechanical stokers. It is simply a question of having a sufficient number of boilers banked. On p. 343 the authors call attention to the question of air leakage. I think that is a very important question, and I do not think anything like sufficient attention is paid in stations to that matter. At the same time I think a good deal of it is due to faulty design. The authors recommend that glazed brickwork should be used. Glazed brickwork is certainly more impervious than ordinary brickwork, but I do not think that is the real cure; we want to go more to the root of the trouble. In water-tube boilers especially, a great deal of the cracking and leakage through external brickwork is due to difference in expansion. We have a big firebrick lining several feet long and several feet high, which is bonded to the external shell of the boiler. It is bound to expand with the tremendous heat we get in the water-tube boiler, and something has to go, and it is generally the external brickwork that goes. A great deal more of this cracking is caused by the bad fixing of ironwork, damper gear, and arrangements of that kind fitted on to boilers. They are put on in a bad way with Lewis bolts, not enough of them, and badly spaced. I have myself often seen boilers with brickwork damaged by the strain of ironwork that was never meant to be put on them. We have found it a good plan to build up the firebrick lining entirely separate from the shell of the boiler. That allows the firebrick room to expand without pulling down the outer brickwork. The firebrick lining does not last so long under those conditions; but it is very quickly rebuilt. You might put an external second shell round the boiler. If you have not got room for more brickwork you might build it with ferro-concrete, or something of that kind; but I think it would be a good thing to have an extra shell right round the boiler with an air space of two or three inches. In that way you would get an external shell that was more or less cold, and which would not be troubled with this expansion strain, while at the same time you would have the great advantage of getting heat insulation and tight brickwork. Having an air space is the best insulator you can have for heat. The authors have made some remarks with regard to chain-grate stokers. Mr. Rosenthal has referred to that subject, and I should like to endorse some of his remarks. I find that the chain-grate stoker, properly designed and properly worked, is absolutely smokeless. The authors say that mechanical stokers require cleaning. The chain-grate stoker does not require cleaning. The stokers can be run for a week on end without any cleaning whatever. The smoke question is almost entirely a question of suitable brickwork. The authors give good rules in the paper for designing brickwork, but recommend as ideal the stoker which is shown in Fig. 8. If you look at that figure I think you will find it is meant for the rocking inclined grate stoker, and I think it contradicts the very principle that is laid down by the authors. It is about as bad a setting as you could have. There is only a very short arch, and the gases can get right away on to the cold tubes. On p. 349 there are some references to superheaters and jackets. The superheating question has been gone into by previous

Mr. Holland.

speakers, but I should like to put in a word for the steam jacket. We know they are not very economical when you have a high superheat. Still, we engineers cannot do without jackets. As long as engines and dynamos break down we must be able to get engines away at a minute's notice, and an engine must be kept hot to do that, and to do that we must either have a steam jacket or a leaky stop-valve. A by-pass valve is about equivalent to a leaky valve in economy. I should like to endorse what the authors have said about the Ados recorder. We find it a very good plan to fit an Ados machine to serve any one of four boilers, and by an arrangement of cocks and pipes we couple it to any boiler we please. We have it in a large wooden cupboard to prevent any man tampering with it, but we provide a small window, so that the men can look through and see the chart. By those means we can promote a healthy rivalry among the men as to who can get the best CO_2 line. We have no difficulty with regular working—not with a test load or under special conditions—in getting 12 to 14 per cent. CO_2 with a chain-grate stoker. I also agree with the authors that it does require expert attendance. We find these machines in regular working need cleaning once a week, and occasionally they need checking with some other type of apparatus.

Mr. Gaster.

Mr. L. GASTER: The points on which I should like to say a few words refer to the use of liquid fuel, and the use of proper refractory materials for furnace linings. The reference made by the authors to the merit of liquid fuel appears to me to be insufficient, as I believe liquid fuel deserves a more careful consideration. It is scarcely fair to speak of liquid fuel as an expensive material, in view of its very high calorific value, of the absence of clinker and ashes, and of the considerable saving in stoking, besides possessing many other advantages. Owing to these qualities, it is quite possible that liquid fuel will soon receive serious consideration at the hands of the central station engineers, particularly in the case of plants working in connection with dust destructors, or at stations where a portion of the plant has to be kept in reserve, and ready to be brought into action at very short notice. For this purpose there is no doubt that the liquid fuel may come to be considered not as a competitor of coal, but as very satisfactory auxiliary in raising steam at central power-stations. The quantity of available liquid fuel is daily increasing, and with the extended use of electricity for boring and pumping oil wells, with the further development of oil fields, and the increased facilities for transporting large quantities of it in bulk, it is tolerably certain that a reduction in the price will follow in time, and it is by no means likely, as the authors of the paper think, that the more general employment of liquid fuel will deplete the market and cause a rise in prices.

I quite agree with the authors in what they say regarding the use of proper refractory linings in furnaces. This subject deserves much greater attention than has been given to it hitherto. There are several refractory materials known to be useful for furnace linings, like Carborundum, Silica Carbide, Crystallised Magnesite, particularly the one obtained from India,* and last but not least Siloxicon, one of the

* E. Kilburn Scott, *Transactions of the Faraday Society*, vol. 1, 1905.

latest products obtained by the aid of the electric furnace, the properties of which were described by me in a short note in the Transactions of the Faraday Society.* It is believed that the products of the electric furnace are the most efficient for use as a protecting coating to the ordinary furnace brick linings. Mr. Gaster.

I should like, however, to hear from the authors what in their experience is the best material to use in lining furnaces.

Mr. Mordey.

Mr. W. M. MORDEY: May I give a figure that may be useful as a rough guide in estimating radiation losses and realising their importance. In designing and testing transformers some years ago I found that the loss from the surfaces of iron transformer cases, painted grey, was one watt for three square inches when the surface was raised to the very moderate temperature of 40° F. above the air—that is 48 watts per sq. foot, or 420 Board of Trade units a year a sq. foot.

Whilst we are all very interested in improving the efficiency of steam plant—and unite in thanking the authors for helping to that end—I, for my part, always feel how hopeless the subject is—even the very best results are very disappointing, and it is still more disappointing to know that the improvements attainable, practically, are not likely to be more than 1 or 2 per cent. The boilers are not most to blame—the chief losses are not there. Take one figure: Suppose we get down to 4 lbs. of good coal per unit—a result not yet attained, I think, in any station—that means an overall efficiency of 6 per cent. or less. By all means let us try and get the best we can out of steam, by improving our accessories and by care in details, but let us keep very clearly in our minds the fact that to get any efficiency of which we shall not be ashamed to speak, something much more radical is wanted.

Mr. Molesworth.

Mr. W. H. MOLESWORTH: The authors have made no reference to the use of pulverised coal, and I should be glad if they will state whether the following figures agree with their own experience. The test was made to compare hand firing and pulverised coal: boilers of the Babcock Wilcox type being used. One boiler was arranged for hand firing and the other equipped for the utilisation of pulverised coal, but in all other respects the boilers were identical, as also the auxiliary plant, and the conditions under which the tests were carried out.

	Pulverised Coal.	Hand Firing.
Coal per hour per sq. ft. of grate	19·27 lbs.	18·65 lbs.
Equivalent evaporation per hour per sq. ft. of heating surface.	3·65 „	2·38 „
Equivalent evaporation per lb. of fuel	7·21 „	4·86 „

These tests showed that with the same amount of coal there was an

* *Transactions of the Faraday Society*, vol. 1, p. 118, 1905.

Mr. Molesworth.

increase in evaporation of 48 per cent. in favour of pulverised coal, or taking the same evaporation a saving of 29 per cent. in coal. The coal used was bituminous, that for the pulverised tests being finely ground while that for the hand firing tests was as received from the colliery. While operations with pulverised coal were going on the furnace was practically smokeless. If the authors can give any figures of cost and coal consumption relating to tests with pulverised coal, I am sure they would be a valuable addition to this very interesting paper.

Mr. Thomson.

Mr. W. C. THOMSON: I quite agree with the authors that it is possible to burn bituminous fuels with the furnaces constructed for these without creating smoke if, and this is an important point, you already have a fire. In a power-house of any considerable size, especially in a lighting station, there may be in use some twenty-five to thirty boilers on the peak of the load. For 12 to 14 hours five boilers will be sufficient to meet the demand, and the authors appear to have overlooked the question as to how the additional twenty to twenty-five boilers are to be put away in the evening, burning bituminous fuels, without creating smoke. The local authorities do not allow you to make smoke at any time, and it is my opinion that it is absolutely impossible to start up a number of boilers burning bituminous fuel either from banked fires or in some cases no fires at all, without creating such a smoke as to be a nuisance. It entails therefore the building up of the fires with Welsh coal, and this is impracticable in most central stations.

I also disagree with what was said by Mr. Rosenthal to the effect that mechanical stokers burning bituminous coal are the means of preventing smoke. The chain grate is no better than many others, and there is no mechanical stoker on the market which is capable of dealing with the conditions mentioned above.

(*Communicated.*) An important point not put forward by the authors is that provided your chimney draught is not excessive and good bituminous fuel is used, a much larger evaporation per hour can be obtained from the boilers than by the use of Welsh coal. This effects a double saving—in the first place capital cost, as additional boilers can be dispensed with, and secondly, the loss due to banking or relighting fires.

On p. 330 of the paper the authors state it is not their purpose to go at length into the theory of water softening for steam-raising purposes, but they state on p. 331 that any water over 15 degrees of hardness should be softened. With this I do not altogether agree. With a large battery of boilers it is absolutely necessary, but with a small plant, say two or three boilers, the capital cost of the water-softening apparatus and the cost of working same, I think would be found to be an expensive luxury. There are several water-softening apparatus in the market, the manufacturers of which state the cost of softening is approximately 0·75 pence per 1,000 gallons. From some very careful records I have had kept of a water-softening plant for a period of twelve months I find the cost is more than double. The makers do not appear to take into consideration that the apparatus requires cleaning, repairs, and wages expended in attending to it. I

Mr. Thomson.

find that the lime and soda alone come to 0·76 pence, water for sludging, 0·15, wages in attending, 0·43, repairs, 0·23, stores, etc., 0·01, making a total of 1·58 pence per 1,000 gallons, which does not include the cost of the engineer's time spent in testing the water. The lime used in this apparatus is the best that can be obtained; the cost could be reduced slightly by using an inferior quality, but I found that by doing this more attention is required, and the results obtained are not so good. Notwithstanding the fact that your water is softened you will still have a certain quantity of scale to deal with, and in plants which have already been in use some years without the use of softened water a considerable amount of scale will have accumulated.

I should be glad if the authors would give an opinion as to the best means they have found for getting rid of scale. There are hundreds of boiler compositions on the market, many of which are quite useless and some even detrimental, but all very expensive, especially when the majority of them contain 99 per cent. of water. The most suitable composition I have found is mica ground very fine. Mica has the property of expanding when cold and contracting under heat, and if it is mixed with a little soda and a quantity proportionate to the amount of water evaporated passed in each day with the feed-water, I have found that the mica works underneath the scale, and when the boiler is shut out of work and allowed to cool, the mica in expanding bursts away any scale that may be adhering to the sides of the plates. This is especially the case in Lancashire boilers in the narrow ways where it is almost impossible to chip. It also prevents new scale being formed, a deposit being thrown down to the bottom of the boiler in the form of mud. It is an important point, which I think should be investigated further, as to the best means of getting rid of old scale or the prevention of new without having any detrimental effect on the plates.

As to the cost of fuel, I agree to a certain extent with the remarks made by Mr. Boot that where your power is situated at a considerable distance from the source of supply, necessitating heavy charges for carriage, it is not economical to burn the cheapest class of fuel. I think it will be found in England, if the heat units contained in the coal are proportionate to the price paid, the cost of the coal at the pit mouth should be approximately equivalent to the carriage charges, *i.e.*, that the most economical point is reached if the carriage charge is 10s., and the cost of the coal at the pit mouth is about the same price. If the carriage is 5s., you can burn an inferior class of coal costing approximately 5s. at the pit mouth. Colonel Crompton mentioned in the discussion that he had succeeded in reducing the pounds of coal per unit to 1·9. I should like to know if this was with the use of bituminous coal or Welsh, and if under test conditions or under an ordinary week's running conditions, the figure given being, I take it, with the use of thermal storage and superheated steam.

Mr Ruthven Murray.

Mr. E. T. Ruthven Murray: It is to be regretted that more engineers actually in charge of steam plant have not discussed this interesting paper. A great many points touched on are of enormous importance in promoting fuel economy. The authors appear to suggest

Mr. Ruthven Murray.

that those who design plant do so on *laissez faire* principles, and the purchasers put down whatever the makers choose to give them irrespective of future trouble and expense. The truth is, that when designing a boiler-house plant one cannot afford to experiment. It is the maker's place to demonstrate the improvements which can be effected in the plant he supplies rather than the purchaser's. Numerous small matters involving very little expense might receive more attention than they do, and one or two speakers have already referred to one of these, namely, the reduction of radiation and air leakage losses in boiler settings. A great deal can be done to economise heat in a very simple and inexpensive manner by the use of glazed brickwork settings. I find that glazed brick facings do not increase the cost of the settings by more than 12 per cent., while they are of great value in reducing the fuel costs. They not only prevent leakage of air through the brickwork, owing to the fact that the glazed surface is impervious, but if the joints crack, owing to expansion and contraction of the brickwork, you notice it at once, and they can be pointed up every day. But there is a second advantage in the glazed surface as compared with the usual rough one, namely, the heat is less easily dissipated, and by its retention the settings themselves remain enormously hot. I tested some this afternoon and found that the external surface of the brickwork settings, in boilers that were being moderately fired, got up to as much as 140° F. with a boiler-house temperature of 52° F. If the heat can thus be retained in the setting instead of being dissipated into the surrounding atmosphere, a large economy results. As a matter of fact with these same settings it is quite a common thing for the boilers to be pumped up with cold water after the fires are banked, and to find full pressure on them after they have been standing for twelve or fourteen hours. The boiler pressure actually rises with banked fires. Briefly, then, the use of glazed brick settings affords a means, at a small expense, of keeping out the cold air which usually leaks through the brickwork settings, and retaining a very large proportion of the heat of the furnace which is usually dissipated.

I quite agree with one of the other speakers, that the design shown in the figure on p. 348 does not, in practice, necessarily suffice to prevent smoke. There is a large power-house near me, the presence of which is evident by reason of what issues from its chimney shaft, although the boilers are fitted with the furnace shown. The other points that I want to touch on I will communicate in writing, as time is short.

(*Communicated.*) I am surprised that the authors have only made a passing reference to the firing of boilers from producers, as so many advantages would result that they must eventually be largely used, especially for large power schemes in colliery districts. An increase in efficiency as regards consumption, a decrease in capital outlay on plant, in attendance, and—in the almost entire absence of conveying plant, and the entire absence of mechanical stokers—in maintenance, would result from their use. And there would be no smoke. Each producer might supply a bank of six or a dozen boilers in much the same way that benches of retorts are fired from producers in a gas

Mr Ruthven Murray.

works. Of course such a scheme would only work at its best when the plant was in continuous operation, but in comparatively small power plants regenerative gas furnaces might be used for each boiler, and some of the before-mentioned advantages be secured.

The authors quite seriously refer on p. 338 to "the economy to be secured by employing cheap and uneconomical engines in getting over brief peaks in a lighting station." Although it is many years since this course was first proposed no engineer appears to have adopted the suggestion; and it is little wonder, for we all know that the plant doing the "peak load" to-day will be taking the day load to-morrow, and the investment would be a bad one, even if an engine builder could be found who would sell, or at any rate admit to selling, a "cheap and uneconomical engine."

It is further stated on p. 344 that "practical considerations render it necessary to add an excess of air" to ensure complete combustion of fuel. For this reason, and also because the addition of this excess of air, if cold, lowers the furnace temperature and reduces its efficiency so greatly, it would at least be as advantageous to use an "economiser" to heat the air supply to the furnace as to use one to heat the feed-water. Owing to air being such a bad conductor the difficulty appears to be to construct a suitable apparatus, although the experience with the plant at Sheffield shows what excellent results, and how great an economy can be obtained by attention to this point.

With regard to the use of superheated steam I have only the type of superheater the authors condemn, on water-tube boilers, but some of the results obtained in practice may be of interest. I have some engines which are of two-cylinder compound Ferranti type, built with the steam supply carried through the receiver, so that the steam entering the low-pressure cylinder should at least be dry. With an absolute pressure of 185 lbs. at the boiler the temperature was 480° F., showing that the superheat was 105° F. Outside the throttle valve of the engine (after the steam had passed through the receiver) the pressure was 115 lbs., and the temperature 390° F. or superheated 52° F.; in the low-pressure receiver the pressure was 30 lbs. and the temperature 250° F., or 0·4 F. above the temperature of saturated steam at that pressure. The above is an average result from numerous tests and shows the good which results from even moderate superheating. The steam pipes between the boilers and engines are short and very well lagged. Unfortunately it is not possible to make the tests suggested by the authors on p. 351 to prove what economy results from the re-heating in the receivers.

Mr. Thwaite.

Mr. B. H. Thwaite (*communicated*): The paper presented by the authors is full of practical suggestions. I sincerely congratulate them both on having produced a most excellent paper dealing with a most difficult and complicated subject. The attainment of a really satisfactory thermal efficiency from the combustion of semi-bituminous coal under the conditions of fluctuating power demands, can only be obtained by the displacement of the steam boiler furnace power installation with the combustion chamber associated with a gas engine installation in which the power units have varying increments of power potential,

Mr. Thwaite. so that at least a three-quarter load can be distributed to each gas engine unit. I notice that Mr. Booth has overlooked the blast furnace as a power source; no doubt he will remedy this omission in his reply. The specification that will secure a fairly satisfactory character of combustion for steam-raising purposes using hydrocarbonaceous coal should include the following clauses :—

The resistance to the flow of the secondary air supply into the combustion zone to be adjusted so as to balance satisfactorily the resistance established by the thickness of the fuel-bed on the grate.

The zone of combustion to have an environment of refractory material of such a constructional character that its surfaces become incandescent to a degree that will secure the ignition of the combustible gases.

The fuel, if hydrocarbonaceous or bituminous (to the extent of over 10 per cent. of volatile hydrocarbons), to be continuously fed on to the grate in small and well-distributed increments, so that the proportion of the evolution of the volatile hydrocarbons will be fairly constant.

It may be stated generally that combustion giving a slightly coloured effluent from the top of the chimney-stack is more efficient than is a colourless one secured by an excess of oxygen or air.

Mr. Taylor. Mr. A. M. Taylor (*communicated*): I would like to ask Messrs. Booth and Kershaw if they could corroborate a diagram (Fig. 10, published in the paper read by me before the Birmingham Local Section on December 14, 1904, on "Stand-by Charges and Motor Load Development"), which I have attempted to compile from the results obtained on a full-load test of a Babcock boiler? I have attempted to estimate the proportionate amounts of coal used for different proportions of the full-load output of the boiler by the following method :—Taking the known fact that a boiler which is doing no work of any sort, but merely maintaining full pressure, requires about 6 per cent. of the boiler feed which it requires on full-load; I take 6 per cent. of 73 per cent. (the heat imparted to water at full-load), which gives, say, 4 per cent. of the total heat in the coal at full-load for the no-load heat consumption (that is, coal consumption) due to this cause. Then I assume that the radiation losses of the boiler are nearly constant—and this is the point on which I would specially be glad of the authors' opinion—and add 6 per cent. more coal to the 4 per cent. already obtained, making 10 per cent. The remaining 5 per cent., composing the total of 15 per cent. for the no-load coal consumption, I estimate on the basis of the coal already accounted for, taking into account that the dampers are practically closed, and that the item of "imperfect combustion" is, therefore, proportionately greater than on full-load, though the other item of "heat carried off by the gases" is very greatly less.

Mr. Nicholson. Mr. F. H. Nicholson (*communicated*): I have had personal experience with a liquid fuel burner, and it is with regard to this special side I wish to make a few remarks. On p. 338 of the paper, near the bottom, there is a paragraph about liquid fuel reading as follows:— "Where coal is the fuel, the provision of a second supply will present difficulties that may be more serious than the provision of more boilers; but with a system of liquid fuel, the conveyance of the fuel to the

furnaces is a simple matter, and it may well pay to employ expensive liquid fuel during heavy loads of short duration, if capital outlay is thereby reduced together with rent and attendance." Mr. Nicholson.

Before going further it may be worth while to give some particulars of liquid fuel for those who have not had any experience with it. To obtain the best results with liquid fuel it is most important that the combustion chamber be made big enough, also that a proper air supply is arranged for. When these points are seen to, and provided also an efficient burner is used, the oil will be properly consumed, and no smoke will issue from the chimney, thereby the work obtained from one ton of oil will be double of that yielded by one ton of coal.

A few figures here may be of interest, in confirmation of my statement. I am sorry I cannot give figures for electricity works, but this is due to my not having them, as I do not know of any station where liquid fuel is used; anyway, I hope the illustrations I give below will give you some idea as to what can be done. The London, Brighton, and South Coast Railway fitted up a set of Johnstone's liquid fuel burners on to their engine Sheffield, which, I believe, is about twenty years old; the set was composed of two burners, which were found to be quite sufficient to do the work the engine was capable of doing. This engine did its usual work over a period of three weeks; the total number of miles run was 2,128, with an average load of 14 vehicles; the total fuel consumption was 26·26 lbs. per mile. This engine was also put on to run the express trains between London and Brighton with the following results:—From Brighton to London, with 15 vehicles, the fuel consumption was 20 lbs. per mile. On the return journey from London to Brighton, with 19 vehicles, the fuel consumption was 23 lbs. per mile. The London, Brighton, and South Coast Railway told us that the best they were able to do with coal with 12 vehicles was 43 lbs. of coal per mile. At Davidsons & Co.'s Sirocco Engineering Works a trial was made on a tea-drying plant with the following result:—Consumption of oil was 80 lbs. per hour, as against 200 lbs. of best steam coal; these two results were obtained on the same apparatus.

These figures will show for themselves what is the ratio of the saving in fuel consumption; it is, therefore, not out of the way to say that when oil is used in a good burner, half the fuel is required to do the same work in comparison to the best steam coal; of course with the inferior coals this ratio increases in favour of oil.

In dealing with the economical side of the question, when oil is used it is most essential that the question should be looked into beyond merely the question of comparative consumptions of fuel. When this side of the question is only considered, it will show as a rule, when the prices of oil are double those of the best steam coal, that there is practically no difference in cost of fuel. It is not a fair comparison to stop at this point; for example, with liquid fuel the reduction in labour in the boiler-house is about 4 to 1. In fact, it is quite possible for one man to look after between 12 and 15 boilers, provided the boilers are conveniently arranged for him to do this. Therefore, in looking into the economical side with liquid fuel, the following points should all be considered at the same time in comparing with coal or any other fuel.

Mr. Nicholson.

A list of the advantages, therefore, will be of interest, and will show how oil fuel is adaptable to various economies. (1) The ease with which oil can be stored and removed from point to point, thus doing away with a considerable amount of labour. (2) The furnaces can be fired mechanically; oil makes no ash or clinker. It also can be burned at maximum rate, or can be turned off entirely at any instant. (3) A very large power of boilers requires very little labour in stoking.

These are the general advantages. Further detail advantages are:— (1) Diminished loss of heat up the chimney, owing to the clean condition in which the boiler tubes can be kept, and to the smaller amount of air which has to pass through the combustion chamber for a given fuel consumption. (2) A more equal distribution of heat in the combustion chamber, as the fire doors do not have to be opened for stoking, consequently a higher efficiency is obtained; also unequal strains in the boiler tubes, etc., due to uneven heating, are also avoided. (3) No danger of dirty fires on a hard run, as fires do not have to be cleaned with liquid fuel. (4) Reduction in cost of handling, as this is done mechanically and by gravitation. (5) No firing tools required; consequently the furnace lining, brickwork, etc., last longer. (6) Absence of dust, ashes, and clinker, thereby cleaner boiler-house and more room for storage of fuel. (7) Oil does not deteriorate to the same extent as coal. (8) Ease with which fires can be regulated from a low to a most intense heat almost instantaneously, thereby a further saving of fuel can be got. (9) Lessening of manual labour in stoking; the proportion is about 4 to 1. (10) Greater increase of steaming capacity, the difference being about 35 per cent. in favour of oil. (11) Comparison in space taken up for storage is about 36 per cent. in favour of oil—that is to say, 1 ton of oil takes up 36 per cent. less space than 1 ton of coal. (12) No smoke from the chimney, thereby fines can be avoided.

When, therefore, all the above advantages are taken into account, I think that in the majority of cases oil fuel will show an economy over coal, when the prices of oil are double those of coal.

Mr. Booth's suggestion that boilers should be fitted up with liquid fuel apparatus, in order to convert over to oil during heavy loads, will probably show an economy. But it is advisable to make the fire-bars in such a way that they can be lowered when oil is to be used; in this way a far greater economy will be gained, as the oil will be burnt to its full advantage. I certainly do not think that it is a good plan to burn oil and coal together, as by doing this neither fuel can give out its maximum heat; one gets a sort of choking effect. No doubt an economy will be gained to a certain degree, but a far greater economy will be gained by lowering the fire-bars and burning oil only. It ought to be quite an easy matter to arrange the fire-bars in such a way that when converting from one to the other the bars could be raised or lowered mechanically.

I most certainly think that oil is the fuel for the future, and no doubt we shall see our electricity works run on even more economical lines than at present. I therefore hope electrical engineers will begin to look into this question of economy of oil fuel more seriously. As oil

is now being found in large quantities in California, thereby the prices will come down and enable oil fuel to compete with coal. Mr. Nicholson.

Mr. C. ALFRED SMITH, B.Sc. (*communicated*): I have read with much interest the paper by Mr. W. H. Booth, but there are two or three points upon which I would ask for further information. (1) Concerning the use of superheated steam, Mr. Booth seems to be in favour of using just enough superheat in order to get superheated steam at the point of cut-off. Might I suggest that in the case of engines that are not jacketed it would be better to have a degree of superheat to such an extent that the steam would be saturated at the toe of the diagram—that is, at the point of the exhaust, as this would help to do away with the evils of cylinder condensation? (2) At the summer meeting of the Institution of Mechanical Engineers I asked Mr. Hillier if he had any experience, or whether he believed in the use of reducing valves in order to obtain the advantages of superheated steam without any of the evils of superheaters. Mr. Hillier thought the suggestion a good one, but had no experience. I would ask Mr. Booth the same question, and whether he would recommend such a system which seems to me to have certain advantages for central station work. Especially is this the case where water-tube boilers are used. The idea is to generate steam at, say, 270 lbs. pressure, and to pass the steam through a reducing valve, which is really a wire-drawing apparatus, so that steam would be delivered at the engine stop-valve at a constant pressure below that of the boiler (say, 180 lbs. pressure) with a temperature of steam at 270 lbs. It seems to me that not only is this system a great advantage for thus obtaining superheated steam, but it allows for any fluctuations in boiler pressure, and thus relieves the governor of the engine of considerable work, because it keeps the pressure in the steampipe comparatively constant. As is, perhaps, not very well known, this system has been used for some years in the Royal Navy, where it is quite common to use boilers at the above-mentioned high steam-pressure. Critics will at once say that this may account for the great trouble with the Belleville boiler, but I have seen cases where there was no such trouble, and where high pressures were used. (3) I would ask Mr. Booth whether liquid fuel might not be used for the peak of the load. I believe that some destroyers have been worked on a system of combined coal and liquid fuel, and it would surely be possible to have a central station boiler so designed that in case of emergency liquid fuel might be used; and although this fuel might be more expensive under ordinary circumstances, it would probably pay in order to force the boilers during the peak of the load or in an emergency. It has also occurred to me that a steam-driven central station might well use some form of oil engine, such as the Diesel engine, so that any very light load might thus be taken up instead of lighting up the furnaces, and in the case of a bad break-down the engine might be run up for lighting the station. In the case of small stations with no storage cells it seems to me that such an engine would be very useful during times of repair, etc., and the time taken for starting it up is not great. I would beg to respectfully offer my congratulations to the authors of the paper. Mr. Smith.

Mr. Bayly.

Mr. C. F. H. BAYLY (*communicated*): The authors say at the top of page 332 that the efficiency of a boiler diminishes enormously as the thickness of the scale increases; this gives the impression that the consumption of coal would be excessive after three to six months' run without cleaning.

A well-known firm of boiler manufacturers say that $\frac{1}{16}''$ scale causes a loss of 13 per cent. of the coal burnt, $\frac{1}{4}''$ scale 38 per cent, and $\frac{1}{2}''$ scale 60 per cent.

In the paper by Messrs. Stromeyer and Baron, quoted by the authors, I find the following:—

> Very hard-worked boiler, $\frac{1}{4}$ sq. ft. heating surface per lb. of coal burnt per hour, with $\frac{1}{8}''$ scale wastes 11·6 per cent. of total fuel compared with that utilised by a perfectly clean boiler.
>
> Moderately-worked boiler, $1\frac{1}{2}$ sq. ft. per lb. of coal, wastes about $6\frac{1}{4}$ per cent. under same conditions as to scale.
>
> Lightly-worked boiler, 4 sq. ft. per lb. of coal, wastes 2·7 per cent.

They sum up by saying, "It may therefore be safely said that even a thick coating of scale does not materially reduce the efficiency of a boiler."

Result of tests on two similar Babcock boilers working an exceptionally steady load, at the average rate of about 6·3 sq. ft. of heating surface per lb. of coal per hour. On the occasion of one test the clean boiler generated 760 units on 3,789 lbs. of coal, and the dirty boiler 803 units on 4,141 lbs., showing an advantage to the clean boiler of about 3 per cent. At another test the clean boiler generated 919 units on 4,547 lbs. of coal, and the dirty boiler 905 units on 4,335 lbs., the weights of coal per unit generated being practically equal. In each case the dirty boiler had been working six or eight months, and had not less than $\frac{1}{8}''$ scale anywhere, and in parts it was considerably over $\frac{1}{4}''$ thick. This shows in a practical—though perhaps not dead accurate—manner that the loss through scale is not so great as is thought in some quarters. It seems, therefore, that the unqualified assertion as to scale enormously diminishing the efficiency is hardly correct.

Mr Bennis.

Mr. A. W. BENNIS (*communicated*): One of the experiments Mr. Kershaw made last week struck me as being particularly instructive if he had carried the conclusions to be drawn from it far enough. He boiled some water in an ice-cream paper for his whisky-and-soda, and triumphantly held up the paper showing that it was *not even blackened*, also showing that the Bunsen flame was smokeless. Now, if you can supply the right amount of air rapidly enough to a fire, and distribute it efficiently, you get smokeless combustion like the Bunsen flame, smoke not being dependent upon environment. As a matter of actual practice it is well known that it is comparatively easy to deal with the smoke of Lancashire boilers, given a good mechanical stoker and self-cleaning furnace, and a fireman with his head screwed on right; but it is most difficult with a water-tube boiler. The refractory furnace the authors show on page 341 may be good for smoke; it undoubtedly is, as we have used it years ago, but it reduces the steaming capacity and

Mr. Bennis.

the economical efficiency of the boiler. No radiant heat can reach the tubes, and though at first sight it would appear that after heating the brickwork up no further loss would occur, in actual practice there is an undoubted loss. It would seem that the low economy at the Kensington and Knightsbridge station was probably due to this cause. Then, when the load goes off, it takes many hours to cool such enormous masses of brickwork—another loss.

Electrical engineers are bound to make things pay. They have to take first cost, upkeep, and efficiency, all into consideration, and nothing that reduces efficiency and output in the end will prevail. A point often lost sight of is that the heat lost in radiation and waste gases is fairly constant, so that if the boiler is driven at its full output the losses from these causes remain about the same, but the relation to the evaporation goes down. This explains the very high efficiencies that Mr. Fedden has obtained at Sheffield with his marine boilers, Bennis's stokers with special hot-air supply to bars, and Ellis & Eaves' induced hot-air draught plant. The efficiency per cent. of five tests averages 80·5 per cent., the lowest being 77·6, and the highest 85·9, the percentage of CO_2 in the flue gases varying from 9·9 up to 13·0, and the highest efficiency was got at the lower percentage of CO_2 in the flue gas.

Working boilers at their full output also means that fewer boilers can be used, a reduction of first cost, as well as greater efficiency. Mr. Dickinson at Leeds gets an hourly evaporation of 16,000 to 17,000 lbs. of water from each of his 30 ft. by 8 ft. 6 in. Lancashire boilers, fitted with Bennis stokers and furnaces, on load. If he ran these at the boilermakers' rated capacity, he would need something like sixty boilers to do the work of thirty, or an increased outlay of at least £30,000, which at 5 per cent. equals £1,500 a year.

The author does not seem to be aware that in the intermittent sprinklers the grate is covered in not less than four complete sections, and only a very little is stoked at a time, so there is very little of the chilling effect on the fire explained by the author. And self-cleaning furnaces—the right ones, of course—do render grate-cleaning unnecessary, and nothing but clinker drops over the bar-ends. If the Vicars stoker, shown on page 345, were to be run with bituminous coal as depicted it would belch forth dense volumes of smoke. Vicars use a steam jet at the back of the bars to force auxiliary cold air in and stop smoke, as enough does not get through the fire. This stoker is a perfect smoke-preventer at medium loads. Half the trouble with smoke is caused by want of understanding mechanical stokers, and the disinclination of clean-handed engineers to go down to the stoke-hole themselves and get to the bottom of things. Use gas analysis apparatus by all means, but they sometimes apparently lie most abominably, unless the operator has had great experience in their use; but if you would take a rake in your hands, go over the fires, and see if they are covered at the back-ends of the bars, and if they are being kept the right thickness (if the fire is not so white hot that you cannot see what you are doing, but can only feel with the rake, it is not in a good enough condition), more money would be saved by attending to this one point than almost any

Mr. Bennis. other in the stoke-hole, and smoke would be much prevented. I never met a fireman yet who always kept this point right.

High Temperature Air Supply.—With cold air feed to bars we can already get a temperature of 2,500° Fahr., and when putting another 300° Fahr. into this, as at the new plant at Neepsend, we get a temperature of, say, 2,800°. The bricks and brick arches commence to melt at these temperatures and run like icicles, so that it seems impracticable to carry this further, and one hardly sees that you want to, for 80 per cent. of the heat units in the coal are transferred to the water and steam, and any further saving would be at a greatly enhanced first cost. I don't think the regenerative system suggested by the author on p. 336 would be at all practical. Imagine the enormous regenerator required to heat air from waste gases, leaving the boiler at, say, 500° Fahr., together with all the reversing valves, and other complications; the cost would be prohibitive.

I fail to see why the author has chosen the Coxe stoker as an ideal chain grate. The ordinary chain grate made by the Babcock and Willcox Co., and others, seems to be a better tool for English users. The Coxe chain grate was designed for anthracite small, and it was found that the small had to be graded very carefully, and the chambers underneath were made for the purpose of supplying a varying air pressure as the coal got thinner and thinner towards the ends of the bars; but with bituminous coal the conditions are quite different, and, as has often been the case, American machines designed to work with quite a different material from ours will not succeed here. I should like to ask the author why he considers the incline grate an ideal type of stoker? It is entirely unsuited for Lancashire type boilers to begin with, and in the second place, the gases would pass off in layers, and the smoky gas giving off the front part of the fire would creep up the brickwork, and get away as black smoke without being mixed with the more diluted air passing through the thin part of the fire at the lower part of the grate.

With regard to thermal storage, I have often wondered why, following the practice of numbers of paper-makers, dyers, brewers, and other large industries, large Lancashire boilers at a high pressure, and reducing valves to the engine, are not more used. You thus get an enormous reserve reservoir of heat, and are able to meet very fluctuating loads. A little paper-works I know at Sunderland put an "Auld's" valve on to their Lancashire boiler, and put out of service their Cornish, and eventually sold it, and saved a very large portion of their coal bill by so doing. There are such thing as automatic steam regulators, and these are used in many industries, but are almost entirely ignored by electric lighting engineers, though I think they would save them a great deal of worry and trouble. Lots of the mills in the north of England are fitted, and they work to half a pound or so of pressure, and are connected up, not only to the main damper between the economiser and chimney, but to control the driving of the stoker, and also the steam jets, if such are used.

There was one point in the Chairman's address to the Manchester Section of the Institution of Electrical Engineers, delivered in

November last, which is worth while taking to heart. "Experience seems to show that the quality of the labour required for the mechanical stoker is quite as high for hand-firing, but the quantity required is less." If best results are to be got it entails efficient supervision. Mr. Bennis.

AUTHORS' REPLY.

Mr. Booth. Mr. W. H. BOOTH (*in reply*): Several of the speakers on the previous occasion and this evening have argued upon the assumption that I am dead against water-tube boilers. That is entirely a mistake. I think I must take you back for a few years. About twenty-one years ago the Babcock boiler came over from America. When that boiler first came over, we—and when I say "we" I refer to the Boiler Insurance Companies in the North of England—we had, by dint of twenty-five years of hard work, succeeded in abolishing cast iron in connection with cylindrical boilers. I alone, I believe, had objected to putting water tubes into furnace or flue. When this boiler came over from America, except for the shell and for the tubes it was made of cast iron. We were told that the good American cast iron was best for boilers. We answered that mild steel was good enough for us. We were told that the circulation in a water-tube boiler was so rapid that there was no deposit of scale in the tubes. We objected, and found that it was not so; and the Babcock Company themselves have not pushed that point of late years, because they, very properly, make a water-softening plant. The Boiler Insurance Companies confined themselves almost solely to questions of safety. Some of us, however, went further than that, and advocated smokelessness and fuel economy. The furnace of the Babcock boiler, as of almost all water-tube boilers, is not suitable for burning bituminous fuels. I have no objection to the water-tube boiler; I do not altogether object to the Babcock boiler as it is now made, because it is made entirely of mild steel. One of the biggest Lancashire boiler makers recently said to me, "The Babcock boiler is a splendidly made boiler," and I said, "It is." But it was not so originally. For instance, it had punched rivet holes, and we objected to punched holes. We did not swallow everything that was thrust upon us in those days simply because it had come from beyond the shores of this country. When Colonel Crompton says that I have advanced and come round to the water-tube boiler, I am rather inclined to say, "Has not the water-tube boiler come round to me in all except the furnace?" The furnace is still generally made on what I consider a wrong method; yet there are furnaces with water-tube boilers—(and I specially point them out in the paper)—that are perfectly smokeless; there are some at Kensington and Notting Hill, and Col. Crompton has one at least in his works at Chelmsford; and if any one will compare that furnace with that of the boiler next to it, he will see the enormous difference there is between their behaviours. As regards chain-grate stokers and mechanical stokers generally, I have had seven years' more experience of mechanical stokers than Mr. Rosenthal, and I am sorry to say I know that mechanical stokers are not a cure for smoke. The chain-grate stoker may be made smokeless, if you let in a sufficiently excessive

Mr Booth. amount of air, as also the under-fired Lancashire boiler, for example. The water-tube boiler furnace will not be cured simply by putting in mechanical stokers unless you are content to have a very small percentage of CO_2 manufactured, and that, of course, is a loss; but otherwise, as we stated here in the paper, the water-tube boiler is better in some respects than the Lancashire boiler, because you can put a better furnace to it; you can put one of these outside furnaces, such as is described in the paper, and you may get smokelessness. Some of the speakers to-night have referred to the illustration on page 348, and said that we put that forward as a furnace to consume smoke, or prevent smoke. We do nothing of the sort. We put that forward simply to show a form of inclined grate which helps to prevent an excessive amount of air getting into the fire. This is a picture of a stoker which is not in use in this country; it is an American stoker. What we put forward as a very good form of furnace is that on page 341. That is working, I believe, perfectly. Another speaker this evening raised a good many objections, all of which he will find fully replied to in the paper. For instance, he said that we slighted batteries. We say nothing of the sort. We say that if you can get a good accumulator battery you have more economy than you can get with all the care you take of your boilers or your steam plant. There is nothing like a battery if you can get it to last. We merely threw out a challenge to the battery people

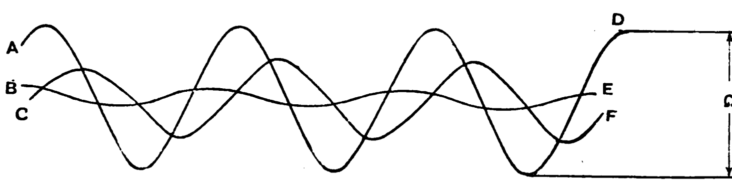


FIG. D.

to come forward and say, "We have a battery that will last so many years." We throw no slight whatever on batteries; I am a great believer in what they ought to do, at any rate. Then the same speaker said we make no reference to the special firing of express boilers. We do make reference to that. We say that the express boiler with liquid fuel can be used to get over the peaks of the load, but liquid fuel is expensive, although I do not think it would be expensive if used for the higher peaks of the load. You could put the boilers on and off, and they could contain no water, or very little water. With regard to water-tube boilers, I am connected with a water-tube boiler which is as far in advance of any water-tube boiler as the present water-tube boiler is in advance of what it was itself twenty-one years ago. There is no necessity in order to keep an engine warm to have a leaking stop-valve. It seems to me that there are such things as by-pass valves—I have heard of them. Colonel Crompton thought we were very moderate with regard to the temperature of superheating. Unfortunately I am connected with a superheater which will give a high superheat more safely and more durably than any other superheater, so that I am astounded at my own moderation.

Mr. Booth.

Assume that you have a hand-fired boiler; you get a range of temperature in your flue like the curve A B in Fig. D. If you have very thin tubes in your superheater, naturally you get the temperature of those tubes following very closely the range of temperature in the gases. But if you take a very heavy and thick tube you get a distinctly less irregular temperature curve C D; you get a difference of phase, but even that is not sufficient to control the superheat properly. Therefore we put inside ours a small water tube, through which we run water as pure as possible, after it has been softened by the heat of the boiler, or by a chemical softener, and instead of getting a curve like C D, we endeavour to get a curve more in the form of E F. It is evident as regards very high superheat that our modification has something in it, because if you take the formula $3Fe+4(H_2O)^{900}$ F., you get Fe_3O_4 plus 8 of hydrogen; that is to say, you get a dissociation effect. This Fe_3O_4 is oxide of iron on the inside of the tube.

We control the temperature in the superheater in order to get a steady superheat in the engine, to prevent it from coming to grief. Assuming that E F in Fig. E is the highest possible temperature your tubes can stand, if you have a range of temperature G in the gases, you must evidently keep your mean temperature much below the maximum temperature which you dare use; but if you get a temperature range in your superheater more like H, is it not evident that the mean temperature of the superheat is the straight line drawn

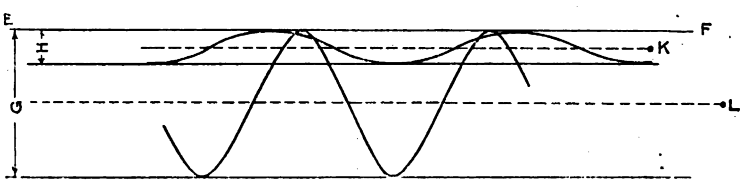


FIG. E.

through K, whereas in the other case it is a straight line drawn through L, so that instead of getting a mean superheat L, if you get a proper superheat control, you get a temperature K almost up to the maximum temperature that you can stand. I have always laid that down for ordinary practice as about 500° F. in the cylinder, and I should not like to recommend anything further at present. We cannot alter the chemistry of water and iron, I am afraid; and there are practical limits beyond which we cannot go. I will reply in writing to the other criticisms that have been passed on the paper.

I quite agree with Colonel Crompton on the value of high-feed temperature. In superheater practice the water-pipe referred to as controlling the temperature of the superheat is made to do duty in heating feed up to boiler temperature, and we find an unmistakable advantage from thus confining the boiler to evaporative duty. Nor do I advocate waiting for reasons before employing proved advantages. By a suitably constructed external furnace, a Lancashire boiler end-plate would not be damaged. It would be kept clear of the fire.

With regard to the vexed question of cylinder condensation, I

Mr. Booth. would point out that the leakage so much emphasised by Professor Nicholson is simply due to the mechanical action of the sliding parts which ride on a film of water, and push it towards the locus of least pressure. But this leakage is not so much a cause of loss as an effect of an already suffered loss. The water is first produced by cylinder condensation, and then it is made to leak. Abolish cylinder condensation and the leakage of water will stop. Abolish leakage if you can, but you will not abolish cylinder condensation. Superheat is not an economy because it stops leakage, but because it stops the cylinder condensation. If Professors Nicholson and Callender deny initial condensation, then I must entirely dissent from their findings.

With steady loads we are already down to the 1·5 lbs. of coal per h.p.-hour with controlled superheated steam, which Colonel Crompton thinks ought to be attained.

If I understand Mr. A. M. Taylor correctly, his method of finding the fuel for different proportions of load seems to be practically correct. The stand-by losses proportion, so far as regards radiation, will not vary much at full-load or no-load so long as the boiler pressure is constant.

Replying to Mr. Thwaite, while recognising the importance of blast-furnace gas, the subject would have added unduly to the length of this paper. But there are some two millions of horse-power going to waste in this country which could very well be utilised in traction work on railways, on canals, if we allowed these to exist, and in chemical industries and electro-metallurgy. We might at least learn from the Germans and the Belgians to use blast-furnace gas and not waste it. It fell to me to make a test on the first engine ever worked with blast-furnace gas. It was Mr. Thwaite's own pioneer installation. That engine is running yet, and the results I obtained at that time (1896) have been corroborated by all subsequent work, showing that Mr. Thwaite's calculations and estimates were wonderfully accurate.

Mr. F. H. Nicholson, while substantially correct in what he says of liquid fuel, is, I fancy, too optimistic. In preparing my book on liquid fuel and its combustion, my researches showed me that the world's production of petroleum was a mere fraction of the coal production. Hence my conclusion that liquid fuel should be used for the sharp peaks of load. The United States Naval Liquid Fuel Board object to mixed coal and oil firing, but Mr. Holden seemed to find it satisfactory in locomotives, and M. Bertin seems to say that solid and liquid fuel, burned together, have actually greater efficiency than when burned separately. They appear to require less excess of air, the liquid fuel finding its oxygen to some extent in the gases from the coal-fired grate below. Mr. Smith's question on superheat is practically replied to in the reply to Colonel Crompton, and any further reply might best take the form of a question. Will an initial temperature of 500° F. be usually existent after cut-off?

The use of reducing valves is not to be advised. The effect of wire-drawing is very small, as may be found by comparing the total heat of steam at the initial and at the wire-drawn pressure. Such wire-drawing will not even evaporate the moisture in ordinary steam. I have already

in the paper specially indicated liquid fuel for peaks of load, and at present would not go beyond such peak duty. Mr. Booth.

As regards the diminution of efficiency of a boiler from scale effects referred to by Mr. Bayly, it may be pointed out that the diminution of efficiency of one part of the heating surfaces raises the efficiency of other parts, so that the full effect of scale is by no means so great as a mere comparison of two areas clean and dirty under the same conditions, apart from scale, would indicate. Then in a Lancashire boiler, for example, scale usually shuns the most effective surfaces and settles in the least effective. Mr. Stromeyer may be read with profit on this point, as Mr. Bayly intimates.

Mr. Bennis, in my opinion, is wrong in that his Bunsen effect is not attained, nor can he abolish smoke with his sprinkling stoker *per se.* He is also wrong in what he says about refractory furnaces, and I hope he will permit me to doubt his figure of 85·9 per cent. efficiency. Mr. Bennis, of course, holds a special brief for sprinkling stokers, but though he assumes I do not understand their mode of action, he ought to be aware that no matter how he puts on coal, it will produce an aggregate cooling of the fire equivalent to the latent heat of gas distillation. The Coxe stoker was shown as an illustration of air regulation. I am sorry Mr. Bennis has so little opinion of air regulation. Also he does not appear to grasp the gravity effect in closing a fire as it was intended to be grasped, and he speaks of gas coming off in layers as though he did not understand the use of an arch to prevent this; and then he finds fault with an inclined grate because it cannot be put inside a Lancashire boiler, yet I have seen inclined grates inside these boilers. It was only suggested in the form illustrated for externally fired boilers. Nor is it clear why Mr. Bennis, who believes so strongly in covering a grate with fuel, should go out of his way to praise the common type of chain grate which cannot be kept covered evenly, nor covered at all without a gas-producer pit behind it to finish off the unburned fuel if the grate travels fast enough to preserve some cover of fuel. If a sprinkling stoker has a virtue it is that, when it can be induced to give even spreading of fuel, it will keep a grate covered, and a really good sprinkler, did it not fill the flues with dust, would be something to secure, and might be worth trying for. But no sprinkling stoker can avoid the defects of its qualities, and Mr. Bennis must be able to show that he has a sprinkler stoker which does not result in an excessive amount of dust in the flues before he claims too much for this type of machine. He assumes my ignorance of the action of his type of stoker, and thereby he displays prominently that he is not aware of the very special and peculiar opportunities that fortune has placed in my way of becoming particularly acquainted with the vagaries of other types of stoking machines as well as of his own.

Mr. Halpin has added a valuable section to the discussion. As regards the practicability of controlling superheat by an internal water tube, I need only say that after six years of continuous work we can say no joint has failed and no trouble has been experienced from deposit, for the circulation water is heat-purified in the boiler itself if not purified earlier. I am very gratified to have Mr. Halpin's endorse-

Mr. Booth. ment of the importance of stage heating of a working fluid in practice.

Further, in reply to Mr. Rosenthal, I must adhere to the statement that in superheat calculations we cannot assume that the superheat will endure beyond the point of cut-off, and therefore no thermo-dynamic gain can be secured. I fail to follow Mr. Rosenthal at this point, for I certainly do not deny the coal economy due to superheat, and, in a recent instance, when we were able to guarantee an economy of 15 per cent., we did actually secure 25 per cent. by a controllable superheater, but it was not done thermo-dynamically but purely by reduction in cylinder condensation, etc. Since the meeting I have had to test a chain grate, and found it only produced 6½ and 7 per cent. of CO_2 entirely owing to the excessive volume of air admitted through the burned-out parts of the fuel.

I really cannot permit Mr. Rosenthal to remain uncontradicted in his reference to the custom of burning only 6 lb. of coal per square foot of grate per hour twenty years ago. The regular rate recognised in the textile districts was 21 lb. per hour, and I have known where three Lancashire boilers, 7 ft. diameter, with 33 square feet of grate surface each, consumed 75 tons in a factory week of 56 hours, including banking. To talk of 6 lb. is absolutely misleading.

Mr. Holland omits to note that in regard to express boilers for peak loads the fuel recommended was liquid, which certainly obviates the necessity of excessive fire-room staff. It would be of interest did Mr. Holland say what he means by proper working of a chain-grate stoker. Considering the importance of the subject, it would be useful to know how he prevents air getting through the bare parts of the grate. No one has yet done this without special devices, and I have watched chain-grate stokers as long as I can remember. I am much afraid that the station engineer looks on smokelessness as everything. Perhaps Mr. Holland's boiler cleaners find a higher percentage of CO_2 than facts would bear out. Mr. Holland also draws, quite unwarrantably from Fig. 8, erroneous conclusions. It was put forward as a grate, not as a setting. An inclined moving grate is the only grate which has the property, by aid of gravity, of causing the burning fuel to consolidate upon the end of the grate so as to prevent the otherwise inevitable inrush of air in excess through the bare grate. It seems to me to be arguing against the plainest facts to suggest that any grate which burns bare of fuel can possibly be satisfactory. The grate of Fig. 8 aims at *ideal* conditions of grate, and it is merely begging the question to assume that this Fig. 8 shows the setting we advocate in the face of the Fig. 2 which we put forward as embodying the principles we do advocate. Mr. Holland has evidently looked at the pictures and failed to read the text. His system of making an engine warm by the aid of leaking stop-valves is, I admit, quite consistent with the other practices named, viz., analysis by boiler cleaners, but it does not seem to me to be capable of being included under the head of good maintenance. A by-pass valve is in no sense the equivalent of a leaking stop-valve which never ceases to waste steam. That it should be considered so seems to support one speaker's references to methods of barbarism.

Mr. Booth.

In reply to Mr. Molesworth, the use of coal in the state of fine powder seems to be the correct use. Air mixture is more easy to regulate, and everything appears favourable if only the difficulty can be overcome of filling the flues with dust and distributing it over the surrounding area. Coal can be ground for 1s. per ton, and it must contain at least 20 per cent. of volatile matter or it will not fire. The comparative tests given do not, however, bear much value, for the hand-firing results are excessively poor and the dust tests very mediocre,

In reply to Mr. Gaster, it can hardly be said that there is any firebrick on the market that is really first class. One hears of over-arches enduring at most for a year. Siloxicon and other products of the electric furnace will probably come into more extended use. Meanwhile no firebrick ought to be used that has not been thoroughly dried for a long time. Far too many arches are built of weather-soaked bricks and fired at once, and ruined by such hasty work.

In reference to Mr. Mordey's strictures on the poor efficiency of steam plant, he should remember that in the North of England we are producing power and heating a mill also for 1½ lb. of coal per h.p.-hour by the aid of superheated steam and a fairly steady load, and there is only wanting a reliable accumulator and reasonably intelligent steam engineering to enable equivalent results to be secured in electrical work, which suffers so much loss as a result of poor load-factors, for which the accumulator presents the sole remedy. Mr. Mordey almost alone has endeavoured, by the use of gas power, to get higher efficiencies from the fuel, but here again let it be pointed out that blast-furnace gas has been available for over ten years, within easy reach of some half-dozen large power-stations in Wales, the Midlands, and the North, which most readily come to mind, and no engineer responsible for these stations has been able to feel himself capable of grasping the fuel economy that such a user of gas would secure. Is it that we can neither copy nor learn from the foreigner who is reaping the benefit of this English invention of Thwaite?

The objection raised by Mr. Thomson has some force so far as regards starting boilers up from cold. One way is to use excessive air until the first batch of coal has been coked, and it might be possible to start up with coke to some extent. Anyhow, it is better to use an excess of air for a few minutes than to use it all day. One method of getting rid of old scale is to boil up and cool down a boiler once or twice with a powerful dose of soda. Mica in natural rock seems to produce a weak texture, and perhaps has that effect in the case of scale. I would like to suggest to station engineers who have scaled boilers to scrap that they should try the effect of a 10 or 20 per cent. solution of hydrochloric acid in removing scale. Probably the acid would leave the plates alone so long as any scale remained to be dissolved. If the acid does not injure an old boiler it might be then trusted in new boilers. Old scale will also come loose in time if treated with softened water. The locomotives on a railway using water from a chalk well have been cleared of scale by changing over to a well in the Tunbridge sandstone.

Mr. Booth.

While it is correct that glazed bricks retain heat, as Mr. Ruthven Murray states, the coolness of a porous brick boiler wall probably arises from the inflow of air which carries back outflowing heat into the flue. It is, I believe, impossible for him to have seen smoke from a station near his own fitted with the stoker of page 347, for I am told there is not one in this country, and I must again point out that several speakers might have read the last paragraph on page 346 before jumping to the conclusion that Fig. 8 was put forward as a *setting*. Mr. Murray's reference to page 338 is equally unfortunate. We specially deprecate the idea of economy from cheap engines, but at the same time if the cheap engine did not imply more steam generating plant there ought to be no difficulty in a station with the 12 per cent. load factor of most lighting stations in apportioning the engines to fit such conditions. A cheap uneconomical engine need be very little else than a high-class economical engine overloaded, and therefore cheap during the period it is worked at late cut-off.

As regards reheaters, I hope some day to be able to show the benefit or otherwise of a reheater in which all the steam flowing through the reheater is passed directly back to the boiler. We hope in this way to effect reheating with economy.

Mr. Kershaw.

Mr. J. B. C. KERSHAW (*communicated reply*): Mr. Booth has dealt very fully with the greater number of the criticisms of our joint-paper, so far as these relate to furnace construction, mechanical stoking, superheating, and air-leakage losses. It is only necessary, therefore, for me to deal with the criticisms to which Mr. Booth has not replied, relating to (1) the chemical tests required in a well-managed boiler-house; (2) the heat conductivity of boiler scales; (3) the maximum temperature attainable in boiler furnaces; (4) the methods of preheating the air supply of boilers.

As regards the subject of chemical testing, Mr. Boot has stated that electrical engineers have not time to carry out all the testing described in the paper; and two other speakers—Mr. Rosenthal and Mr. Holland—have stated that no special expert training or knowledge is required for this work. To quote the former: "To analyse flue gases, coal, and water is a very simple matter"; while Mr. Holland says: "We have a certain number of men who have been trained to this class of work in our boiler-cleaning gang"; and the context shows that "this class of work" includes chemical and other tests of the waste-gases. My reply to Mr. Boot's criticism is that we do not, in the paper, advocate that shift engineers should be employed in chemical-testing work. What station engineers should do, is to appoint a specially trained man to take charge of all such work, or resort to outside help in the matter. That chemical work of this kind pays is absolutely proved by experience. Messrs. Crossfields, of Warrington, have a chemist (Mr. Dale) who devotes himself exclusively to chemical testing in connection with their steam-raising plant, which is not so large as that found in many electricity works, and they have sent him to take part in this discussion in order to corroborate what we have said on this point. The appointment is not a new one, and the work has now been carried on for some years. Would Messrs. Crossfields continue this appointment if it did

Mr. Kershaw.

not bring in some return? Personally I have to thank Mr. Dale for a most useful contribution to the discussion on this paper.

With regard to Messrs. Rosenthal's and Holland's remarks, one can only attribute them to ignorance of the real difficulties of obtaining accurate and reliable results in this branch of technical analysis. No doubt, if one purchases a calorimeter and places it in the hands of an engineer, he will get some results with it. The point is—are the results accurate and reliable? The same criticism applies to tests made with the CO_2 testing apparatus. Any one—even Mr. Holland's trained boiler-cleaners—could use it and imagine they were making tests. The real point is—are tests made by such men of any value? As a chemist who has had considerable experience of this class of work, my answer is in the negative. I can guarantee Mr. Holland that most educated chemists who know anything about gas-analysis would tell him that results obtained by his boiler-cleaners were not worth the paper upon which they were written. I am pleased to note that both Mr. Dale and Mr. Druitt Halpin support the authors in contending that specially trained men are required for this work. The latter referred to a case in which untrained assistance in this work led to a collapse of the boiler tubes.

I can prove how easy it is for men lacking this special training to go wrong in such matters, by quoting some calorific results obtained by one of the best-known electrical engineers in the North of England in his first experiments with a fuel calorimeter. The calorific value of the best coal is between 13,000 and 14,500 British thermal units. This gentleman obtained a whole series of tests ranging from 20,000 to 25,000 British thermal units per lb. of fuel burnt, and actually put these figures into circulation amongst his friends. However, I have some hope of both Mr. Rosenthal and Mr. Holland learning by experience, for I note that the latter admits that expert attention is required for keeping an Ados recorder in order. Doubtless in time he will discover that it is easier to get incorrect than correct results with other forms of testing instruments, including calorimeters, and that an incorrect result in tests of this kind is worse than no result at all.

With respect to Mr. Bayly's remarks on boiler scale, I admit that it would have been more correct if we had inserted the words "It is the generally held opinion" before the statement at the top of page 332. Most authorities on steam boilers, with the single exception of Mr. Stromeyer, have reiterated the opinion that scale, and especially oily scale, was a very bad conductor of heat. As pointed out by Mr. Booth, in his communicated reply, Mr. Bayly's figures from a practical steam-raising trial are inconclusive on this point, since the heat passes through the plates into the water at some later stage of the progress of the flue-gases along the flues, even when a boiler is heavily scaled. The apparatus for testing the heat-conducting ratios of scales and boiler plates, which I exhibited at the last meeting, is designed for clearing up this point. Although only few tests upon scales have yet been made with it, these tests indicate that the popular ideas upon the subject are incorrect, and that scale is not such a bad conductor of heat as is generally supposed. Detailed results of tests made with this apparatus will be published at a later date.

Mr. Kershaw.

Another aspect of the effects of scale deposits upon the steaming capacity of a boiler is raised by Mr. Druitt Halpin's inquiry respecting our remarks upon the utility of zinc rods inside a boiler. My reply on this point is—that the statement criticised is quite correct. A perfectly clean, smooth, and flat metal surface does not facilitate the formation of steam bubbles, and a boiler, the plates of which are covered with a rough lime deposit or have zinc rods or plates riveted to them at certain places, will produce steam more easily and regularly than one which is perfectly clean or is unprovided with any metal projections. The reason is, that points and projections in a mass of boiling water serve as points of attachment for the originally very minute steam bubbles, and that these increase in size until their buoyancy carries them up and away from their first support. Chemists frequently make use of this principle when boiling liquids in glass vessels in the laboratory, and drop into the beaker or flask a small coil of platinum wire, or a bent piece of platinum foil, in order to facilitate the formation of steam bubbles and to avoid "bumping."

I now come to the criticism by Mr. Druitt Halpin of the figures given for the theoretically possible temperature attainable in boiler furnaces by the combustion of fuel. The difference between Mr. Halpin's results and those given in the paper is caused by the different values taken for the specific heat of the exit gases. Mr. Halpin appears to have used the value 0·240 in obtaining his figure of 4,400° F. Since these exit gases are a mixture of air, carbonic acid, unburnt hydrocarbons, and aqueous vapour, and there is reason to believe that the specific heat increases as the temperature rises, Mr. Halpin's value of 0·240 is too low, and his maximum temperature calculation is thus rendered incorrect.

Finally, in reply to the criticism of Mr. Bennis upon the proposed methods of extracting the heat from the waste gases for the purpose of preheating the secondary air supply, I may state that the plan described on page 336 of our paper is in daily use in practically every glass works in the country, and that the interest and the cost of the regenerative portion of the furnace plant is covered many times over by the higher efficiency obtained from the furnaces. As pointed out more than once in our paper, refractory linings to furnaces that will stand temperatures up to 4,000° F. must be employed before such a plan is practicable, but the electric furnace products now being manufactured at Niagara Falls, and at other places, are likely to solve this problem of finding suitable refractory materials.

In conclusion, may I thank Colonel Crompton, Mr. Dale, and Mr. Druitt Halpin for their very interesting and valuable contributions to the discussion on this paper, and may I express the hope that the general level of electrical engineer's practice with regard to the scientific control of their boiler plants, is not represented by Mr. Holland's use of his boiler-cleaners for carrying out all the testing work required, and of leaky stop-valves for warming his engine cylinders? True progress will not be attained by advance along that line of economy, whatever the apparent gain in trouble and in salaries may be.

The President.

The PRESIDENT: Before moving a vote of thanks to the authors for their paper, I would like to say in reference to it that the subject of fuel economy in a station, which has really formed the principal subject of the discussion, is very important, and will no doubt contribute to solve the question as to how that low efficiency of 6·3 per cent. can be raised a little. To my mind there could not be a better means of improving the working of boilers than the various instruments—there are two or three on the market—which have been devised to give a continuous record of the percentage of CO_2 in the chimney gases, especially if the stokers have an opportunity of observing for themselves how the firing is going on, and what success is attending their efforts. I am sure that will go a long way towards diminishing the coal bills of central stations. I now propose that the meeting should accord a hearty vote of thanks to the authors for their interesting paper.

The resolution was carried with acclamation.

Proceedings of the Four Hundred and Seventeenth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 26, 1905—Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on January 12, 1905, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

Geo. H. Clapham.
Arnold Greaves Hansard.
Sydney Geo. Leech.
Frederic Pooley.
Alexander B. Randall.

From the class of Associates to that of Members—

Charles Jones.

From the class of Associates to that of Associate Members—

Evelyn Fawssett.
Donald Seaton Munro.
William D. Kirkpatrick.
Ayton Herbert Read.

From the class of Students to that of Associate Members—

Ewen McKinnon Kerr.
John Milton.

Messrs. C. O. Grimshaw and J. Fiddes-Brown were appointed scrutineeers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Associate Members.

Henry Herbert Couzens.
Alfred Crowther.
John Nevius Dodd.
David B. Ingram.
Baron Augustus Mannerheim.
William Henry Moore.
Arthur Seymour Munro.
Alfred Peaker.
Frederick K. Preston.
Thomas Bertram Reader.
Thomas W. Sampson.
William T. Taylor.

Associates.

Lancelot H. A. Shadwell.
Thomas Wadsworth.

Students.

William Aked.
Sidney Mottram Andrews.
John T. Appleton.
Albert Auer.
Herbert A. Bastable.
George C. Blackburn.
Frederick G. Broadhead.
Samuel Brooks.
Thomas Alfred Brown.
William Browning.
Richard Patrick M. Canty.
Henry Ambrose Carney.
Laurence Hudson A. Carr.
Arthur Edward Clarke.
George William Cosby.
Purushottam Ganesh Dãni.
Robert Ernest Dawson.
Alfred J. C. De Renzi.
Cecil Holmes Dobson.
George Ayerst Egles.
Charles F. Embleton.
John Gilbert Farrow.
Reginald A. F. Fawcus.
Herbert A. Finlaison.
Dundas Fox.
John Wesley Fraser.
James Parker Garner.
William W. Greenwood.
Clayton H. W. Grimshaw.
John W. Hambleton.
Cecil Claud A. Hardie.
Arnold W. Elsmere Harris.
Trevor Hedberg.
Harald Helweg-Larsen.
Percy Harry I. Humphreys.
Philip Kemp.
Ernest William Lace.
Edmund Lawson.
John Hughes Lemon.
Lionel John Lepine.
Arthur E. McConnell.
Robert Loudon McCulloch.
John Stanley Mail.
Alick Maling.
Silas Young Maling.
Charles Edwin May.
Edward Nanney G. Morris.
John Harold Mousley.
John Hulse Machin Nichols.
Albert Edward O'Dell.
William Arthur Perry.
Kenneth Preston.
Edgar Spencer M. Prichard.
Victor James Radbone.
Henry Sidney Rainforth.
Norman Ramsey.
Arthur Riley.
John Massey Seddon.
Sidney Simpson.
John William Smith.
Noel Banister Tomlinson.
Edward Wm. Tunbridge.
Wesley Turner.
George Wade Wadsworth.
Thomas Frederick Wall.
John William Wheeler.
Stanley Mackay Wright.
Anthony R. Wuest.

Donations to the *Library* were announced as having been received since the last meeting from Mr. H. Armagnat; to the *Building Fund* from Messrs. A. Burton, H. C. Channon, G. N. Cheesman, S. S. Grant, J. T. Haynes, W. B. Marr, J. T. Morris, D. S. Paxton, F. P. Seager, J. M. Smythe, and H. W. Young; and to the *Benevolent Fund* from Messrs. J. Bailey, F. G. C. Baldwin, A. H. Bate, G. N. Cheesman, Dr. C. V. Drysdale, H. C. Gunton, H. G. Harris, J. T. Haynes, W. N. J. King, C. Stirling, A. A. C. Swinton, J. Woodside, M. S. Chambers, K. Edgcumbe, E. P. Harvey, Colonel H. S. Hassard, C. C. Hawkins, Lord Kelvin, A. E. Levin, E. Manville, W. M. Mordey, W. H. Patchell, W. J. S. Pyper, S. R. Roget, A. Siemens, A. J. Stubbs, G. G. Tomkins, A. P. Trotter, and H. J. Wagg, to whom the thanks of the meeting were duly accorded.

The discussion of Messrs. Booth and Kershaw's paper was continued (see page 357). The meeting adjourned at 9.40 p.m.

MANCHESTER LOCAL SECTION.

COMPENSATED ALTERNATE CURRENT GENERATORS.

By MILES WALKER, B.A., Associate.

(Paper read at Meeting of Section, Nov. 29, 1904.)

Before entering upon a discussion of the methods that have been proposed for compensating alternate current generators, it is well to state the main ends which such methods should have in view; in other words, what kind of regulation do we wish to obtain? This depends, in some measure, upon the work which the machine is put to. We may divide the cases in which alternators are employed into the six following heads :—

(1) Large power transmission schemes, where power is transmitted from a cheap source to large and small consumers, some near by and some far away, who draw current at any power-factor.

(2) Large power stations in towns, where the bulk of the load is for lighting, and where power customers are not allowed to make large draughts at low power-factors.

(3) Large power transmission schemes for some single purpose, as, for instance, the supply to an electric railway, where the bulk of the power is converted into direct current by apparatus whose power-factor is high.

(4) Large factories using many motors and some power for lighting.

(5) Power transmission to a few induction motors, as, for instance, those required for haulage gear.

(6) Small installations for mixed lighting and power.

In cases (1), (5), and (6) very good regulating generators are required, because of the sudden demands that may be made at low power-factors. In (5) and (6) these demands may be great in proportion to the size of the generators. In (1) the sudden demands will not be so great in comparison with the whole load, but the customers for the lighting power will be affected by small changes in the voltage, and the drop in the line will aggravate the variation in voltage.

In case (2) a good regulating generator is required, notwithstanding the high power-factor of the load, because at lighting-up time the load comes on suddenly, and, however great the station may be in comparison with the individual consumer, the incandescent lamps will be affected if the generator does not regulate well.

Case (3) is the easiest one to meet, for the load is taken at a high power-factor, and, however large the sudden demands may be, they are small compared with the total load on the station; moreover, small variations in the voltage will not call for complaint.

In case (4) the disturbance to the lighting is likely to be great when motors are started up, unless a generator of fair regulation is employed.

In all the above cases good regulation is desirable; in (1) and (2) it is most important. The generator to regulate well must respond instantaneously to the demand. If, when the load is thrown on, the voltage falls and then rises slowly by some automatic means, as is the case with most compensated generators, the machine does not fulfil the requirements. In cases (1), (2), (4), and (6) such a generator would be of very little more service than a machine of the simple type, whose voltage could be brought up on a rheostat, and, in practice, would not operate as well as a non-compensated machine of good inherent regulation. The compensating mechanism, if it is to be of real service, must be instantaneous in operation. It is for this reason mainly that compensating machines are not used in large generating stations to-day. Another reason is that these machines, when run in parallel, will have large cross currents flowing between themselves, and will tend to "hunt" unless further complications are added to steady the load on each machine.

In all large modern power plants the old simple type of field magnet is employed, and the tendency (particularly in cases 1 and 2) is to call for better and still better inherent regulation. This can only be secured by making the ampere turns on the field magnet great, relatively to the ampere turns on the armature, and good regulation in this way can only be secured by increasing the cost of the generator. Roughly speaking, a 6 per cent. regulating generator will cost 1·5 times as much as a 12 per cent. regulating generator of the same output.

It is not right to judge the regulating qualities of a machine by the rise in voltage which occurs when the load is thrown off. The criterion, from the user's point of view, is the percentage fall from normal voltage when the load is thrown on. Consider cases (1) and (2), for instance: A machine is running lightly loaded, and considerable demand for power is suddenly made. If the regulation is not of the right kind the lights will flicker perceptibly. It is only of secondary importance to the user to know that when the load is thrown off the voltage cannot rise very high. I wish to emphasise this point particularly, because of late years there has been a tendency among manufacturers to define the regulating qualities of an alternator by stating the rise in voltage when the load is thrown off.

If we are to have compensated alternate current generators in the future, they must not only be instantaneous in their operation and run well in parallel, but they must be free from complication and from gear that requires attention. One of the great advantages which an alternate current generator has over a direct current generator is in the absence of a commutator. The addition of a commutator will always be

regarded as a backward step, unless it can be shown that it is free from sparking, is inexpensive and permanent in its nature. Another important requirement is the applicability to steam turbine-generators. Most of the large steam-driven alternate current generators in the future will be turbo generators. The commutator is not a desirable adjunct to a high-speed machine.

We will confine our attention to synchronous machines, for these hold the commercial field of to-day, and are likely to do so in the future. The subject of asynchronous generators is too large to touch upon in this paper, and the methods of compensating them would require a paper to itself. The table on page 405 gives a classification of the various methods of compensating synchronous generators, and shows how they are related to each other. Most of the methods operate by changing the exciting current.

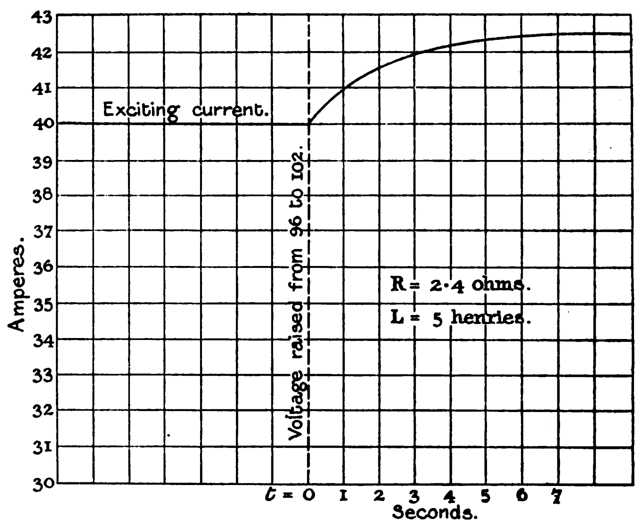


FIG. 1.—Behaviour of exciting current when exciting voltage is raised.

One method which I will describe in this paper preserves the field excitation constant, and utilises the armature field to raise the voltage. It is impossible to change the field current with sufficient rapidity to prevent fluctuation in the voltage when a considerable fraction of the load is thrown on. To see how true this is, consider the value of the self-induction of a generator field. A twenty-five cycle generator of 500 k.w. capacity, running at 300 r.p.m., 125 volts excitation, will have a self-induction in the field of about 5 henries and a resistance of about 2·4 ohms. Suppose that while it is running at half-load with 40 amperes in the field, one-fifth load in addition at power-factor 0·9 is suddenly switched on, and the voltage falls 4 per cent. The exciter, let us assume, is compensated so as immediately to raise the voltage of excitation 6 per cent. Fig. 1 shows what happens to the exciting

METHODS OF COMPENSATING SYNCHRONOUS A. C. GENERATORS.

- Altering excitation of field.
  - By rectified A. Current supplied to separate winding.
    - By commutator on generator.
      - Single-phase current (Westinghouse Co.)
      - Polyphase current (General Electric Co.)
    - By commutator run by synchronous motor.
    - By electrolytic or vacuum rectifier.
  - By rectified current supplied to main winding, dependent on power-factor (Heyland), (Latour).
  - By operating on exciter with Alternate Current.
    - Direct driven synchronous exciter with alternate current fed into D. C. winding (Rice).
    - Direct driven exciter fed with A. current in independent winding (Leblanc), (Danielson), (Boucherot).
    - Frequency changer feeding alternate current into armature of direct driven exciter (Steinmetz).
    - By alternate current operating on field magnets of exciter (Crompton).
  - By rheostat controlled by automatic means.
- By armature field.

current. It begins at 40, and rises gradually to 42·4 amperes (the current necessary to restore the voltage, taking into account saturation of iron). The voltage drops the instant the load is thrown on. After one second it has half recovered itself, and in about four seconds it is restored to practically full value. That is not perfect regulation. Consumers object to these little fluctuations.

The advantage of employing the armature field to restore the voltage is that its effect is instantaneous.

We will consider first the method of compensation by altering the field excitation. Broadly, there are four methods of doing this:

(*a*) By a rectified current supplied to a separate winding. (Simple composite wound generators.)

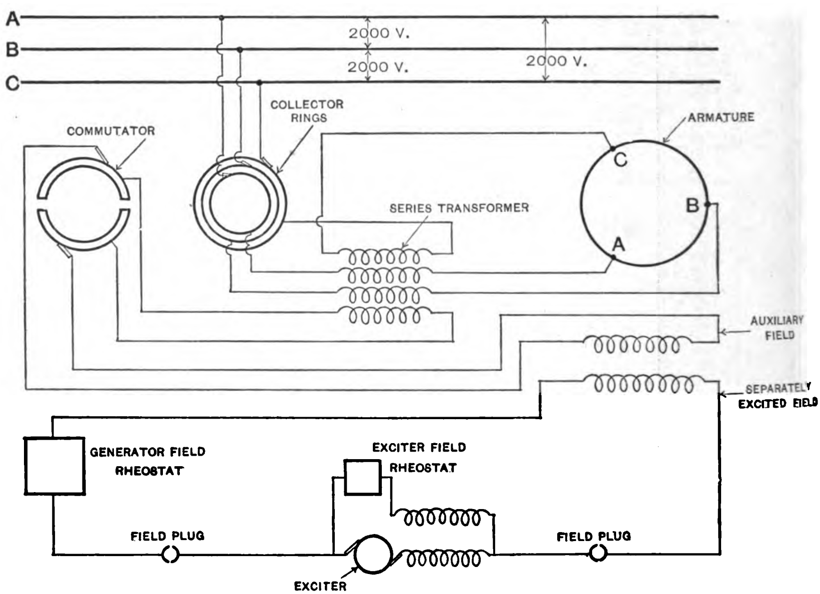


FIG. 2.—Diagram of connections for Westinghouse 3-phase composite wound generator.

(*b*) By a rectified current increasing with decreasing power-factor supplied to the main winding (as in the generators of Heyland and Latour).

(*c*) By operating upon the exciter with the alternating current, so as to raise its voltage (as in the methods of Rice and Leblanc).

(*d*) By operating a rheostat automatically. There are numerous methods of doing this, but they do not fall within the compass of this paper.

The simple rectification of alternate current for exciting purposes is well illustrated in the case of the old Westinghouse composite wound generator. This machine has been on the market since the early days

of alternators, and is still doing good service on loads of nearly constant power-factor.

Fig. 2 gives a diagram of the connections of this type of machine. The current from the armature passes through a series transformer, producing a secondary current which is proportional to the load. This secondary current passes to a commutator, by which it is converted into a uni-directional current, which goes to an auxiliary field winding, so that as the load increases, the field becomes stronger.

The rectification of an alternate current supplied to an auxiliary winding by means of a simple commutator is an exceedingly interesting problem, because the conditions that control sparkless commutation

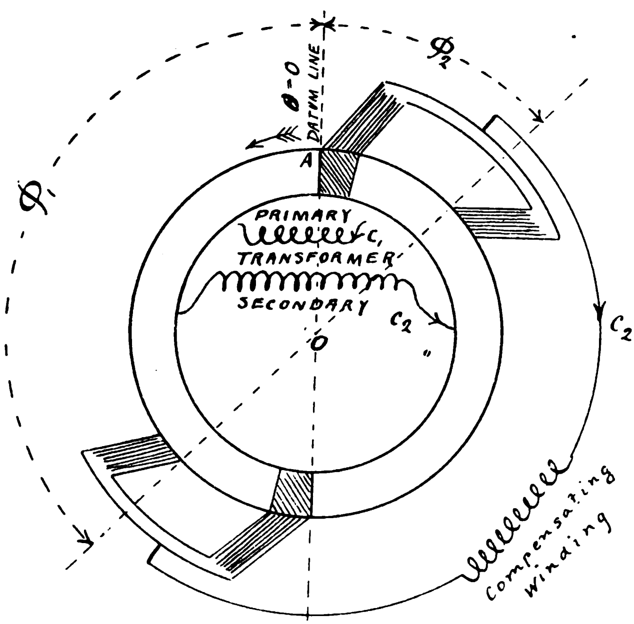


FIG. 3.

lend themselves with wonderful precision to theoretical investigation. One would think at first glance that, with the great self-induction in the circuit, sparking must always occur, but the short-circuiting of the segments of the commutator during a certain interval enables the commutation to be carried out perfectly, even with copper brushes and without any shifting of these brushes between no load and full load, provided the power-factor remains constant.

The conditions for sparkless commutation can be determined by the following considerations:—

Fig. 3 is a diagram of a commutator and brushes for a 2-pole machine. The line O—A is supposed to rotate with the commutator, making after any number of seconds, t, the angle θ with the datum

line fixed in space. There are 2 positive brushes, one of them in advance of the other, the angle of lead being denoted by ϕ_2. There are 2 negative brushes, which we will take as having the same lead.

Let the current in the primary of the transformer c_1 follow the law

$$c_1 = C_1 \sin(\theta + \omega).$$

Let N be the total flux through the transformer multiplied by the turns in the secondary, and multiplied by 10^{-8}.

Then let $N = l(c_1 - c_2)$ where c_2 is the instantaneous value of the current in the secondary. The directions taken as positive in the circuits are shown by the arrow heads.

$$l = \frac{4\pi \times (\text{turns of secondary})^2 \times \text{area of path} \times \mu}{10^9 \times \text{length of path}}$$

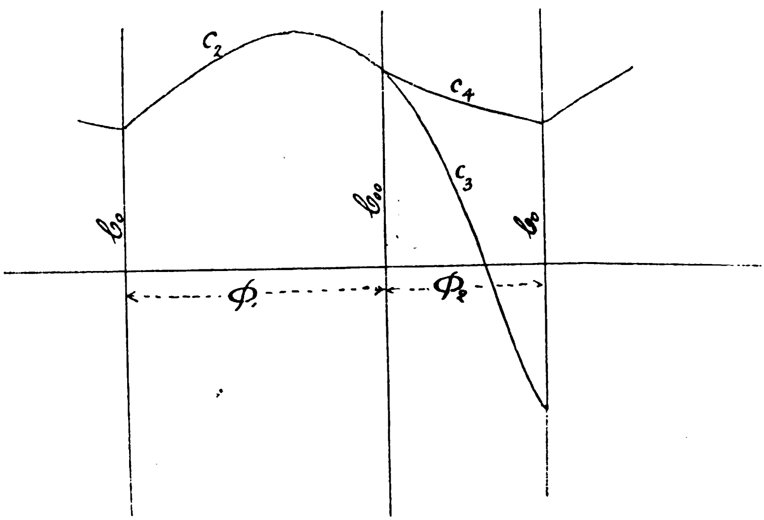


FIG. 3A.—Behaviour of Rectified Current and Short-circuit Currents.

depending on the construction of the transformer. Take the ratio of transformation 1 : 1. It is required to find the law of c_2 when given the following data :—

C_1 = max. value of primary current.
r = resistance of secondary coil of transformer.
R = „ compensating winding.
R_2 = „ total circuit $R+r$.
L = coefficient of self-induction of compensating winding.
l = „ „ of secondary of transformer.

Given also the circumstance that the brushes are set in a non-sparking position.

There are two periods in the half-turn of the commutator which must be considered separately.

I. The period during which the commutator turns through the angle ϕ_1. Here the secondary is in series with the compensated winding (called the active period).

II. The period (ϕ_2) during which the secondary is short-circuited and the compensating winding is also short-circuited (called the short-circuit period).

Let c_2 equal the current in the secondary and compensating winding during the first period.

$c_3 =$ current in secondary during short-circuit.

$c_4 =$ current in compensating winding during short-circuit.

The figure on preceding page gives the general forms (Fig. 3A).

I. For the first period

$$c_2 R_2 + L\frac{dc_2}{dt} = \frac{dN}{dt} = l\left(\frac{dc_1}{dt} - \frac{dc_2}{dt}\right),$$

$$c_2 R_2 + \left(L + l\right)\frac{dc_2}{dt} = l\frac{dc_1}{dt},$$

$$\frac{dc_1}{dt} = \frac{dc_1}{d\theta}\cdot\frac{d\theta}{dt} = 2\pi n\frac{dc_1}{d\theta}.$$

$$\theta = 2\pi nt \text{ where } n = \text{frequency}.$$

$$\frac{d\theta}{dt} = 2\pi n.$$

Let

$$2\pi n(L + l) = x_2$$
$$2\pi nl = x_1$$

$$c_2 R_2 + x_2\frac{dc_2}{d\theta} = x_1\frac{dc_1}{d\theta} = x_1 C_1 \cos(\theta + \omega)$$

$$c_2 = A_2 \varepsilon^{-\frac{R_2}{x_2}\theta} + \frac{x_1}{\sqrt{R_2^2 + x_2^2}} C_1 \cos(\theta + \omega - \sigma_2)$$
$$\text{where } \tan\sigma_2 = \frac{x_2}{R_2}.$$

Without assuming any definite value for C_o let $C_o =$ current in circuit at time $\theta = 0$.

Write

$$\sqrt{R_2^2 + x_2^2} = z_2$$

$$C_o = A_2 + \frac{x_1}{z_2} C_1 \cos(\omega - \sigma_2)$$

$$(1)\quad c_2 = \left\{C_o - \frac{x_1}{z_2} C_1 \cos(\omega - \sigma_2)\right\}\varepsilon^{-\frac{R_2}{x_2}\theta} + \frac{x_1}{z_2} C_1 \cos(\theta + \omega - \sigma_2).$$

II. For the second period, in order to simplify expressions, let θ' commence at the beginning of period No. 2, so that

$$\theta' + \phi_1 = \theta. \qquad c_1 = C_1 \sin(\theta' + \phi_1 + \omega).$$

In the same way as with period No. 1 we get

$$(2)\quad c_3 = \left\{C_{oo} - \frac{x_1}{z_1} C_1 \cos(\phi_1 + \omega - \sigma_1)\right\}\varepsilon^{-\frac{r}{x_1}\theta'} + \frac{x_1}{z_1} C_1 \cos(\theta' + \phi_1 + \omega - \sigma_1),$$

where C_{oo} is the current at the beginning of the second period and

$$z_1 = \sqrt{r^2 + x_1^2} \text{ and } \tan \sigma_1 = \frac{x_1}{r}.$$

III. Consider the law of variation of the current in the compensating winding during period No. 2.

$$c_4 R + L \frac{dc_4}{dt} = 0,$$

$$c_4 R = -x \frac{dc_4}{d\theta'},$$

$$c_4 = A_4 \varepsilon^{-\frac{R}{x}\theta}$$

When $\theta' = 0$; $A_4 = C_{oo}$,

(3) $$c_4 = C_{oo} \varepsilon^{-\frac{R}{x}\theta'}.$$

In equations (1), (2), and (3) we have five quantities that may be varied, viz., θ, ω, ϕ, C_o, and C_{oo}. These quantities are, however, connected by the following relations:—

When $\theta = \phi_1$, $c_2 = C_{oo}$.

When $\theta' = \phi_2 = (\pi - \phi_1)$, $c_3 = -C_o$ and $c_4 = C_o$.

so that $$C_{oo} \varepsilon^{-\frac{R}{x}\phi_2} = C_o.$$

This follows from the prescribed condition of non-sparking, for if the brushes do not spark $-c_3 = c_4$ for the instant $\theta' = \phi_2$.

At first sight these relations seem sufficient to determine c_2, c_3, c_4 as functions of θ and ω only.

For putting $\theta = \phi_1$ in (1) we get

$$C_{oo} = f(C_o, \omega, \phi_1)$$

but

$$C_{oo} = f_1(C_o)$$

so

$$C_{oo} = f_2(\omega, \phi_1).$$

Similarly putting $\theta' = (\pi - \phi_1)$ in (2) we get

$$C_{oo} = f_3(\omega\, \phi_1)$$

$$\therefore f_2(\omega\, \phi_1) = f_3(\omega\, \phi_1) \text{ or } \phi_1 = f_4(\omega).$$

That is to say, that if we prescribe any lead ω of the primary current in advance of the datum line of commutation, the position of non-sparking is given by the equations: and further, C_o and C_{oo} might be ascertained, and ultimately c_2, c_3 and c_4 expressed as functions of θ.

If, however, we try to carry out the above processes, we have to solve equations of the general form

$$\varepsilon^{a\theta} + A \sin \theta = 0.$$

This equation has an infinite number of roots.

To avoid this difficulty we may solve for ω instead of solving for ϕ_1. That is to say, we can prescribe a certain setting of the brushes and find the angle ω which will give sparkless commutation.

Where ϕ_1 and ϕ_2 are given $\varepsilon^{-\frac{R}{x}\phi_2}$, $\varepsilon^{-\frac{r}{x_1}\phi_2}$, and $\varepsilon^{-\frac{R_2}{x_2}\phi_1}$ are known constants say k_1, k_2, and k_3 respectively.

From (3) we have $C_o = k_1 C_{oo}$.
From (1) putting $\theta = \phi_1$ we have

$$C_{oo} = k_3 C_o - k_3 \frac{x_1}{z_2} C_1 \cos(\omega - \sigma_2) + \frac{x_1}{z_2} C_1 \cos(\phi_1 - \sigma_2 + \omega)$$

$$C_{oo} = k_1 k_3 C_{oo} - k_3 \frac{x_1}{z_2} C_1 \cos(\omega - \sigma_2) + \frac{x_1}{z_2} C_1 \cos(\phi_1 - \sigma_2 + \omega)$$

$$C_{oo}(1 - k_1 k_3) = - k_3 \frac{x_1}{z_2} C_1 \cos(\omega - \sigma_2) + \frac{x_1}{z_2} C_1 \cos(\phi_1 - \sigma_2 + \omega).$$

From (2), putting $\theta = \phi_2$, we have

$$- C_o = k_2 C_{oo} - k_2 \frac{x_1}{z_1} C_1 \cos(\phi_1 - \sigma_1 + \omega) + \frac{x_1}{z_1} C_1 \cos(\pi - \sigma_1 + \omega)$$

$$- C_{oo}(k_1 + k_2) = - k_2 \frac{x_1}{z_1} C_1 \cos(\phi_1 - \sigma_1 + \omega) + \frac{x_1}{z_1} C_1 \cos(\pi - \sigma_1 + \omega).$$

Expanding terms such as $\cos \{(\phi_1 - \sigma_1) + \omega\}$ into

$$\cos(\phi_1 - \sigma_1)\cos\omega - \sin(\phi_1 - \sigma_1)\sin\omega$$

in both (1) and (2) we get

$$\frac{1}{1 - k_1 k_3}\left[\frac{1}{z_2}\left\{(\cos(\phi_1 - \sigma_2) - k_3 \cos\sigma_2)\cos\omega - (\sin(\phi_1 - \sigma_2) + k_3 \sin\sigma_1)\sin\omega\right\}\right]$$
$$= - \frac{1}{k_1 + k_2}\left[\frac{1}{z_1}\left\{(\cos(\pi - \sigma_1) - k_2\cos(\phi_1 - \sigma_1))\cos\omega - (\sin(\pi - \sigma_1) - k_2 \sin(\phi_1 - \sigma_1))\sin\omega\right\}\right]$$

If we write

$$A = \left[- \frac{k_1 + k_2}{1 - k_1 k_3}\frac{1}{z_2}\left\{\cos(\phi_1 - \sigma_2) - k_3\cos\sigma_2\right\} - \frac{1}{z_1}\cos(\pi - \sigma_1) + \frac{R_2}{z_1}\cos(\phi_1 - \sigma_1)\right]$$

$$B = \left[\frac{k_2}{z_1}\sin(\phi_1 - \sigma_1) - \frac{1}{z_1}\sin(\pi - \sigma_1) - \frac{k_1 + k_2}{1 - k_1 k_3}\frac{1}{z_2}\left\{\sin(\phi_1 - \sigma_2) + k_2 \sin\sigma_2\right\}\right]$$

then from the above equations we get

$$(4) \qquad \tan\omega = \frac{A}{B}$$

That is to say, that ω can be calculated if we are given ϕ, and the other data on page 413.

In the curves given in Figs. 4, 5, and 6 below, ω has been calculated for different values of ϕ, and the curves of c_2 and c_4 plotted from the equations (1) and (3). The actual wave forms obtained in practice are plotted alongside in dotted lines. It will be seen that when ϕ_1 is small (as shown below, the machine works best with ϕ_1 small) the calculated curves agree with the actual fairly well. One point to note is that ω is independent of the value of C_1. That is to say, that the theoretical sparkless position of the brushes remains constant for all loads, provided the current does not change its phase. This is nearly true in practice when ϕ_1 is not too great. When ϕ_1 is great the sparkless position in practice does not agree with the theoretical, and it does not remain the same when the load is changed. The reason of this is explained below.

In order to plot the wave forms of the compensating current, it is necessary to know L. This is by no means a simple quantity, being made up of the self-inductions of both the separately excited winding and the compensating winding, according to the laws of two coils wound upon the same magnetic circuit. It will be found in practice, however, that $2\pi n\mathrm{L}$ and $2\pi nl$ are both so large compared with R and r that it is sufficient to know L and l only approximately in order to plot approximate wave forms. In any case they are not really constants, and we can only hope to know rough values. Where we have a machine at hand the average value of L for purposes of calculation may be estimated by passing an A.C. current of known frequency and at known voltage through the comp. winding and measuring the power-factor of the circuit.

Having obtained the power-factor $\cos\psi$ we can multiply the voltage by $\sin\psi$ and obtain the effective reactance voltage, $2\pi n\mathrm{LC}$, and hence L. It will be seen that, however complex L may be (being made up of numerous factors, including the self-induction of eddy current paths in the iron), it is equivalent in its operation to a certain quantity of magnetic flux threading through the compensating winding, and therefore represents a certain store of energy for every ampere passing through that winding. As the current sinks according to the law

$$c_4 = \mathrm{C}_{\infty}\varepsilon^{-\frac{\mathrm{R}}{x}\theta'}$$

that energy is thrown back into the circuit, and is expended on the resistance R. The rate of fall of c_4 will be controlled by R, and will scarcely be affected by the conduction of iron, that being small compared with $\frac{1}{\mathrm{R}}$. The conduction of the separate excitation winding we have put out of consideration, because we have taken for our L not the total flux which passes through the compensating winding when one ampere passes in it, but only the flux which produces the reactance $2\pi n\mathrm{L}$ in the compensating winding when the conditions of both circuits are the same as in the normal operation of the machine.

In other words, c_4 will sink at about the same rate as if the only flux involved were $\mathrm{L}c_4$, and the only resistance involved were R. The estimation of L from the drawings of a machine would be difficult, but it is probable that the comparison of a number of calculated cases with experimental results in different types would enable any one to estimate L with sufficient accuracy.

Three years ago the author experimented on these composite wound machines, recording the positions of the brushes, and taking the wave form of the currents to see how far the phenomena observed agreed with what might be foretold by theory, and particularly to see what prospect there might be of applying a simple commutator of this kind to large central station generators, and compounding them at low power-factors.

The following list gives the values of the constants in one of the machines in question :—

$r = 0{\cdot}1$ ohm
$R = 0{\cdot}55 + 0{\cdot}223 = 0{\cdot}773$
$R_2 = 0{\cdot}873$
$l = 0{\cdot}026$ henry
$L = 0{\cdot}0202$ „
$x_1 = 8{\cdot}17$
$x_2 = 14{\cdot}5$
$x = 6{\cdot}35$
$z_1 = 8{\cdot}18$
$z_2 = 14{\cdot}55$

$\sigma_1 = 89^\circ\ 18'$ $\quad \sin \sigma_1 = 1 \quad \cos \sigma_1 = 0{\cdot}012$
$\sigma_2 = 86^\circ\ 33'$ $\quad \sin \sigma_2 = 0{\cdot}998 \quad \cos \sigma_2 = 0{\cdot}06$

Filling in these values in the equation (4) above for $\tan \omega$, and taking three different leads, namely, 0·7, 0·5, and 0·08 of the pitch, we get ω in the different cases. Having obtained ω in each case, we can plot c_2 and

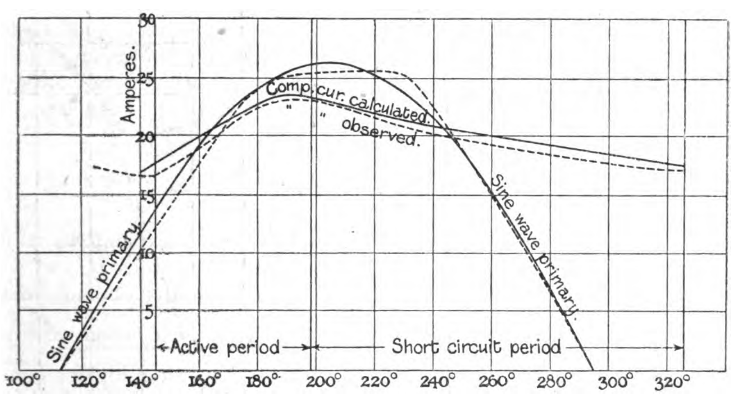


FIG. 4.

c_4 from the equations (1) and (3). This has been done in the curves, Figs. 4, 5, and 6, which show how nearly the actual wave forms follow the calculated wave forms. Fig. 4 shows the operation of the machine under normal conditions, with a big lead ($\phi_2 = 0{\cdot}7$ of pitch) given to the brushes. It is with a great lead that the commutation is most stable, for the reasons to be seen later.

The full lines in Fig. 4 give the calculated wave forms, assuming a sine wave for the armature current. The dotted curves show the actual observed wave forms of the armature current and rectified current. It will be seen that the theory not only enables one to calculate very closely the value of the rectified current, but also to foretell with great precision the position of the brushes at which good commutation will take place.

During the active period, beginning at about 145° in Fig. 4, the primary current is on the upward slope. This causes an increasing magnetising current in the transformer, producing an electro-motive force in the secondary circuit, and causing the secondary current to

rise, as indicated by the line marked "comp. current observed." At about 198° the positive and negative brushes are both short-circuited by the commutator. The current in the secondary of the transformer is left to follow the primary current according to the laws stated above, while the current in the magnetising coils begins immediately to sink according to a logarithmic law, it being maintained only by the self-induction of the winding. At the point 325° the active period commences again, the condition of non-sparking being that at this instant the current in the secondary winding of the transformer shall be equal to the current in the magnetising coils. In plotting Figs. 4, 5, and 6, the phase position of the full lines in relation to the active and

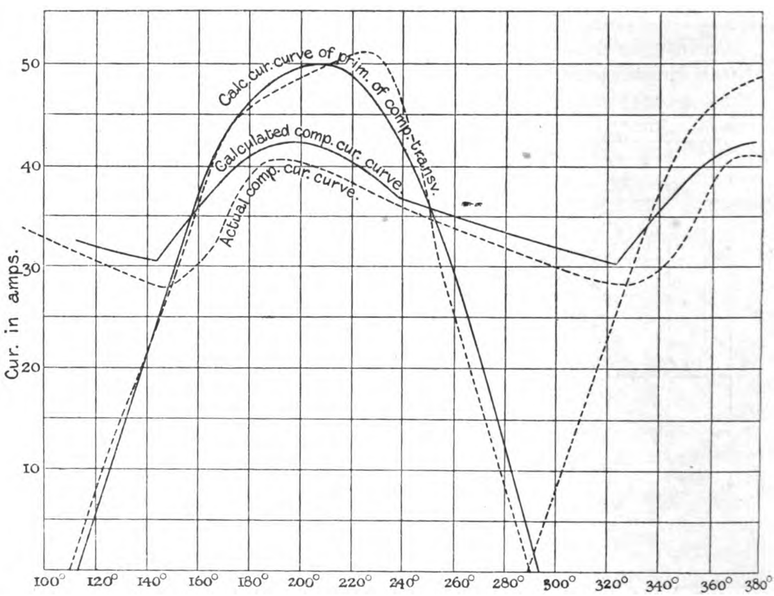


FIG. 5.

short-circuit periods is that indicated by the above theory, while the phase position of the dotted lines is that observed in practice.

In Fig. 5 the lead is 0·5 of pitch; this gives almost maximum compounding current when the brushes are rocked to the sparkless position. Here also the theoretical curves agree very closely with the calculated curves, considering the deviation of the armature current wave from the true sine form.

In Fig. 6 the lead is very small, being equal to only 0·08 of pitch. This is a condition under which the machine is not run in practice, because with a small lead the commutator is sensitive to variations of load; and, though a sparkless position can be found, the change in load causes flashing at the brushes.

The reason of this is easily seen in Fig. 6. For the greater part of a revolution the rectified current is under the control of the secondary

current in the series transformer. With a theoretically perfect series transformer and a pure sine wave in the primary, the secondary current would follow the primary exactly, but partly on account of the saturation of the iron in the transformer, and partly on account of the flat top to the armature current wave form, the secondary current is not able to follow the primary, so that the theory is to a certain extent inapplicable. In Fig. 6 the observed secondary wave form rises at first (between 140° and 180°) with the primary. At 180° the primary becomes flat-topped, which means that there is no increasing magnetising current in the series transformer. The secondary electro-motive force being almost zero, the secondary current begins to sink according to a logarithmic law, just as though the short-circuited period had set in. This has the effect of disturbing the theoretical values of the currents

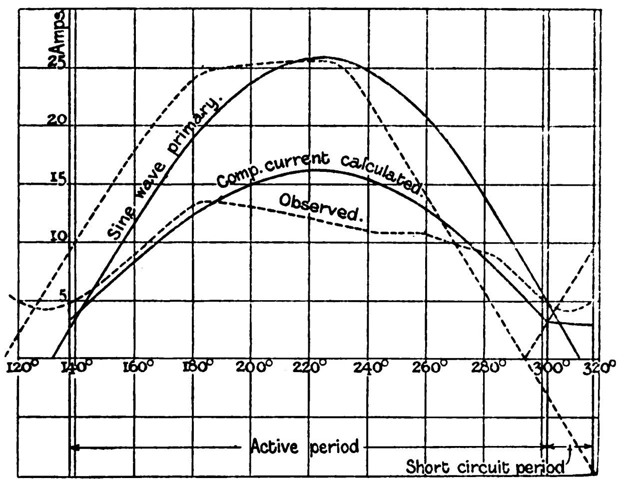


FIG. 6.

in the magnetising coils and the transformer secondary at the commencement of the active period, so that the position of the brushes for sparkless commutation differs very widely from what would be arrived at by sparkless theory.

The conclusions arrived at are as follows :—

For a given normal lead between the brushes and with a definite power-factor, there is a perfectly definite "position of rocking" at which sparkless commutation will occur, and this position is independent of the load. It would be possible to run sparklessly at any power-factor, provided the phase position of the commutator followed the phase position of the current. This can be achieved by driving the commutator by a small synchronous motor placed in series with the current to be commutated. The motor can be made so small that its reactive effect upon the current is negligible.

The slight sparking caused by the changes in wave form, which prevent the theory being completely applicable, can be obviated by

using a number of brushes with varying lead connected in parallel through resistances.

Next the question arose, could the excitation of the generator by means of a simple commutator of this kind be made dependent on the power-factor, so that, as the power-factor decreased, the excitation increased? The answer is that it could. We cannot go fully into this matter here, because compensation by rectification of the alternate current is not the main subject of this paper. It is sufficient to point out that in a polyphase machine (whatever the number of phases) it is

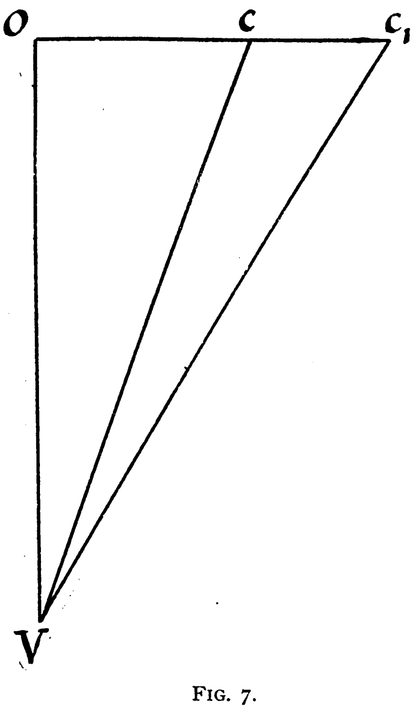


FIG. 7.

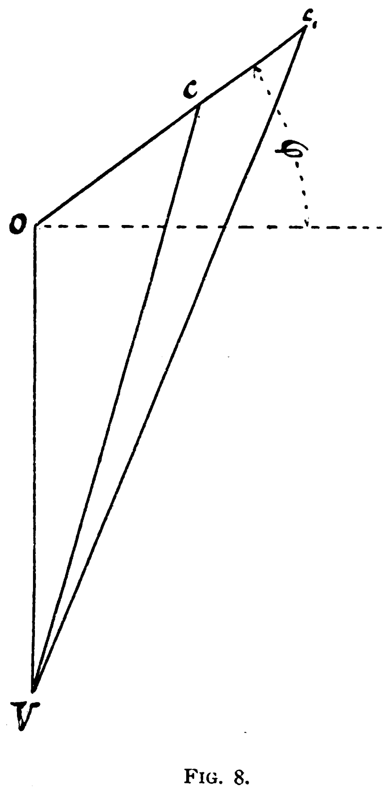


FIG. 8.

always possible by a simple arrangement of transformers to obtain two currents at right angles in phase. One of these, O V, Fig. 7, can be made constant, and fixed in position by the phase of the voltage. The other, O C, can be made proportional to the load and at right angles to O V when the load has a power-factor unity and making an obtuse angle to it at lower power-factors (see Fig. 8). It is well known that the resultants, such as V C, V C_1, etc., are almost proportional to the exciting currents required under the different conditions of load, provided O C and O V are properly proportioned to the characteristics of the generator. If then the current V C is made uni-directional by

means of a sparkless commutator, as indicated by the above theory, we have achieved the desired result. Such a method, however, does not give a generator which responds instantaneously to sudden changes of load, because it is impossible to change the field current suddenly. For this reason attention was directed to another method of compounding, which promised to be instantaneous in operation and to do away with the commutator altogether.

But, returning to our classification table, we have not yet finished with the rectified current methods. The General Electric Company of America many years ago adopted the style of commutator shown in Fig. 9. The three ends of a star winding, which would ordinarily be connected to a common junction, are connected to a three-part commutator, which supplies uni-directional current to two brushes in series with the auxiliary winding.* Where the number of poles is greater than two, the number of segments of the commutator is proportionately increased. Shunt resistances are used to help out the commutation, and enable the amount of compounding to be adjusted.

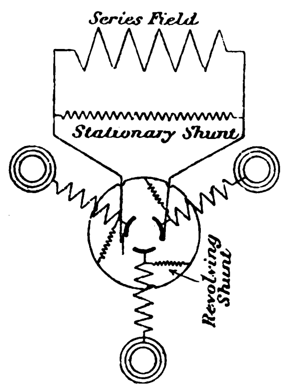


FIG. 9.—Diagram of General Electric Co.'s Method of Compounding.

The electrolytic rectifier has received a great deal of attention of late years, and various experimenters have so increased its efficiency and lasting qualities that there seems some prospect in the future of its competing with the commutator.

The Cooper Hewitt mercury-vapour rectifier, too, is coming every day nearer and nearer to perfection.

One of the uses to which these rectifiers can be applied is the compounding of alternate current generators. The amount of current to be rectified is small in comparison with the output of the generator, and the employment of the method described in Figs. 7 and 8 gives at once a generator compounded for any power-factor.

We now come to what is certainly the most successful self-exciting compounding A.C. generator upon the market—the generator devised by Alexander Heyland. The machine is excited by a rectified current, which increases with the load, and also as the power-factor becomes lower, so as to maintain almost constant voltage for any condition of load. It is provided with a special winding, which greatly facilitates the commutation of the current.

Every electrical engineer is familiar with Heyland's famous graphic construction, which enables the student to realise at a glance what is going on inside an induction motor. Shortly after its publication Heyland devised a method of supplying the magnetising current at a

* For wave forms of commutated current of this machine, see paper by D. C. Jackson, "The Composite Wound Alternator," *Transactions of Amer. Inst. Elec. Eng.*, vol. xv., pp. 402–408.

low frequency to the rotor of an induction motor instead of supplying it to the stator at a high frequency, in that way increasing the power-factor to unity. He then extended the method to the self-excitation and compounding of asynchronous generators,* thereby enabling a generator of this class to take any kind of load, whether inductive or not. An application of the same principles, with a modification of the field windings, brought him to the self-excitation and compounding of synchronous generators. Whatever the similarity in principle may be between the asynchronous and synchronous cases, the simplest way of

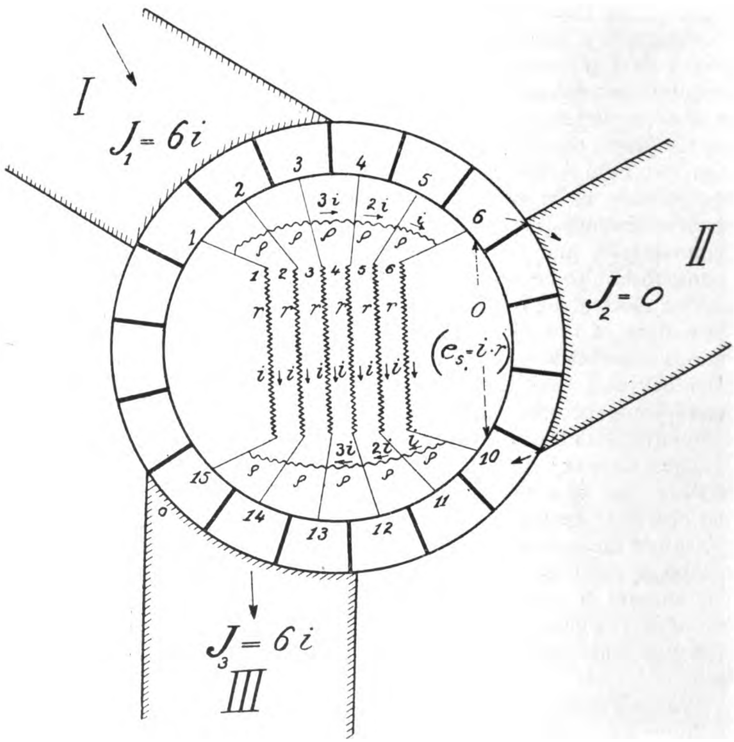


FIG. 10.—Diagram of Heyland s Method of Compounding.

explaining the latter type of generator lies in regarding it as being excited by a rectified current through a number of field windings connected in parallel.

Fig. 10, which is taken from Heyland's late paper, read before the St. Louis Convention, shows the principle as applied to a two-pole generator. Here six exciting circuits (r) are placed in parallel, being inter-connected by resistance (ρ). The end of each circuit is connected to a commutator-bar, as shown, and a 3-phase current is fed

* *Elektrotechn. Zeitschr.*, vol. xxii., pp. 1021–1023.

into the commutator by the three brushes. Consider that at the instant when the commutator brushes are in the position shown the current in the upper brush I. is sinking from a maximum, being, in fact, o·866 of the maximum value. The current will divide between the different windings, and go by brush III. As the commutator rotates clockwise, the current in brush II. will gradually increase to its maximum until the instant when the bars Nos. 3, 4, and 5 are opposite it. The current will then be partly led out by brush I. and partly by brush III. As each brush receives its maximum current it

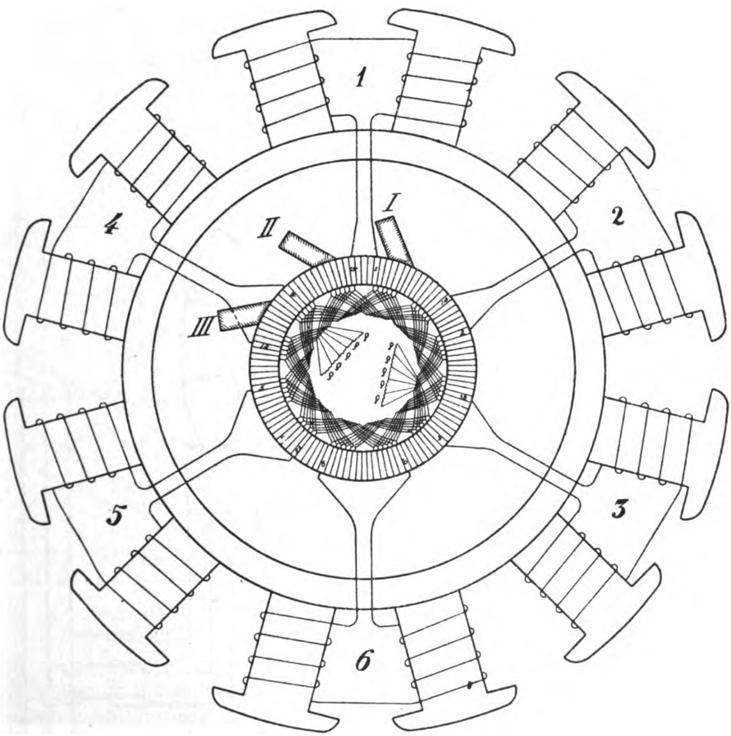


FIG. 11.—Heyland's Connections to Field Magnet.

also receives its complement of bars into which to feed it. This is the condition of affairs which would give the six circuits their lowest apparent resistance, but it is obvious that a considerable change may be made between the phase positions of the current and commutator without materially affecting the commutation, because the resistances ρ tend to distribute the current in all the circuits. Note that the current is always passing the same way through the six circuits.

The commutation of the current is aided by fixing the values of the resistances r and ρ so that when a brush (such as II.) spans commutator bars 10 and 6 these two bars are at the same potential, and no current

tends to flow through the short-circuiting brush. For instance, in Fig. 10 the value of ρ is one-twelfth of the value of r. The self-induction of the windings r is so great compared with the resistance that as the commutator revolves the current i remains practically constant. The distribution of current is, therefore, very nearly as shown in Fig. 10. The drop in potential between bars 3 and 13 is $i\,r = 12\,i\,\rho$. The drop

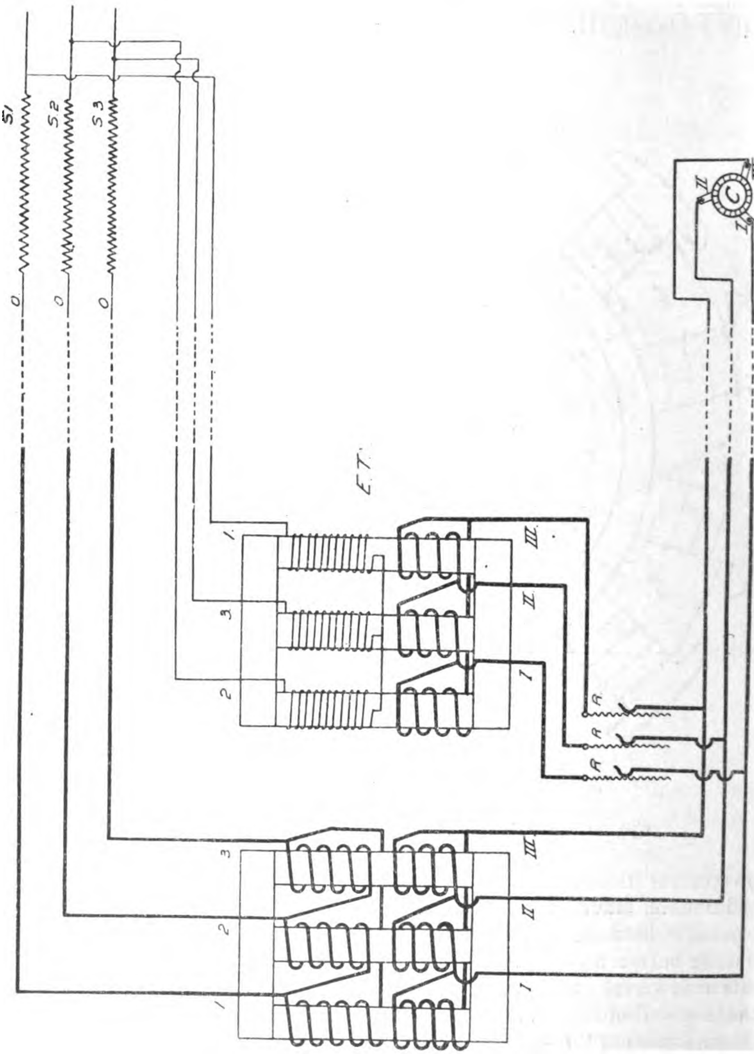


Fig. 12.—Connections of Transformers to Heyland's Commutation.

between bars 3 and 6 is 6 i, and between bars 10 and 13 it is 6 i. Therefore, between bars 6 and 10 it is zero. The current in r between 6 and 10 is maintained for the instant by the self-induction of the winding. Thus no sparking occurs, as brush II. leaves bar 10.

Fig. 11 shows one of the ways of winding the six circuits on a twelve-pole machine. It will be observed that the form of the field magnet need not differ in any way from that of simple uncompensated field magnets.

The fact that there are always several paths open for the current, and that the ends of the commutator bars are bridged by resistance, obviates the tendency to spark. The 3-phase current is supplied partly from a transformer connected as a shunt across the mains, which supplies a nearly constant current, whose phase is fixed by the

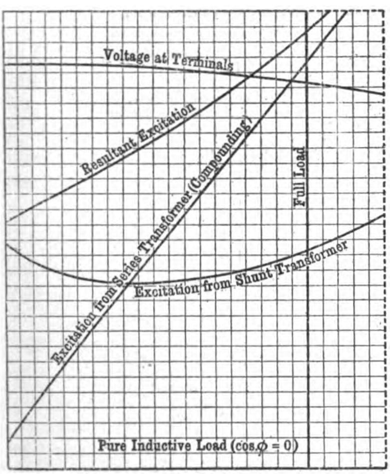


FIG. 13.—Regulation on Zero Power factor of Heyland 360 k.w. Generator.

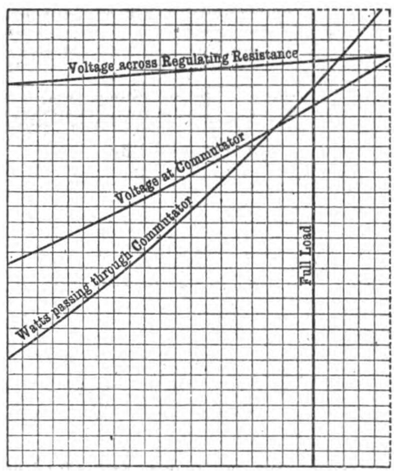


FIG. 14.—Particulars of Excitation.

voltage of the generator and partly by a transformer in series with the mains which supplies currents proportional to and in phase with the armature currents. These currents are combined, as shown in Fig. 12, the result being that, as the load increases, the resultant 3-phase currents increase, and the lower the power-factor the greater the increase. In order to adjust the compounding, the machine is run at zero power-factor, and adjustable resistances are inserted in the shunt circuit. The number of turns in the series and shunt-transformers can be changed so as to give the desired over-compounding either at non-inductive load or at any power-factor. I am indebted to Messrs. Witting, Eborall and Co., who supply these generators in this country, for very full information about them, and for the diagrams here given.

Figs. 13 and 14 illustrate the performance of a recent 360 k.w.

3-phase generator of the Heyland type, built by Kolben and Company, and designed to give a constant pressure of 5,200 volts at its terminals at all loads and a power-factor of 0·8. The compounding tests were carried out on a load of practically zero power-factor, and even under these conditions the full load pressure drop was only 4·8 per cent. (see Fig. 13). On a power-factor of 0·8 the voltage was constant between no load and full load. Fig. 14 gives particulars of the exciting watts at different loads. The characteristics of this generator are such that on short-circuit it will give only 1·7 times full load current with normal no load excitation. Notwithstanding this heavy armature reaction, the generator compounds exactly at 0·8 power-factor, and the exciting watts at full load are less than 1·9 per cent. of the total output. This shows that by adopting Heyland's method a great saving can be effected in the size of the machine, for a generator which would, if

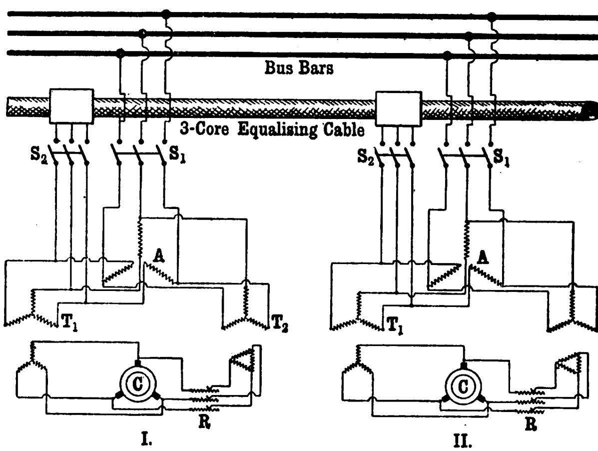


FIG. 15.—Connections for two Heyland Generators in Parallel.

uncompounded, drop 16 per cent. on non-inductive load is made to regulate well.

Fig. 15 shows the connection for two self-exciting and compounded high pressure 3-phase generators operating in parallel. It is, of course, necessary to have equalising circuits between the generators just as with compounded direct-current machines.

An objection has been raised to this type of generator on the ground that when the number of poles is great the commutator would become cumbersome and complicated. In answer to this is shown in Fig. 16, a drawing of a 350 k.w., 3-phase, 50-cycle generator running at 125 r.p.m. (and having, therefore, forty-eight poles), built for operating an electrically-operated winding engine in Belgium. Notwithstanding the unfavourable conditions, the makers had no more difficulty with the commutator than with that of a direct-current machine. Mr. Heyland has proposed a modification of his method of compounding which can be used with existing machines. Instead of

making the generator self-exciting, he proposes to retain the direct coupled exciter and to compound the latter. In addition to its ordinary shunt winding, the exciter is provided with an inter-connected multiple circuit winding on its field magnet. This is supplied with a rectified current through a commutator, in the same way as the main generator field might be supplied in the method described above. The connections to the commutator are clearly illustrated in Fig. 17. The commutator rotates synchronously, and the current it supplies raises the voltage of the exciter.

Further information on these generators may be found in some excellent articles by Mr. Eborall in the *Electrician*.*

The method of M. Latour † is similar to the method proposed by Heyland for exciting an asynchronous generator. If we take an ordinary 2-pole direct current armature and feed 3-phase current into the commutator by means of three brushes placed at 120° to one another we will produce a revolving magnetic field. If now we rotate the armature in the same direction as the rotating field and in synchronism with it, the conductors, revolving with the field, will not be cut by it, and the field will produce no back E.M.F. The conductors on the armature will behave to the outside circuit as if they had resistance, but not self-induction. This revolving field can now be used as a revolving field magnet by being placed inside a stationary armature.

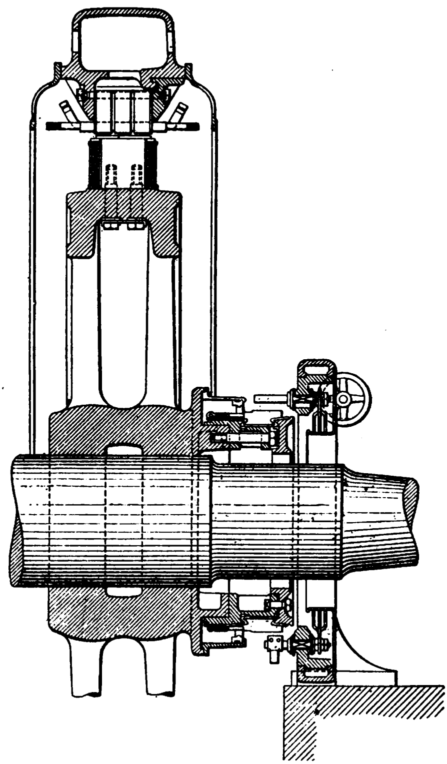


FIG. 16.—Heyland's 48-pole Generator.

We now come to a large class of methods in which the exciter is operated upon by alternate currents, so as to increase the voltage of excitation. If an A.C. armature is carrying a current which is leading on the E.M.F., the current tends to strengthen the field, because at the

* *Electrician*, vol. 51, pp. 442, 486, and vol. 53, p. 509. Also A. Heyland, *ibid.*, vol. 51, p. 969. *Elektrotechn. Zeitsch.*, vol. 22, p. 1021, and vol. 23, p. 560.

† *L'Industrie Électrique*, vol. 11, pp. 77, 101. *L'Eclairage Électrique*, vol. 29, pp. 113, 328, and vol. 31, p. 50. *Société Internat. des Électriciens, Bulletin*, vol. 2, p. 383. Also criticism of Latour's method by P. Bunet, *l'Industrie Électrique*, vol. 10, p. 472.

instant when it is at its maximum the conductors carrying it are on the same side of the pole as the field winding conductors which carry direct current in the same direction. This principle is easily applied to an exciter, so as to increase its voltage as the A.C. load is increased and to make the increase greater when the power-factor is lower. The alternate current may either pass through independent windings on the armature of the exciter, or it may be passed directly into the ordinary direct current winding.

In 1897 E. W. Rice, jun., published* a method in which the voltage of the exciter was affected in this way. Fig. 18 shows the application of his method to a single-phase machine with a revolving armature. G is the alternate current generator, having armature coils A and field magnet F. The brushes B_1 and B_2 collect the alternate current from the armature. E is the exciter on the same shaft. In this case it has

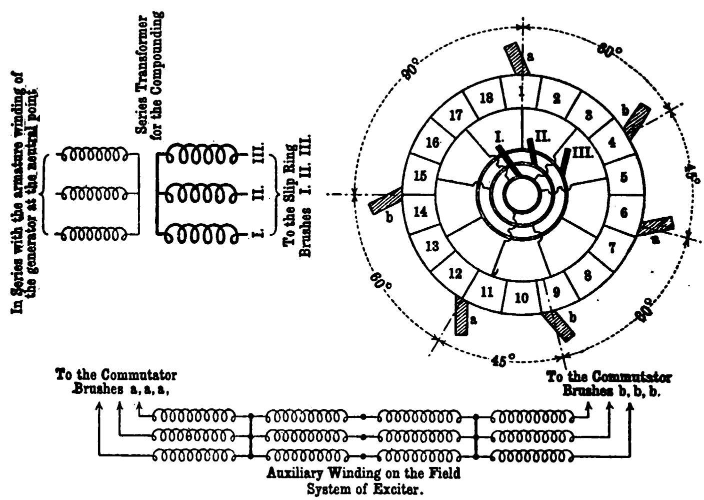


FIG. 17.—Connections to Commutator of Heyland's Exciter.

the same number of poles as the alternator. The armature *a* is of the ordinary D.C. multi-polar type, but the alternate current from the generator is led into it at the point p_1 shown in the figure, and after circulating in the winding passes to the collector ring r_2. The point at which the current enters the armature is so chosen that a current lagging on the alternator E.M.F. will, at its maximum value, augment the field excitation of the exciter. In Fig. 19 the same principle is employed, but here the current from a series transformer is passed into the D.C. armature, thus enabling the voltage and current to be more conveniently adapted. Rice also describes an independent exciter running synchronously with the generator, having collector rings on the armature into which the alternate current is passed. Another method † due to Mr. Rice is shown in Fig. 19A. Here the

* See Patent No. 29,585 of 1897. † See Patent No. 16,128 of 1900.

alternate current which is fed into the exciter is derived from two transformers, the primary of one being connected in shunt with the generator, and the primary of the other being connected in series with the mains. The choke coil and resistance shown at 12 enable the

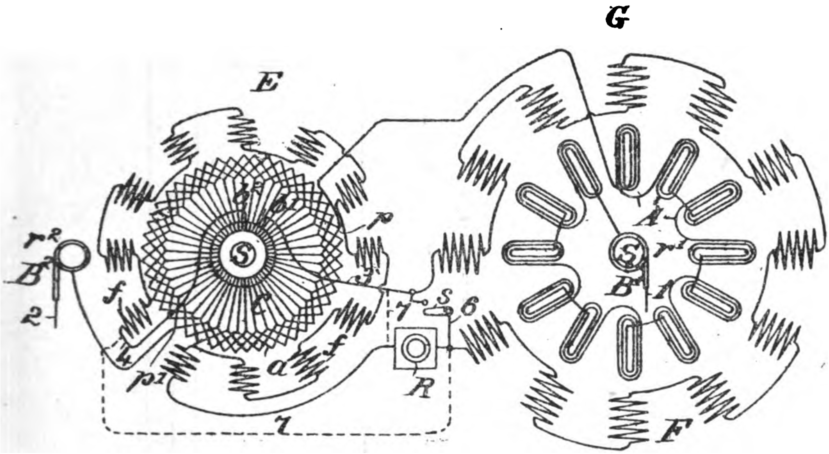


FIG. 18.—Rice's Method (1897).

compounding to be adjusted for any power-factor. Machines of this type have been worked commercially on a somewhat extended scale by the General Electric Company of America.* The main objection to them is that the voltage does not respond instantaneously to sudden

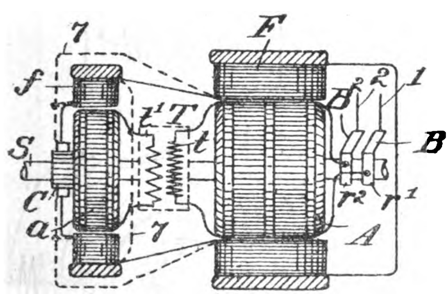


FIG. 19.—Rice's Method with Transformer.

demands on the load. To get over the objection of a large exciter with as many poles as the generator or spur gear which was found necessary when a small exciter was employed, Steinmetz put forward † the suggestion shown in Fig. 20. The alternate current from the

* H. G. Reist, *West. Electn.*, vol. 25, p. 309; *Electrical Review*, vol. 49, p. 343. E. J. Berg, *Elec. Wld. and Engineer*, vol. 37, p. 676.

† See Patent No. 15,379 of 1900.

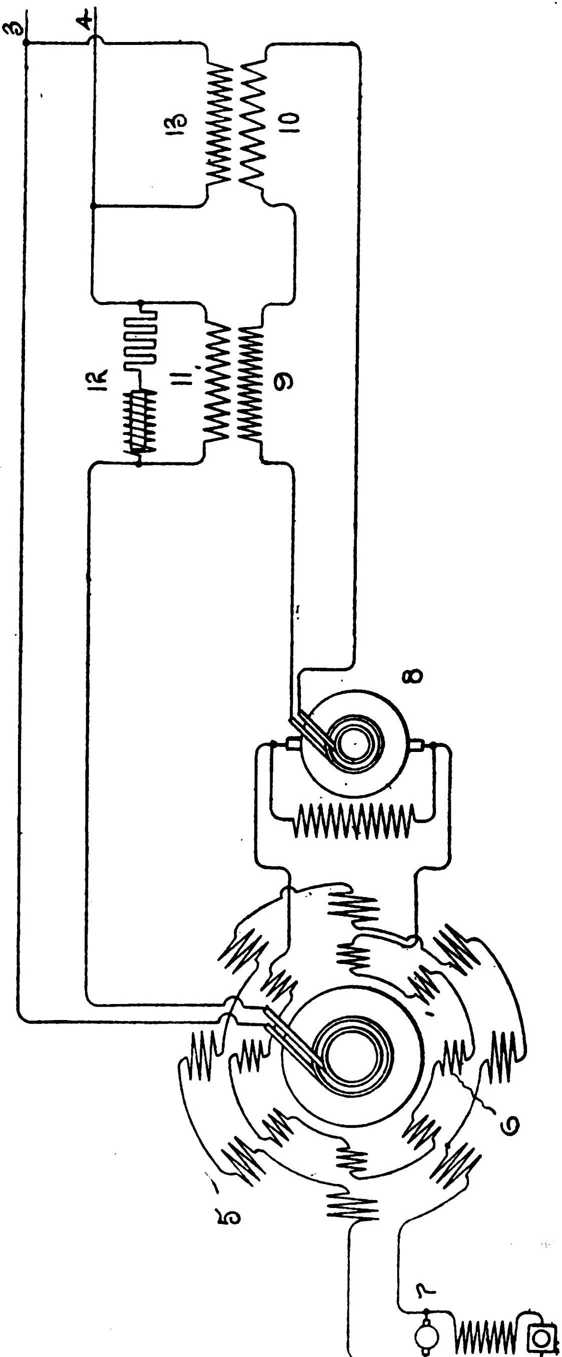


Fig. 19A.

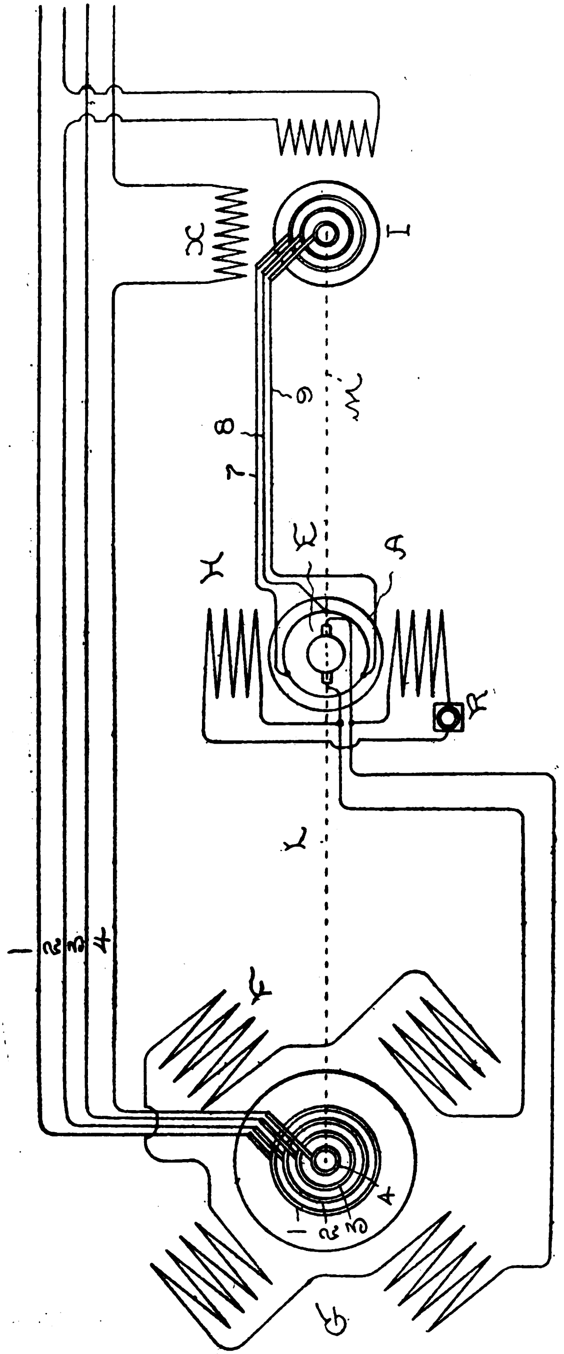


FIG. 20.—Steinmetz's Variation.

generator is first of all passed through a frequency changer. A current of lower frequency was thus obtained from the secondary winding, which was proportional to the main current. This current is suitable for feeding into an exciter direct driven from the generator, but having a fewer number of poles.

Another device, due to Steinmetz,* is shown in Fig. 21. Here an exciter on the same shaft, and having the same number of poles as the generator (the poles in this case being unwound), is magnetised entirely by alternate currents in the armature, and supplies a current which augments the excitation of an independent exciter, which in turn has its electro-motive force increased as the load comes on. The drawback

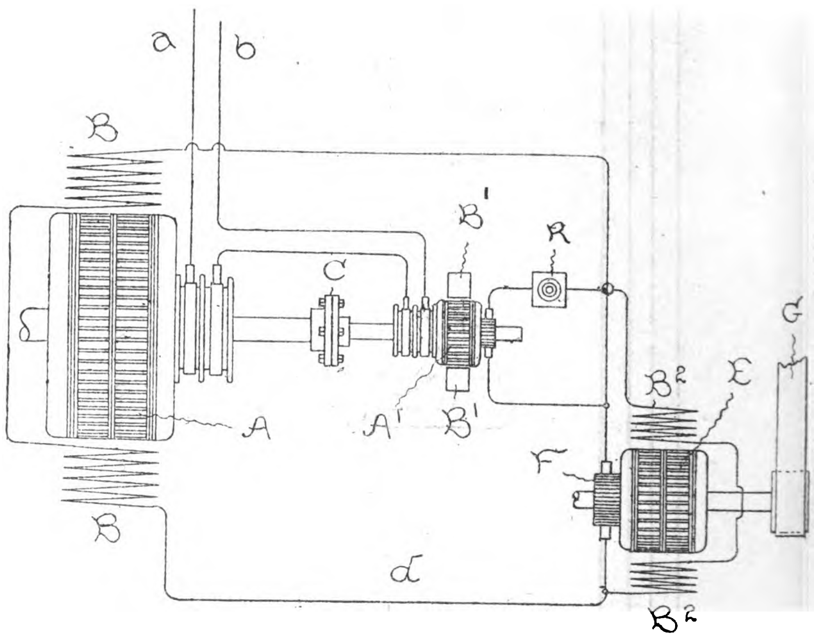


FIG. 21.—Steinmetz's Compensated Exciter.

of this plan is that the self-inductions both of the exciter field and of the main generator field delay the increase of excitation which is necessary to maintain the voltage constant.

A method patented † by Mr. R. B. Ransford in 1898 is somewhat analogous to Rice's method. The alternate current is derived from two transformers (see Fig. 22), the primary of one of which is connected in shunt with the generator, while the primary of the other is connected in series with the mains. We thus get an alternate current which is proportional to the requisite exciting current at all loads, as indicated above in Figs. 7 and 8. The exciter may be regarded as a

* See Patent No. 23,738 of 1899. † See Patent No. 27,370 of 1898.

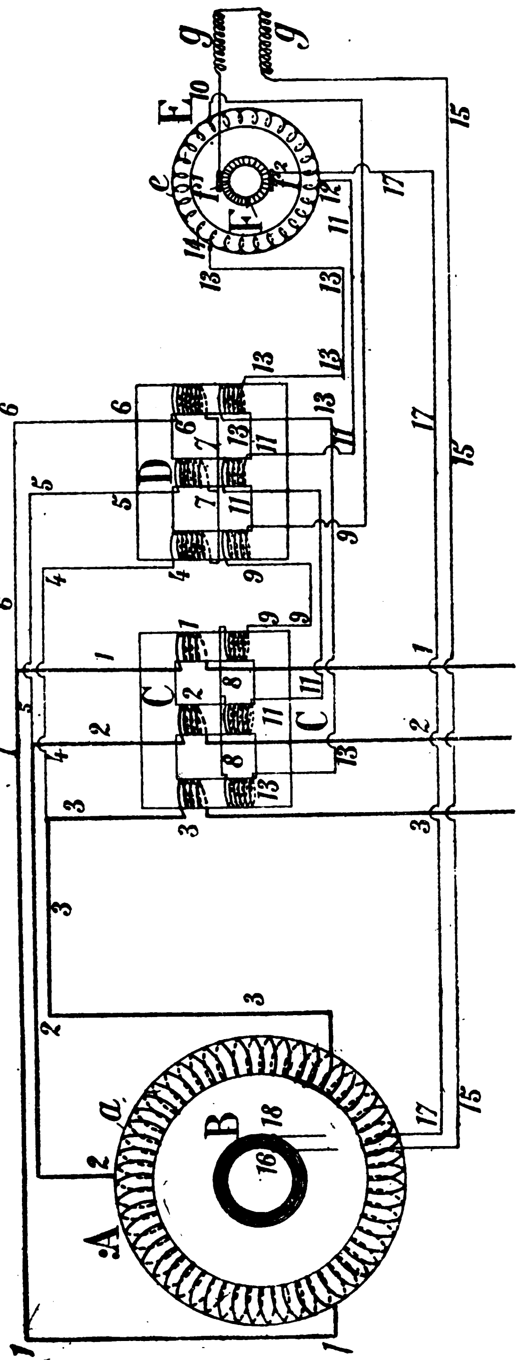


FIG. 22.—Ransford's Method.

rotary converter, which converts this alternate current into direct current.

M. Leblanc has constructed an exciter which so varies the current it supplies as to give almost constant voltage at the terminals of the

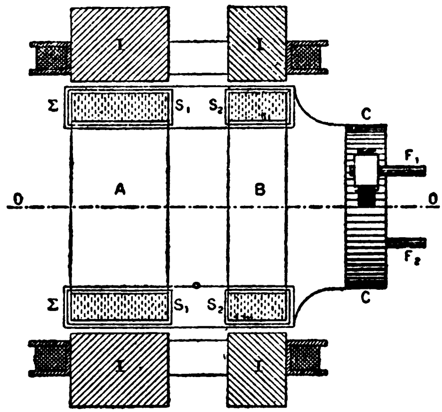


FIG. 23.—Leblanc's Exciter.

generator for any kind of load. The general construction of this machine is shown in Fig. 23. It consists of two ring windings on a

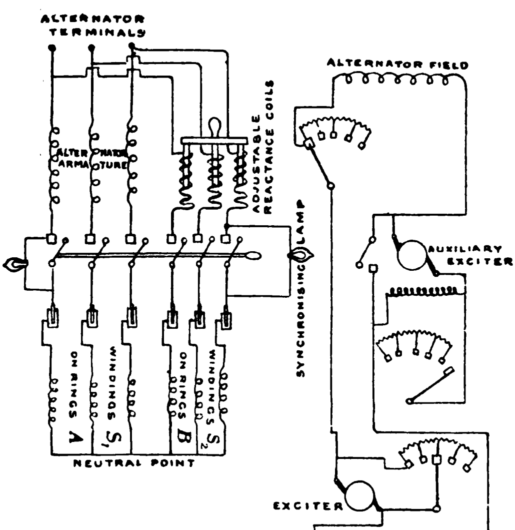


FIG. 24.—Connections to Leblanc's Exciter.

common shaft. The first, S_1, is placed in series with the armature winding of the alternator, and the second, S_2, is connected as a shunt. A third winding ρ is wound around both, and is the same as an ordinary

direct current winding connected to a commutator. The exciter runs as a synchronous motor, taking from the circuits S_1 and S_2 the power necessary to keep up its motion and produce the continuous current which is collected at the brushes. When the current lags in the alternator it circulates in the winding S_1 in the direction which tends to strengthen the field, and makes the exciting current greater. The manner of connecting up the circuits of this machine is shown in Fig. 24. Further particulars of this construction and its performance will be found in the articles referred to below.*

E. Danielson independently invented a similar method,† using only the coils in series with the alternate current armature. He had a direct connected exciter, and the alternate current passed round the armature of this before going to the collector rings.

M. Boucherot has devised a method which falls under this class, but this paper would be too extended if I entered upon an explanation of it.‡

Messrs. Crompton & Co. have devised § a method in which the alternate current is passed around the field magnets of the exciter, producing a wavy uni-directional current, which, by the aid of certain choke coils and resistances, is made to increase the excitation of the alternate current generator on loads of low power-factor.

We now come to consider the method ‖ in which the field excitation is maintained constant, and in which use is made of the armature reaction to strengthen the field on load. Fig. 25 shows the arrangement of the generator field circuit. Each pole consists of at least two parts, a saturated part and an unsaturated part. The first is wound with a magnetising coil, and there may or may not be another magnetising coil placed around the whole pole. This second winding may be necessary for varying the normal voltage of the machine. In Fig. 25 the broad pole is very highly saturated in the region marked with shaded lines, while the narrow pole on the right is unsaturated, and at no load is unmagnetised. The field rotates counter-clockwise, and as the pole depicted in Fig. 25 is a south pole, it gives rise to an electromotive force in the conductors above it, which is in the sense indicated by the crosses—that is to say, away from the observer. Any current in the armature which is in phase or nearly in phase with this electromotive force will tend to demagnetise the saturated part of the pole and magnetise the unsaturated part. The flux from the saturated part cannot be changed to any great extent, while the unsaturated part becomes highly magnetised by armature reaction and increases the electro-motive force of the armature. The British Westinghouse Electrical and Manufacturing Company, Limited, have thoroughly tested this type of machine, and in all cases where the power-factor is not

* *Comptes Rendus*, vol. 127, p. 716. *L'Éclairage Électrique*, vol. 17, pp. 425, 473, 509, and 547; vol. 20, pp. 171, 205, 253, 292, 404, 447, and 498. *Electrician*, vol. 45, p. 844. See also Patents No. 22,333, 1896, and 21,165, 1898.

† *Elektrotechn. Zeitsch.*, vol. 20, p. 38.

‡ Strymeersch, *Assoc. Ing. Él. Liege, Bull.* vol. 1, p. 19. Also Patent No. 9,533 of 1900.

§ See *Elektrotechn. Zeitsch.*, vol. 41, p. 894.

‖ Patent No. 18,870 of 1902.

lower than 0·85 they find it possible to make a machine hold its voltage from no load to full load. In the great majority of cases in which alternate current generators are used, such as cases (1), (2), and (3), given at the beginning of this paper, the power-factor is higher than 0·85, so that a compound alternator without commutator or any com-

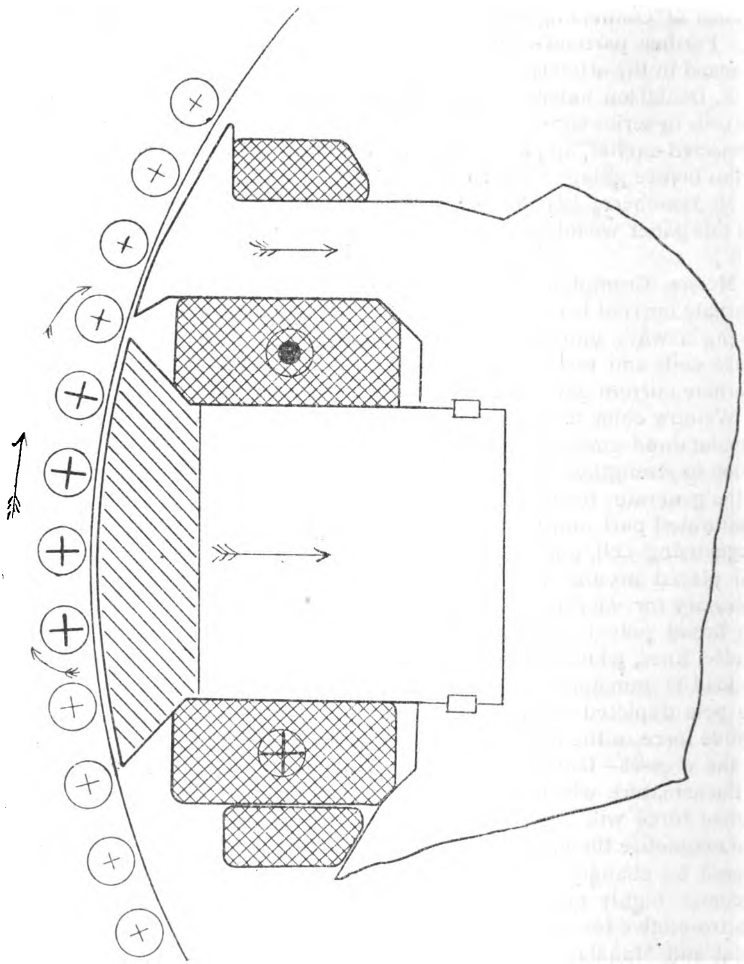


FIG. 25.—Author's Method of Compounding A. C. Generators.

plications can be supplied. The armature, in increasing the E.M.F., is instantaneous in its action. In cases where the power-factor ranges between 0·9 and 0·95, as in a great number of lighting stations, considerable over-compounding can be guaranteed. The following are the results of a test made on a 150 k.w. three-phase generator, 415 to 440 volts, 25 periods, 500 r.p.m. The generator was belted to a shunt wound D.C. motor, and loaded on a rack of iron wire (power factor

0·97). With 28 amperes in the main field coils, and no current in the auxiliary field, the voltage was 416 at no load. On throwing on a load of 150 k.w., and correcting the speed, the voltage rose to 445 volts, showing a compounding of 6 per cent. between no load and full load. On another test the main field current was diminished and the voltage brought up on the auxiliary field winding. The rise in voltage on load was not so great as before. By fixing the current in the two windings at a suitable figure, it was found possible to make the machine hold its voltage exactly between no load and full load. The generator was then loaded on a 100 k.w. rotary converter through transformers. The rotary ran well in parallel, loaded and unloaded, at power-factors varying between unity and 0·7. The synchronising current could be reduced to a value too small to read upon the instruments by adjusting the field of the rotary. When the rotary was loaded so as to produce full load in the generator with unity power-factor, a rise in voltage of 10 per cent. was obtained between no load and full load. At a power-factor of 0·87 the voltage was maintained constant at all loads. The drawback to this method of compounding is that as the power-factor decreases the magnetising effect of the armature also decreases, but this objection is not so serious as might at first be supposed, for in the majority of cases the generator is carrying all day the greater part of its non-inductive load. Take the case, for instance, of a station supplying principally incandescent lamps. All day the generator has to supply the magnetising current to transformers, and when the load comes on suddenly in the evening the power-factor increases, so that a generator of the type described would have exactly the right kind of regulation. Similarly, where a generator is loaded with induction motors running light, a load on the motor increases the power-factor, and again the generator behaves well. In cases where sudden and large draughts of power at low power are required, this type of generator would not be suitable.

It may appear at first sight that a generator with a field magnet shaped as shown in Fig. 25, would not have a good E.M.F. wave form. It has been found that by proper design the wave form at full load can be made to approximate very closely to a sine wave, in fact, more closely than the wave form of the old simple type generator.

At no load the wave form will not depart much from the true sine wave, but will have a slightly peaked form—the form, in fact, which has been found to be most economical with transformers working on light loads.

DISCUSSION.

Mr. M. B. FIELD: The author is to be congratulated upon his attempt to devise a method whereby the defect of sluggishness in the response of the alternator to variations in the load conditions is remedied. Mr. Field.

One prominent feature of the author's device which differentiates it from all others is its radical dissymmetry. In the devices previously described in the paper, the only dissymmetry which exists may be said to be solely that due to phase-rotation—a dissymmetry common to all

Mr. Field. multiphase systems, and one which in no way precludes the possibility of rotation in either direction. With the author's device, on the other hand, rotation in one direction only is possible if the required result is to be achieved, and although from a practical point of view this can hardly be urged as a serious disadvantage, it is a point to be borne in mind. Compensation is effected by the armature currents magnetising an auxiliary pole not encircled by the main field magnet winding, in the same direction as the main pole of which it forms a part. Now the voltage of an alternator drops heavily on a lagging current, because this current tends to demagnetise the field. A lagging current, therefore, instead of magnetising the auxiliary poles in the positive direction will magnetise them actually in the negative direction, and we should expect the curious result that whereas a compensation or compounding is the result of the presence of the auxiliary poles in a fairly non-inductive load, an actual decompounding will be the result in a heavily lagging current; that is to say, the regulation of the alternator will be actually worse than if no such auxiliary poles existed at all. It further appears as though the effect of the auxiliary pole in raising the voltage when the machine is supplying a leading current will not be very marked. If the auxiliary pole is so situated that with unity power-factor it becomes strongly magnetised, then if the lead of the current be increased, the magnetisation of the auxiliary pole will, I think, first be strengthened and afterwards decrease.

The question of hunting is an important one. I gather from the paper that the device is still more or less in the experimental stage. It is one thing to assure oneself that a single generator will run well with a rotary without the system surging, and quite another whether two or more of such machines will work in parallel comfortably.

It is, I believe, usually considered that the shifting of the magnetic flux backwards and forwards across the pole face is a fruitful source of hunting, hence the forms of amortisseurs employed. Now the introduction of the auxiliary pole, unmagnetised at no load and magnetised at full load, appears to me (and I hope to be corrected if wrong) to favour this circumferential shift of magnetic flux. We may say that the breadth (circumferentially) of the pole varies with the load ; at no load the breadth is that of the main pole, at full load it is the total breadth of main and auxiliary pole. Under certain conditions when the auxiliary pole is magnetised in the opposite direction to the main pole, the effective breadth may be even less than that of the main pole alone. If a surge of current suddenly take place the effective breadth of pole changes, and the centre of gravity of the magnetic flux emerging from the pole (if we may make use of such an expression) is suddenly shifted. A sudden shift of the magnetic flux will be accompanied by a jerk on the field magnet system exerted by the armature currents. Now consider two such generators working in parallel together on load, and suppose a cross current suddenly started; the c.g. of the magnetic flux will be thrown suddenly forward in the one machine and backward in the other, so that we might expect a positive jerk exerted on the one field magnet and a negative jerk on the other—jerks of opposite signs. If the surge reversed, the jerks would

reverse. Now although this action occurs in all machines to a greater or less extent, the presence of the magnetically sensitive auxiliary pole will augment the action very largely. On the other hand, I imagine amortisseurs are not permissible, as they would introduce sluggishness of response. Consider a three-phase generator. The armature currents constitute a wave of current rotating with the same speed as, and therefore relatively at rest to, the field magnet system. Their effect, therefore, in magnetising or demagnetising the field system is almost identical with that of the field winding; in fact, the armature currents relatively to the field system may almost be considered in the same light as the compound winding of a D.C. compound-wound generator. If now the auxiliary poles be solid or supplied with amortisseurs, it appears to me that when a sudden change of armature current occurs, the poles will behave as though their reluctance was initially high but decreased in the course of a few seconds to the normal value. If we are justified in looking at the effect of armature currents in the light of a compound winding as indicated above, it would seem that the ordinary alternator compounded by means of series coils on the field magnets through which a rectified current flows (provided the magnet system be laminated) would be no more sluggish than that proposed by the author. Mr. Field.

It would be interesting to know more about the wave shape under differing load conditions and power-factors; perhaps the author will be good enough to include such information in his reply. The wave shapes on a low power-factor when the auxiliary pole is magnetised in the opposite direction would be of particular interest.

Lastly, a point which I admit does not fall directly under the scope of the paper except as a matter of comparison. I refer to independent potential regulators—not automatic devices acting on the alternator field rheostats, but direct automatic potential regulators. Imagine as a datum of reference, the pull of a solenoid energised by a few accumulator cells. To this is referred the pull of solenoid energised from the A.C. 'bus-bars. The two solenoids in combination operate a relay which operates an I.R.T. potential regulator. Similar arrangements have been tried, with what success I do not know. The advantages are manifold: we should have a device independent of the generator, which could be cut out of circuit immediately if desired. It would permit of easy adjustment of the degree of compounding or overcompounding, would not necessarily involve the sluggishness inherent in other methods, and would be applicable to any system employing any type of alternators.

Mr. WALKER (*in reply*): Mr. Field has referred to the fact that my alternator can only regulate well when rotated in one direction. This is perfectly true, but it is no disadvantage, because alternators never require to have their direction of rotation reversed. Engine builders build their engines to go in one direction only; it is a simple matter to make the direction of rotation of the generator the same as that of the engine. It is true that the lagging component of the current tends to de-magnetise the unsaturated pole, while the working component tends to magnetise it. Now, in most power-stations the power-factor at heavy loads is above 0·9, and at that power-factor the magnetising Mr. Walker.

Mr. Walker. effect is stronger than the de-magnetising, so that when the load comes on the voltage rises. There are, of course, cases in which generators are subject to large sudden demands at a low power-factor. This type of generator would not be suited for such cases. For power-factors as low as 0·75, the generator regulates better than a 6 per cent. regulating machine. For power-factors above 0·9, it is over-compounded. For very low power-factors the drop in voltage is not very great, because the unsaturated pole, even when carrying a negative flux to its full capacity, cannot lower the voltage more than 10 per cent., and when carrying a positive flux to its full capacity it cannot raise the voltage more than 10 per cent.

In reply to Mr. Field's question about the short-circuit current, I can give the following particulars :—

A 150 k.w. generator takes 33 amperes in the field to give normal voltage at no load and full load. When run on short circuit it requires 11 amperes in the field to give full armature current. Theory shows that this is about the exciting current one would expect if there were no auxiliary pole. The short-circuit current in lagging 90 degrees tends to de-magnetise the main pole, but the auxiliary pole is almost at right angles in phase to the main pole, and is not much affected.

I will leave Mr. Field's arguments about the parallel running to be answered by the machines themselves. They run in parallel perfectly, and the reason is not difficult to see. One of the most important factors in good parallel running is a powerful field magnet. This we have in the main saturated pole, which yields a very stiff field with a high flux density in the gap, so that a comparatively small synchronising current yields a great synchronising torque. The flux from the unsaturated part is an unimportant fraction of the whole; it may be

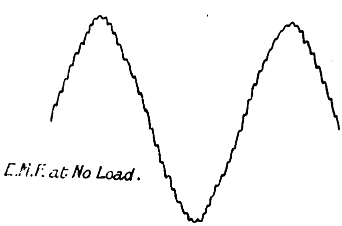


FIG. A.

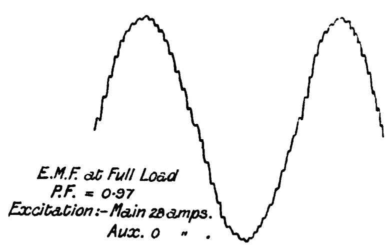


FIG. B.

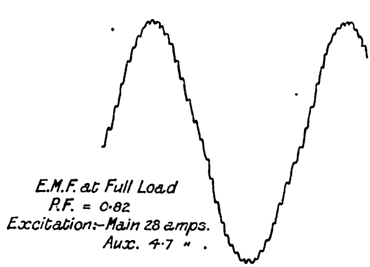


FIG. C.

Mr. Walker.

compared with the cross flux which occurs in all generators. This cross flux may be regarded as self-induction flux of the armature, and self-induction, if not too great, is a useful factor in parallel running. Amortisseurs will not in general be necessary. If they are required, they will be placed on the main pole, where they will not prevent the instantaneous rise of the auxiliary flux. As the main pole shifts from a lagging position to a leading position with regard to the armature current, there is, notwithstanding the saturation, sufficient change in the distribution of the flux to induce powerful currents in an amortisseur.

In reply to Mr. Field's question about wave forms, I give in Figures A, B, and C the wave form of a 150 k.w. generator run at no load, full load unity power-factor, and full load power-factor 0·8 respectively. These curves were traced on an oscillograph. On this machine there are 18 slots per pole, and it will be seen that each slot shows itself on the wave form. This defect will be eliminated in future designs.

MANCHESTER LOCAL SECTION.

HIGH-TENSION SWITCHGEAR.

By LEONARD ANDREWS, M.I.E.E.

(*Paper read at Meeting of Section, December* 13, 1904.)

Although it is barely three years since Mr. Clothier published his excellent paper on Central Station Switchgear, rapid advances have been made since that time, and high tension switchgear is at last receiving its proper share of attention. Instead of being crowded into any small space that happens to be available, it is now recognised as one of the most important sections of a station equipment, and almost unlimited space is devoted to it. One or two serious accidents with high-tension switchgear of compact design have made engineers realise that, efficient and convenient though such switchgear has shown itself to be under normal conditions, the margin of reliability must be further increased in view of the paramount importance of preventing interruptions in the supply of current. An excellent example of modern practice in high-tension switchgear is to be found in the Carville Generating Station at Newcastle. This system was fully described in the paper by Messrs. Merz and McLellan, read before the Institution in London in April, 1904.

The most revolutionary departure from former practice, to be found in nearly every switchboard that has been erected during the past year, is the dividing up of the controlling system into a number of entirely separate units, each section being so isolated as to render a spread of fire from one unit to another practically impossible. This entails the operation of switches placed at a distance from a central operating panel or keyboard.

Mechanical v. Electrical Remote Control.—Whilst the necessity of some type of remote control is generally recognised, considerable difference of opinion exists as to whether it is best to effect this operation electrically or mechanically. American practice has tended towards the almost universal adoption of electrical control, and the number of large installations working successfully under this system at the present time are indisputable testimonies to the care and forethought that must have been expended by those engineers who have developed this system. Whilst, however, we are bound to admire the ingenuity that has been displayed, we should at the same time consider whether the complication that is of necessity involved is fully justifi-

able. The multiplicity of small wires, the magnetic clutches, the numerous springs, and the push button, relay and commutator contacts all constitute weak points in a system, the reliability of which is dependent upon its weakest link.

On the Continent the tendency has been to favour mechanical control. An excellent example of this is the switchgear controlling the 13,000 volt generators, etc., at Paderno, which has now been in successful operation for about seven years.

One argument that is sometimes used in favour of electrical control is that a large mechanically operated three-phase switch cannot be closed sufficiently quickly for synchronising. Although this argument may apply when dealing with very large switches controlling 5,000 k.w. generators, such as are being used in some of the American stations, it certainly does not apply to 1,000 k.w. generators or under. The switches for these can be closed mechanically quite as quickly as an electrically operated switch. Again, we are told that mechanically operated switches must be tripped electrically, and since they are electrically controlled to this extent they might as well be operated electrically entirely. It must, however, be apparent to any one who considers the question that merely to trip a switch that is closed by hand is a very simple problem compared to that of constructing a switch to be opened and closed electrically. Another advantage claimed for electrical control is that it enables the high-tension switches to be placed directly opposite the generators they control, whereas the operating keyboard can be compactly arranged in a very small space, but even this is a doubtful advantage.

Think of the number of small wires that need to be run from each of these distant switches to the operating board for the ammeters, synchronisers, voltmeters, etc., and for operating the motors controlling the main switches. Even for a single-phase system the complication is appalling. Then again, think of the length of high-tension 'bus-bars required for this arrangement. On the whole, it seems much better practice to bring the main cables from the generators to one point of the building, at which centre all the high-tension switches are placed.

In some cases it may be required to place the high-tension switches in a separate building so far removed from the engine-room that mechanical control from an operating board fixed in the engine-room would be extremely difficult. Electrical operation is certainly more satisfactory for controlling switches placed a long distance from the operating board.

This, however, raises another question. Is it necessary, or even advisable in all cases, to place the operating board in the engine-room? It certainly has been the custom to fix the board in such a position that the switchboard attendant can obtain a full view of all the plant in the engine-room; but is not this custom a relic of the days when generating stations were small and the switchboard attendant was also the engineer in charge of the shift?

In all installations where the switchboard attendant's only duties are to manipulate the switches, there is much to be said in favour of placing him in a position where he will be guided in his operations solely by

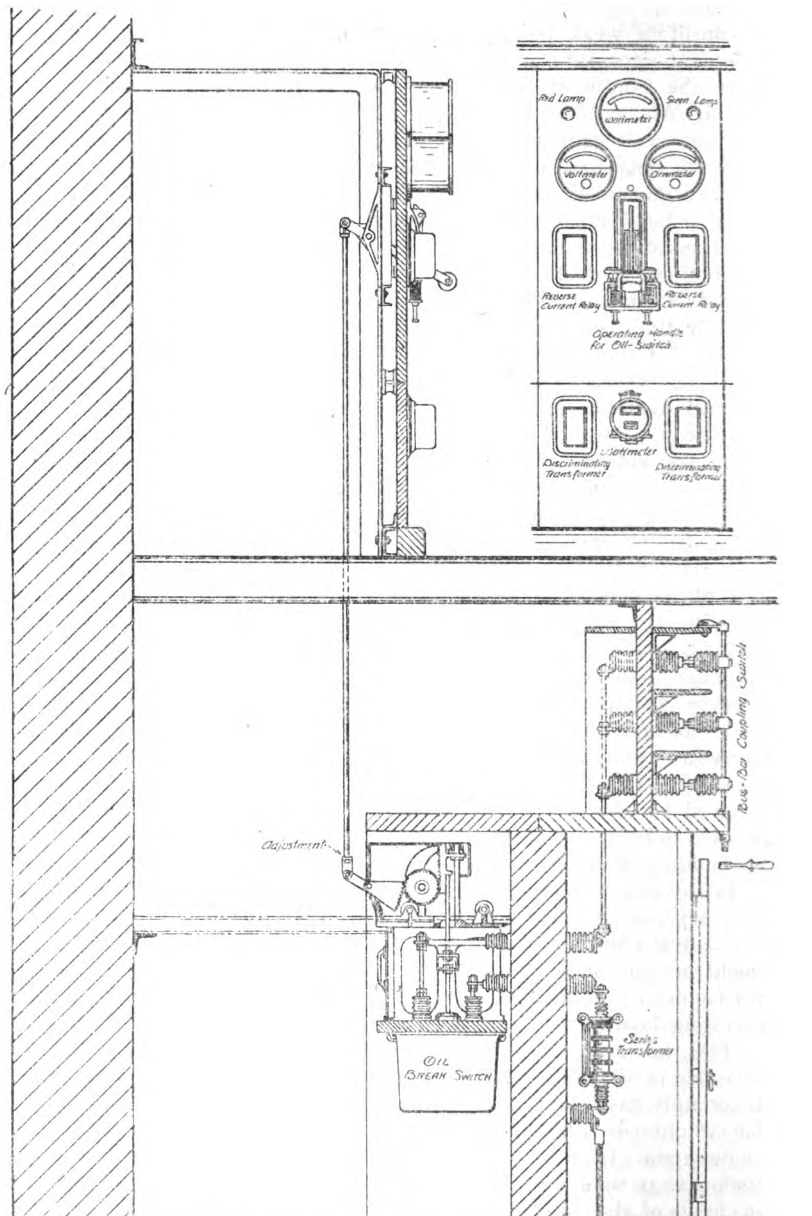

Red Lamp
Wattmeter
Green Lamp
Voltmeter
Ammeter
Reverse Current Relay
Reverse Current Relay
Operating Handle for Oil-Switch
Wattmeter
Discriminating Transformer
Discriminating Transformer
Bus-Bar Coupling Switch
Adjustment
Series Transformer
Oil Break Switch

FIG. I.

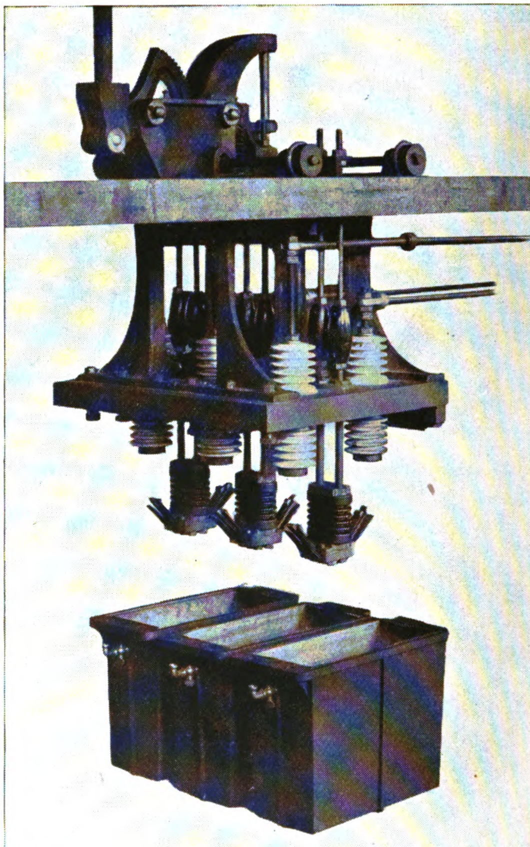


FIG. 2.

the reading of the indicating instruments, and will not have his attention distracted by bursting steam pipes, broken connecting-rods, armatures, flashing to pole pieces, or any of the other exciting incidents that are apt to occur in the best regulated stations.

So far we have considered the question of mechanical versus electrical control in the abstract only. A brief description of the 6,000 volt mechanically operated boards, now in course of erection at Poplar, will show how the problem has been dealt with in this case. Fig. 1 is a sectional view of the high tension board in the main generating station. The construction and arrangement of the high tension boards in the sub-stations are precisely similar. The three-phase high tension oil-break circuit-breakers are fixed in brickwork cells on the ground floor. Each unit or section is by this means efficiently isolated from adjacent sections, and a spread of fire from one section to another is rendered practically impossible. As this confining in narrow brick cells is apt to make some of the back connections inaccessible (and the importance of accessibility cannot be over-rated), provision has been made for withdrawing each circuit-breaker from its cell for overhauling, cleaning, and inspection. For this purpose the circuit-breaker is mounted on rollers, which rest on brackets fixed to the partition walls of the cells. Extension rails are provided, along which the circuit-breakers may be withdrawn. Fig. 2 is reproduced from a photograph showing one of these circuit-breakers withdrawn from its cell.

The connections at the back of the circuit-breaker cells, as well as the series transformers from which current for the instruments and relays, etc., is obtained, are also enclosed in brick cells. No high-tension cables are carried into the circuit-breaker cells, the necessary connections being made by copper rods carried through insulators set in the wall.

A detail to which considerable attention was paid in designing this board is the arrangement of the 'bus-bars. Each 'bus-bar is isolated from adjacent 'bus-bars by horizontal slate slabs, and to guard against any risk of an arc being formed that might destroy this slab, and so short-circuit the bars, precautions have been taken which effectually obviate any possibility of the circuit being opened in either of the 'bus-bar compartments. Isolating plugs are provided to enable any circuit-breaker to be completely disconnected from the 'bus-bars before working upon it. These plugs should, of course, not be removed when carrying current. In the event, however, of an attendant withdrawing a wrong plug by mistake, the circuit will not be broken in the 'bus-bar compartment, but on the other side of a thick stone slab. As the back contact-plugs are again isolated from the contacts of other phases (in this case by vertical slate slabs), the damage is bound to be confined to that particular contact. As a further precaution, the plugs are so constructed that they cannot be withdrawn more than two inches from the back-contacts, so that should an arc be started it will not be a long and destructive one, and the attendant will thus be able to replace the plug and open the main circuit-breaker before much damage is done.

Separation of Phases in Circuit-Breakers.—Another question upon

which opinions differ at present is that of the necessity of separating the contacts of one phase from those of opposite phases in the main circuit-breakers. Some engineers consider that if the circuit is broken under oil the contacts of all phases may be contained in one vessel. Others go to the opposite extreme, and divide each phase from adjacent phases by brick walls. In the former case there appears to be some risk that a slight arc started under oil may spread to the other phases, and so establish a short circuit across the 'bus-bars.

Complete isolation by brick walls, of course, provides a very perfect protection against short circuits between phases, but it introduces other difficulties. Two additional 4½ in. brick walls must increase the width of the switch by at least 9 in., and this is sometimes a serious objection; added to which the placing of the respective circuit-breakers controlling each phase in such narrow and deep cells tends to increase the difficulties of getting at the back-contacts and insulators for inspection and cleaning. The fact that the circuit-breakers of each phase must be connected mechanically for simultaneous operation makes it difficult to withdraw one phase at a time. Both of the above difficulties can doubtless be overcome if it is really necessary. It appears, however, that all that is actually required is that the contacts of the different phases should be contained in separate oil-tanks, the latter being insulated from the high tension connections and from earth. The probability is that if this precaution is taken, and an arc is started in any one phase, it will not be maintained, as the circuit will be broken on the other phases. Should it be maintained, however, on two phases simultaneously, it is probable that complete destruction of the switch is bound to follow, even if the precaution has been taken of separating the phases by brick walls.

Discriminating Circuit-Breakers.—It is no longer necessary to draw attention to the fact that neither fuses nor excess current circuit-breakers should be used between generators and the main 'bus-bars, but that reverse current circuit-breakers should be used in this position. Nearly every switchboard that has been erected during the past year has been so equipped. The only danger now is that alternating-current discriminating devices will be brought into disrepute through being misunderstood.

There appears to be a very general idea that any device that tends to move in one direction when influenced by a forward current, and in the opposite direction under the influence of a reverse current, may be used as an alternating-current discriminating relay. Experience has shown, however, that some of these devices are capable of behaving in a most unexpected manner under the abnormal conditions that are likely to arise when a serious breakdown on any part of the system occurs. It is a very great mistake to suppose that the problem is in any way analogous to that of designing a direct-current cut-out. In the latter case we have a definite sense of direction to deal with, but with alternating currents the only basis to work upon is that of a displacement of phase between the current and the E.M.F.

Now, as long as a generator is supplying current to feed a non-inductive load the phase relations between current and E.M.F. will

be as indicated respectively by the curves I and E, Fig. 3, and by compounding these curves we obtain the curve W representing the torque or pull due to these combined forces. This pull may obviously be used to hold the cut-out in its closed position. If now one of a number of generators working in parallel should fail—due, say, to a bearing seizing—current will flow into this generator, tending to drive it as a motor, and this current will be approximately 180 deg. out of phase with the 'bus-bar E.M.F. That is to say, the phase relations will now be as shown in Fig. 4. We see that the direction of the pull is

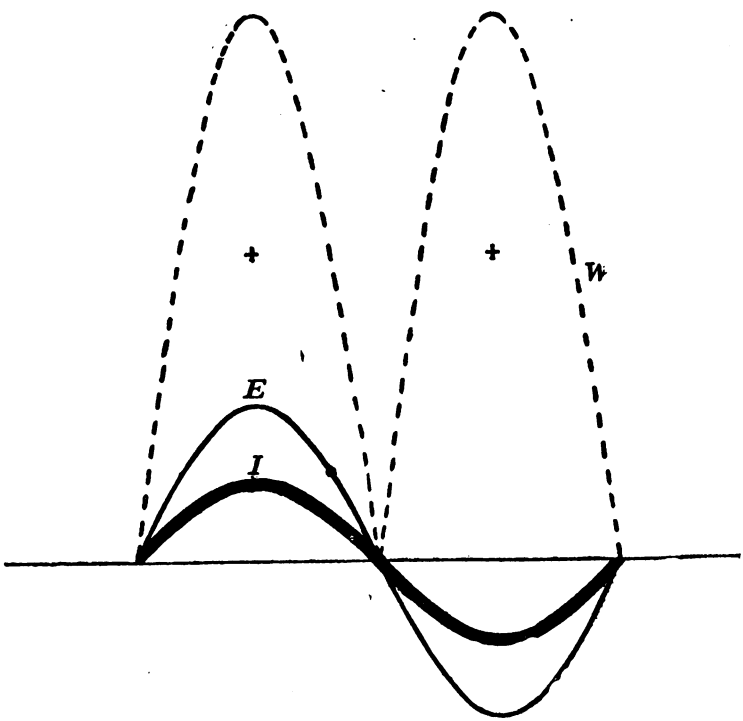


FIG. 3.

now reversed, and it may therefore be used to release the cut-out. So far all is perfectly smooth sailing, and there are any number of methods of constructing cut-outs to deal with these straightforward conditions.

Let us now consider what happens when the field of an alternating-current generator, heavily loaded and connected in parallel with other generators, is open circuited. If the faulty generator could run out of step its speed would increase, and the governor would cut off the steam and so reduce the work on the engine ; but as the generators remain coupled in parallel when the field is opened the faulty generator cannot

race, and consequently its governor is unaffected and the steam supply to the engine is maintained. We have, therefore, two forces to consider: First, an armature in a failing field tending to short-circuit and consequently *to absorb power from the 'bus-bars*; and secondly, *to supply power to the 'bus-bars*. The net result of this combination is a heavy leading current in the faulty machine tending to maintain its field and thereby keep the generators in step. If, however, the faulty machine is not immediately isolated from the 'bus-bars, a heavy synchronising current will flow between the faulty and healthy generators,

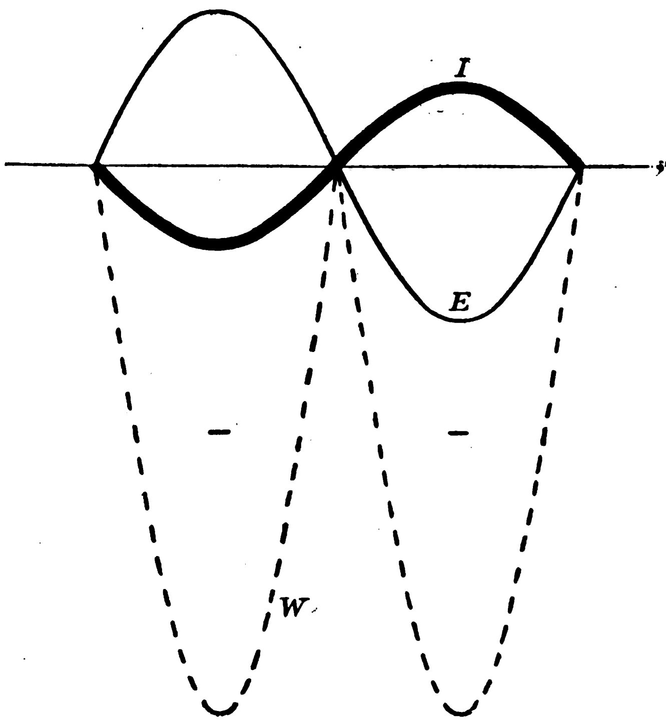


FIG. 4.

and the pressure will rapidly fall, and a complete shut-down will generally ensue.

The phase conditions when a field fails may be represented by the curves shown in Fig. 5. It will be seen that the current curve is approximately 90 *deg. in advance* of the E.M.F. curve, and the pull or torque is alternatively positive and negative, and being just equal and opposite, *their combined effect is nil for any current*.

The following incident may be mentioned to show how very apt one is to jump to a wrong conclusion when testing alternating-current discriminating devices. Between nine and ten years ago we had all our generators at Hastings protected by reverse-current circuit-breakers

of a type that appeared at that time to satisfy every condition likely to arise. These devices were frequently tested by shutting the steam off a generator working in parallel with others, and by opening the field switch under similar conditions. As, however, the governing of the engines was not good, we always took the precaution of shutting off the steam just before opening the field switch. This was done solely to prevent the engines racing, and it never occurred to us that we

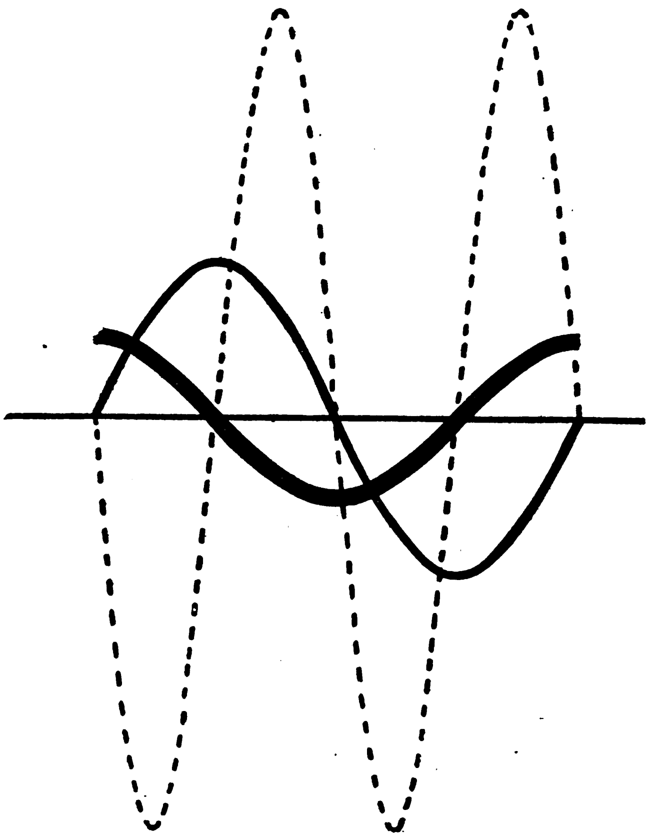


FIG. 5.

were in any way faking the test. We remained in this blissful state of ignorance for close on two years, during which time the only failure that the cut-outs had to deal with was the accidental closing of an exhaust valve of an engine driving a generator working in parallel with others. On this occasion the cut-out saved the situation. A little later we had occasion to test a cut-out controlling a generator that was perfectly governed; we in consequence opened the field circuit, leaving the steam full on to the engine. To our consternation

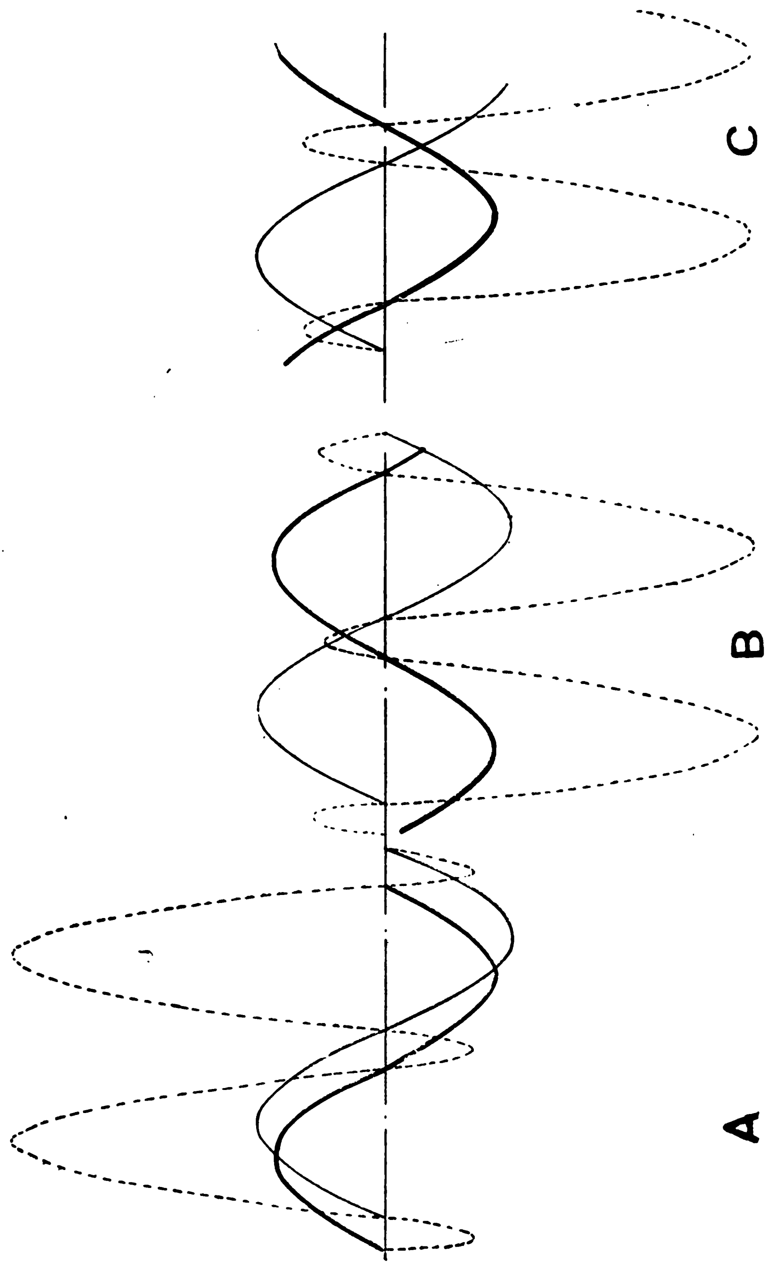


Fig. 6.

we got an enormous current, but the cut-out showed no tendency to operate. We then realised that a device designed to deal with a reverse current, due to a failing prime mover, was useless for isolating a generator having an open-circuited field.

We, of course, appreciated the fact that it was impossible to correct the phase of the series current, and consequently that the only means of obtaining a displacement of more than 90 deg. between the current and the E.M.F. was by permanently shifting the phase of the current in the E.M.F. or shunt-winding of the operating device. This gave us a displacement of phase of approximately 180 deg. between the current and E.M.F. winding upon opening the field. Under these conditions the cut-out worked perfectly, but now failed to operate with a reverse current due to a failing prime mover. It was evident that a compromise was necessary if one device was to be used to deal with the two conditions. We therefore adjusted the inductive resistance in series with the shunt circuit until the shunt circuit lagged about 45 deg. when the series current was feeding a non-inductive load. This adjustment had the effect of altering the phase relations in the cut-out device to that shown by the respective curves in Fig. 6, "A" representing the conditions when the generator controlled was feeding a non-inductive load, "B" when the prime mover failed, and "C" when the generator field was open circuited.

We have always felt that no discriminating circuit-breaker that failed to comply with the following conditions could be considered a satisfactory protective device :—

(*a*) An alternating-current discriminating cut-out must not be released by a low resistance short circuit beyond the cut-out.
(*b*) It must not be released by a current supplied to a highly inductive load beyond the cut-out.
(*c*) It must not fail to be released should the prime mover driving the generator controlled be pulled up by a hot-bearing or similar failure.
(*d*) It must not fail to be released should the field of the generator controlled break down.
(*e*) It must not fail to be released by a very heavy reverse current, even should the pressure across the 'bus-bars drop to as low as 20 per cent. of normal pressure.

There are other conditions equally important, but it will generally be found that a device that complies with all the above is also capable of dealing with the other conditions.

To show how extremely difficult it is to design an alternating-current device to comply with all of these requirements, we have connected up to the College alternating-current supply seven discriminating devices, each one of which depends for its action upon an entirely different principle to that of the others. Connected in series with the main current winding of each of these devices is an ammeter and a special phase indicator, which shows whether the current is in phase with the E.M.F., lagging or leading, approximately 90 deg. on the

E.M.F., or 180 deg. out of phase with it. A voltmeter across the E.M.F. winding indicates the 'bus-bar pressure. The construction of these various devices is shown respectively by Figs. A, B, C, D, E, F, G.

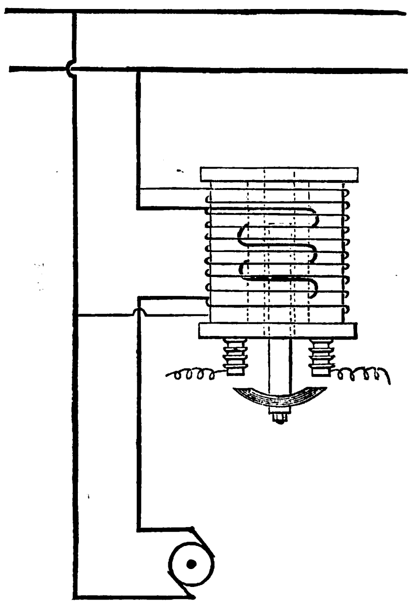


FIG. A.

As the principles upon which the majority of these devices work have been explained in previous publications* it is unnecessary to give more than a brief description of each of them here.

(A) is a simple compound wound solenoid, the thick winding of which is connected in series with the generator to be controlled, the shunt winding being connected across the 'bus-bars. When the current in the series winding is in the reverse direction to that in the shunt winding the two forces oppose each other, but when both currents are in the same direction they reinforce each other and lift the core, thus releasing the cut-out.

(B) is a modification of the above device, having the shunt coils wound half in one direction and half in the reverse direction. When generating the series and shunt windings assist each other below the centre of the coil, and oppose each other above the centre; the result being the core is pulled down. When motoring the field above the centre is strengthened and that below is weakened and the core is pulled up.

(C) is based upon the principle of a shunt-wound motor, the armature of which tends to rotate in one direction when the field is excited by a generating current, and in the opposite direction when it is carrying a motoring current.

(D) is a multipolar device, having a swinging double horseshoe armature, which is polarised by the shunt winding surrounding its centre limb. Its field is excited by a series winding, consisting of a zig-zag casting threaded between the fixed poles. A generating current tends to produce like poles in the fixed field above the armature poles, and unlike poles below; the armature is consequently drawn down. A motoring current reverses the direction of the pull.

(E) consists of a swinging inverted T-shaped pendulum polarised

* *Proc. N.S.E.E.*, Jan., 1897; *Journal I.E.E.*, vol. 27, May, 1898; *Journal I.E.E.*, vol. 32, Jan., 1903; *Electrician*, vol. 53, May 13, 1904; *Electrical Review*, vol. 54, June 3 and 10, 1904; *Electricity Control*, by L. Andrews (Published by Griffin & Co.).

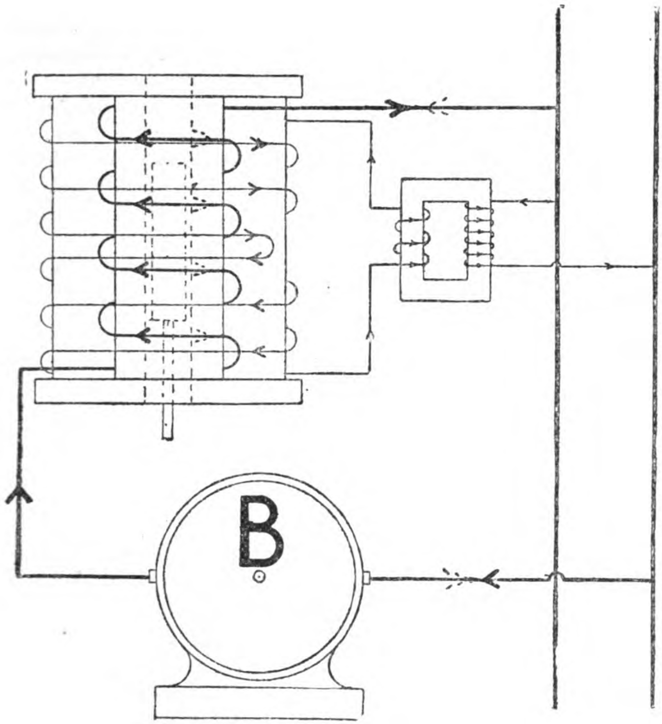


FIG. B.

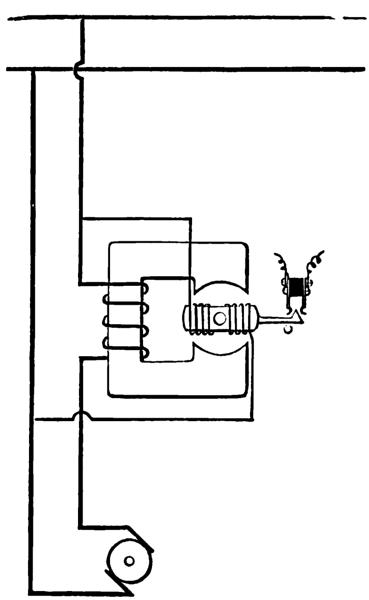


FIG. C.

by the shunt winding. The free ends of the pendulum are encircled by series coils which tend to attract it in one direction with a generating current and in the reverse direction with a motoring current.

(F) is a type of release which we have found most successful for direct-current circuits. It consists of a rectangular closed iron circuit bridged by a movable core. A winding on one limb carries the series current, and the shunt winding is placed on the opposite limb. These windings are both connected up to produce a magnetic flux in the same direction when in phase with each other, but both produce fluxes in opposite directions when they are out of phase. In the latter case the combined fluxes are diverted through the movable core, which is drawn up, and the cut-out released.

(G) is a simple differential solenoid, the two windings of which

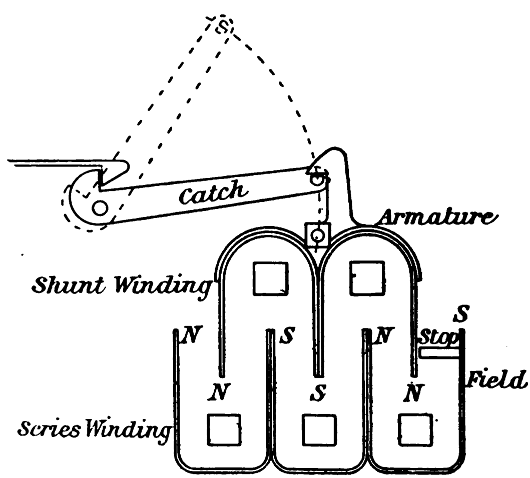


FIG. D.

are connected in parallel across any low-tension source of supply A—B, excited from the main 'bus-bars. These windings are paralleled on one side through a small equaliser, upon which is wound one turn of the cable between the generator to be controlled and the main 'bus-bars. So long as there is no current in this series turn the current from the low-tension source of supply divides equally between the bottom and the top winding of the differential solenoid, and its core is in consequence neither pulled up or down, but is held down by gravity. A generating current in the series turn will destroy the balancing effect of the equaliser, causing more current to flow through the lower coil and less through the upper coil of the differential solenoid, with the result that the core will be held down magnetically as well as by gravity. A reversal of the series current will induce more current to flow through the upper coil and less through the lower one, thus lifting the core and releasing the cut-out.

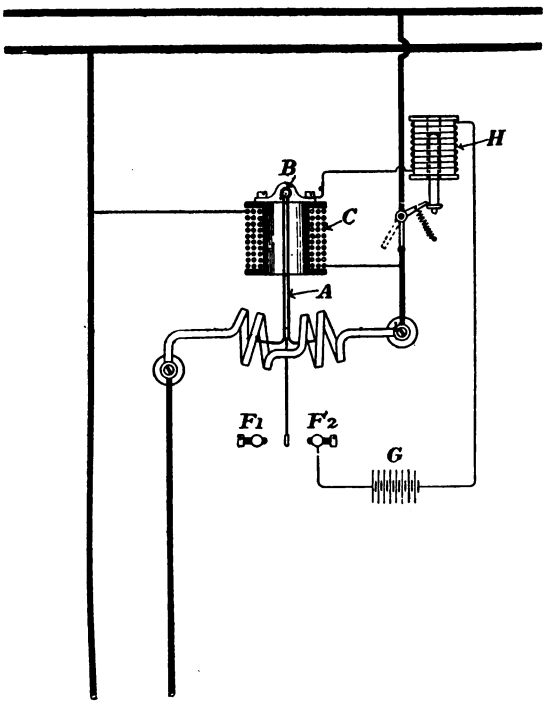

B
C
H
A
F1
F'2
G

FIG. E.

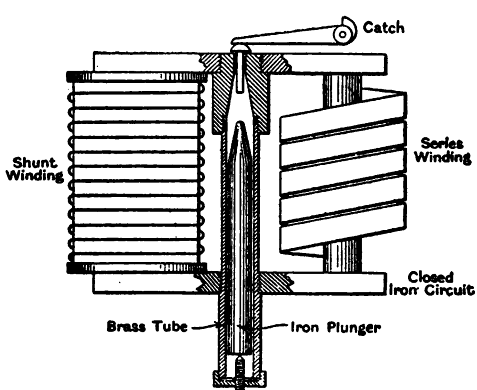

Catch
Shunt Winding
Series Winding
Closed Iron Circuit
Brass Tube
Iron Plunger

FIG. F.

Having now briefly described the various devices to be tested, we will see to what extent they comply with those conditions which every alternating cut-out should fulfil.

If a non-inductive resistance is inserted in series with all the cut-outs and the phase indicator, the latter shows that the current is approximately in phase with the E.M.F. Under these conditions none of the devices show any tendency to release.

If now the direction of the series current relatively to the shunt current is reversed, the phase indicator will show a phase displacement of approximately 180 deg., and all the cut-outs will operate.

Up to this point all the devices appear to be equally satisfactory reverse-current devices. It is not until we reproduce conditions such as we have to deal with in practical working that difficulties begin to arise. One of the first tests that every discriminating cut-out should be submitted to is that of making it carry a heavy excess current in a forward direction. This corresponds to a short circuit on the feeders or distributing system; and it is obvious that a generator cut-out should on no account open under these conditions. On submitting our various cut-outs to this test we find that the devices B, E, D, E, and G are all unaffected by a non-inductive short circuit beyond the cut-out, whereas A and F fail under this test. The failure of A is due to the heavy current in the series winding entirely overpowering the effect of the current in the shunt winding. F fails because the heavy current in the series winding induces an E.M.F. in the shunt winding in the opposite direction to the applied E.M.F., and thus reverses the direction of the current in this winding.

The next condition to be dealt with is that of a highly inductive load beyond the cut-outs. This corresponds to switching on a feeder having a number of large standing motors connected to it. It may be assumed, for instance, that the circuit-breaker of the feeder in question has been accidentally tripped, and that all the motors connected at the time have been brought to a standstill. If these motors are not provided with no-load release cut-outs (and alternating-current motors seldom are), they will take a heavy current when the feeder is switched on, which will lag considerably behind the E.M.F., and unless the cut-outs controlling the generators are free from the defects referred to, every generator working at the time is liable to be cut out of circuit. To demonstrate that some types of cut-outs possess this defect, a non-inductive load is again connected in series with the cut-outs, and without reversing the direction of the current the non-inductive resistance is gradually replaced by inductive resistance. This has the effect of causing device D to operate as it would do if the current was actually reversed. It is evident, however, that no reversal has taken place, as the circuit has not been opened, added to which none of the other devices show any tendency to reverse. This defect appears to be due to the peculiar configuration of the magnetic circuit. It shows how necessary it is that the design of the magnetic circuit should be perfectly simple.

The failure of a generator field is one of the most difficult accidents to deal with, and is at the same time one of those most liable to occur.

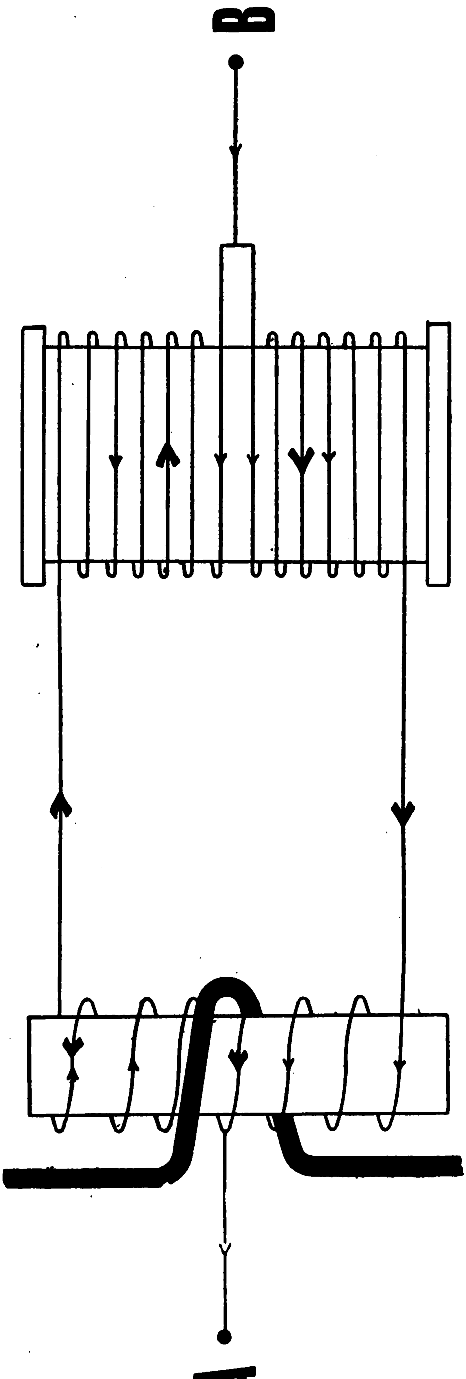


Fig. G.

It may obviously be caused by a failure of an exciter, a break in a field-rheostat, a short circuit across collector rings, or by a short circuit or failure in the field winding. If the load in series with the cut-outs is adjusted to reproduce as nearly as possible the displacement of phase that occurs when a generator field breaks down, all the cut-outs will fail to operate, with the exception of G, which has been specially designed to deal with a fault of this description.

Many types of discriminating cut-outs that will operate reliably on a reverse current equivalent to 10 per cent. of the full-load current fail to operate on a very heavy reverse current accompanied by an appreciable drop of pressure, such, for instance, as may be caused by a serious smash-up of an engine. Upon reducing the non-inductive resistance in series with the cut-outs to give a heavy current, and at the same time reducing the 'bus-bar pressure and finally switching this heavy reverse current on suddenly, A, B, D, F, and G all operate satisfactorily, but C and E fail. This failure is in each case due to the reduced reluctance effect in the cut-out device overcoming the polarity effect.*

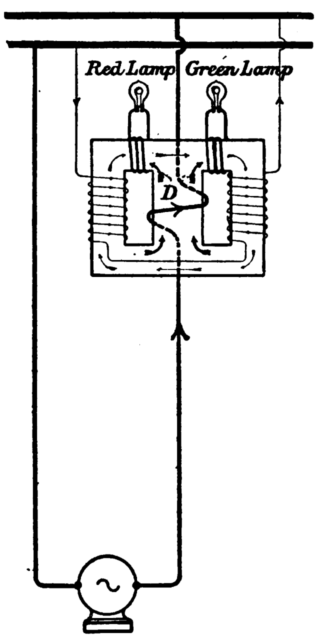


FIG. 7.

The foregoing results show that there are so many known pitfalls to be avoided in designing alternate-current cut-outs that one is inclined to wonder whether there are many others that have not yet been discovered. Whilst it would be rash to declare definitely that there are no others, we feel very confident that the only danger lies in the use of devices that embody some of the defects referred to above. It has been shown that if some perfectly simple device is used, such as G, which is entirely free from these defects, its behaviour is exactly what we should expect it to be under all the widely different conditions that it has been submitted to. Some of you will doubtless recognise this device as being a modification of the discriminating-transformer shown in this room about two years ago (see Fig. 7). It possesses, however, two important features which were not to be found in the original device. In the first place only two windings are required and one magnetic circuit, as compared with the four distinct windings and two magnetic circuits of the device shown in Fig. 7; and in the second place the two windings can be arranged concentrically, thereby entirely overcoming all magnetic leakage troubles.

It may very reasonably be asked if we have experienced any troubles with the double magnetic circuit discriminating transformer, and we

* See *Electrical Review*, June 3 and 10, 1904; *Electrician*, May 13, 1904.

must admit that we have not. We have, however, learnt from experience that it does not by any means follow that a cut-out which works satisfactorily for a long period covers every possible condition which may arise. The abnormal conditions occur so very occasionally that one may closely watch the behaviour of a cut-out for years before discovering that it does not meet every remote requirement. In designing apparatus of this class it is necessary to anticipate troubles, and not to merely wait for them to occur.

Now we have seen that the majority of failures in early types of cut-outs were due either to defects in the design of the magnetic circuit or to abnormal phase displacements. The phase displacements between the main series currents and the 'bus-bar E.M.F. we cannot prevent, but it has been shown that with these alone we can deal. We should be careful, however, to avoid any risk of superimposing upon these natural phase displacements any artificial distortions or displacements of phase that may occur in the protective device itself.

It will be interesting to ascertain what will be the phase and the shape of the E.M.F. curve of one of the secondary windings of the discriminating transformer shown in Fig. 7. This E.M.F. is induced by two independent currents, that may or may not be in phase with each other, added to which the various primary and secondary windings cannot all be wound concentrically with each other, therefore magnetic leakage has to be considered, and finally there are two inter-connected but distinct magnetic circuits to be taken into account. Altogether it is extremely difficult to calculate what the effect of this combination would be under the various conditions that may arise. We can, however, ascertain it experimentally with the greatest accuracy by means of the oscillograph.

Fig. 8 is reproduced from a set of oscillograms obtained from a discriminating transformer of the type shown in Fig. 7, in which the defects referred to above were deliberately exaggerated. "A" is the curve of the current in the series winding, "B" is the 'bus-bar E.M.F. curve, and "C" is the E.M.F. curve of one of the secondary windings. It will be seen that even when A and B are approximately in phase with each other the phase of "C" is displaced and distorted to such an extent that it is impossible to trace in it any resemblance to the primary curves. One curious effect of the combined influences is that the periodicity of the secondary current is approximately trebled. So far as we have been able to ascertain the behaviour of the cut-out is not affected by these distortions of the secondary current, and as in this particular device the release is dependent upon the mean value of the current in the operating solenoids connected across the secondary windings rather than upon the phase of this current there appears no reason to anticipate difficulties. The curves are merely given here to emphasise the necessity of simplicity in reverse-current cut-out design.

The absolute simplicity of the device, illustrated by Fig. G, is a refreshing contrast to the complex conditions referred to above. It appears impossible to imagine any unforeseen difficulties arising in this case. Here we have only two influences to consider. In the first

place we have a simple series transformer, the secondary of which is closed through two solenoids connected in series; and in the second place we have two parallel circuits of equal ohmic and inductive resistance connected across a source of potential supply. When the induced secondary potential and the applied potential are in phase with each other they will oppose each other in the upper winding of the differential solenoid and reinforce each other in the lower winding. When the respective potentials are 180 deg. out of phase with each other they will be in opposite directions in the lower and in the same direction in the upper winding.

The worst condition to be dealt with is that of a 90 deg. displacement of phase between the two potentials. In this case the effect upon the

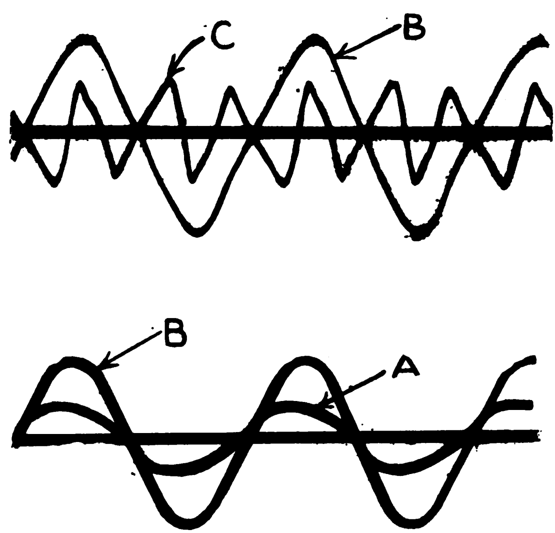


FIG. 8.

differential solenoid will be the same as if both coils were connected in series with the common return of a two-phase system. Two independent currents lagging 90 deg. behind each other will flow through both coils, and as they will be of the same value in each coil, the core will be neither pulled up or down. This defect may obviously be corrected by inserting an inductive resistance in series with the potential supply A—B, as suggested earlier in this paper. Another method of providing a cut-out that will take care of either a failing prime mover or a failing field is to use a double-barrelled relay, consisting of two differential solenoids connected in parallel—a non-inductive resistance being inserted in series with one and a highly inductive resistance with the other. By this means the necessity of making adjustments, in each case to determine the best mean point for both conditions, is entirely avoided.

Another type of discriminating release, that has been tried to some considerable extent in the United States and on the Continent, is based on the principle of an ordinary induction wattmeter. The use of a wattmeter for this purpose appears to have been first suggested by Dr. Silvanus P. Thompson in the discussion on my paper read before the Institution in 1898. Whilst it may be possible to make a satisfactory relay on this principle, one feels that the function of discriminating cut-outs are so important that it is not wise to rely upon such delicate forces as are available from a device of this nature ; added to which all the wattmeters upon which we have experimented appear to be very susceptible to some of the errors referred to earlier in this paper. It is a condition of the most paramount importance that a release gear should be strong and definite in its action, as thereby errors due to friction, dirt, and stray fields, etc., are dominated by those main controlling forces which should govern its action.

Rule for Determining Uses of Discriminating Cut-outs.—We sometimes find that engineers are a little uncertain as to whether discriminating cut-outs or reverse-current cut-outs should be used in certain positions. The following rule appears to apply to all cases:—

"Where current is fed into 'bus-bars from a number of sources of supply connected in parallel, discriminating cut-outs only should be used. Where current leaves 'bus-bars to supply a number of feeders, transformers, motors, or distributors, excess-current cut-outs only should be used."

Time Limit Attachment for Discriminating Cut-outs.—We are often asked whether a discriminating cut-out should operate instantaneously or whether it should possess some time element. On the whole the advantages appear to be on the side of instantaneous action. It is certainly desirable that a generator breaking down should be cut out of circuit with as little loss of time as possible, and with a reverse current of something less than the full load current. If, however, the circuit-breaker is fitted with a time-limit attachment there is a risk that its action may be so delayed as to allow the reverse-current to exceed the normal current by 200 or 300 per cent. before the fault is isolated from the 'bus-bars, and this may result in any synchronous apparatus connected at the time of dropping out of step.

Apparently the only argument in favour of a time-limit attachment is that a reverse-current device without time element is liable to be tripped by the rush of synchronising current from one generator to another when machines are being paralleled, and it is suggested that these momentary rushes are of such short duration that if the discriminating device is provided with some time element attachment the cut-out will not be tripped by these momentary currents. It must be remembered, however, that any time element attachment is liable to prevent the cut-out from operating, in the event of a serious breakdown occurring, until the pressure has dropped to such an extent that the device may have lost all sense of direction. The simplest way of dealing with this difficulty appears to be to either switch off the shunt-windings of the discriminating device at the moment of paralleling, thus rendering the device entirely inoperative, or to greatly reduce the pressure

across this winding when paralleling, thereby correspondingly increasing the reverse-current required to operate the device.

Excess Current Circuit-Breakers.—High-tension fuses for controlling circuits carrying large currents are now being almost exclusively replaced by excess-current circuit-breakers. The advantages of the latter are that, owing to the fact that the circuit can be broken under oil, there is much less danger of a destructive arc being started, and further, the current at which an excess-current circuit-breaker will operate can be determined with far greater accuracy than can the melting-current of a fuse. One advantage of a fuse is that it naturally possesses a certain time-element, whereas a circuit-breaker operates practically instantaneously, whatever the value of the excess current may be.

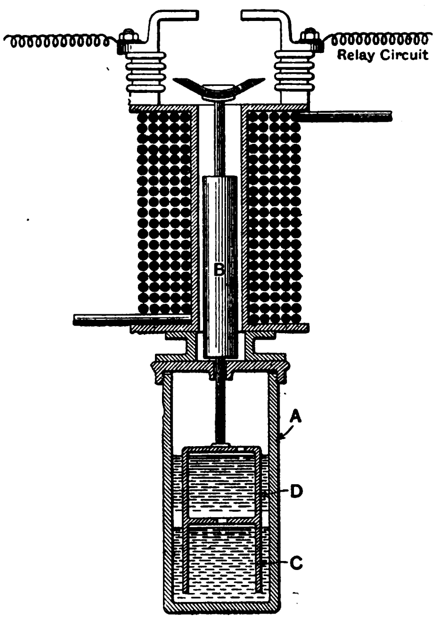


FIG. 9.

Various devices have been suggested for introducing a certain time-element into circuit-breakers. Messrs. Brown Boveri have used for this purpose a device that is constructed on the lines of an ordinary wattmeter. An excessive current in the winding of the wattmeter causes the armature to rotate and wind up a small weight. The rate at which this weight is lifted is approximately proportional to the current in the series winding; when the weight is wound up to its full extent it closes a local circuit, the current through which operates the main circuit-breaker.

A simple device, which appears to possess a time characteristic resembling that of a fuse, is shown in Fig. 9.

The speed at which the core is lifted within the solenoid is controlled by a diving bell immersed in mercury and oil. If the core is subjected to a small pull sufficient only to raise its weight together with the bell, it will rise very slowly, because the tendency for a vacuum to form within the bell will result in the mercury contained in its lower half being lifted. The weight thus added, which checks the upward movement, would, unless special provision were made, bring it to a standstill. The pressure of the atmosphere, however, relieves the vacuum by forcing the oil in the upper half of the bell through the small hole in the middle diaphragm, and the rate at which it does so determines the time taken by the core to travel from its zero position at which it closes

a local circuit and trips the breaker. It will be obvious that the time will approximately depend upon the strength of the pull on the one hand, and the dimensions of the oil inlet on the other. In the event of a dead short-circuit, the mercury contained in the bell is raised bodily, and the action of the instrument is instantaneous.

If a current of, say, 50 amperes is passed through one of these time-limit relays, and the hole in the top of the diving-bell is almost closed, the circuit-breaker is not released until this excess current has been maintained for about a minute. If the current is now increased to, say, 70 amperes the circuit-breaker is released in a much shorter time, and on further increasing the current to, say, 150 to 250 amperes the circuit-breaker is released instantaneously. The rate of operation can be accelerated considerably by adjusting the size of the hole in the top of the diving-bell to allow the oil to flow through it more freely.

DISCUSSION.

Mr. Woodbridge.

Mr. WOODBRIDGE: I have had a considerable amount of experience with reverse-current cut-outs in the States, but I doubt whether it is possible to obtain any device that will deal with all the conditions likely to arise in practice. For instance, a heavy short-circuit on a feeder may result in such violent surgings between all generators and synchronous apparatus connected in parallel at the time, that there would be a risk of some of the reverse-current devices being tripped by these currents. Again at the moment of synchronising, if the incoming generator is not properly in phase when the switch is closed heavy currents will flow from one generator to another in their attempts to pull into step. Finally, I doubt whether a reverse-current circuit-breaker will isolate a generator, should the resistance of its armature break down to earth, as under these conditions the 'bus-bar potential will fall to zero or thereabouts. The practice in some of the large stations in the States has been to connect solidly all generators to the 'bus-bars through hand-operated switches, using no protective devices in this position, though reverse-current devices are sometimes used to close a local circuit through an incandescent lamp, in the event of a generator failing, thus indicating which is the faulty machine.

Mr. Clothier.

Mr. H. W. CLOTHIER: I think the remote control switchgear at the Carville Station, Newcastle-on-Tyne, is a very perfect system. The phases are divided by concrete walls, and the several apparatus by gallery floors, but the capital cost per k.w. of such a system is undoubtedly very high. The initial cost of the mechanically operated remote control gear referred to in the paper will probably be very much less, but, in the absence of more perfect division, I should prefer to have a larger space between the phases. I think the connections of different phases, some of which appear to be exposed, are dangerously close to each other for a 6,000-volt central switchgear, and if the apparatus were made of double the size shown it would be considerably safer. With regard to reverse-current circuit-breakers, I agree with the previous speaker that it is better to rely upon hand-operated switches, using some form of current-direction indicator to guide the attendant

Mr. Clothier. in his operations. Many difficulties in the design are obvious; these must be overcome before the relays will operate with certainty when required. And even then we have evidence of conditions under which a relay will isolate sound parts of the system at the wrong time. I am of the opinion that station engineers will in time discard the use of automatic relays for switches, it being only a matter of time and experience under tests and working conditions. A reverse-current circuit-breaker would certainly not deal with a short-circuit across the 'bus-bars.

Mr. Orton. Mr. W. J. P. ORTON: In my opinion electrical control is not as complicated as it may at first sight appear. All that is necessary for closing a switch electrically is an additional solenoid. Although mechanical remote control is quite satisfactory for circuit-breakers controlling 1,000 k.w. or so, it is, I think, unworkable for the very large powers that sometimes have to be dealt with. Dealing with the separation of phases by brick walls or fireproof partitions, this is entirely a question of pressure. For very high pressure I consider it necessary to isolate the phases completely.

Dr. Garrard. Dr. C. C. GARRARD: I would ask the author if he could give a definition of what he means by an alternating reverse current, as there is really no such thing. I consider the principle upon which device G is constructed to be a wrong one. If no artificial phase displacement be introduced, the force on the moving part is proportional to that current component which, when multiplied by the voltage, gives the reverse power which is flowing. With a 90 deg. lag this current component would be equal to zero, and therefore there would be no force on the moving part; that is to say, the device would not operate on a wattless current. To get over this the author proposes to put induction in series with the shunt coils—an arrangement which is open to the very grave objection that if the device be then set to operate with a certain value of current lagging 90 degs., it would also operate with a larger current lagging less than 90 degs. behind the voltage. It therefore follows that the device suffers from one of the very defects quoted in the paper, namely: "(B) the device must not be released by a current supplied to a highly inductive load beyond the cut-out." Without the inductive resistance in series with the shunt, the force on the moving part is proportional to that current component which, multiplied by the voltage, gives the reverse power flowing. In a wattmeter device the actuating force is proportional to the product of this component and the voltage. A wattmeter, therefore, with a given expenditure of energy, and other things being equal, should have a greater actuating force than the one described in the paper, especially as a wattmeter is generally a more efficient motor than an electro-magnet. The great controlling force obtained in the author's device is simply a matter of the energy consumed therein, and I should like to know what was the watt consumption in the apparatus at full load.

Mr. Pearce. Mr. S. L. PEARCE: It appears to me that the design of discriminating cut-outs has not yet reached finality, and so long as there is any doubt about their behaviour it is better to trust solely to hand-operated switches. No automatic cut-outs of any description were used between

the generators and the 'bus-bars at the Shepherd's Bush Station of the Central London Railway, and this arrangement has, up to the present, proved satisfactory. Two breakdowns, as far as I know, came under my notice due to the failure of generator fields. On one occasion the switchboard attendant detected the fault and switched out the faulty machine; on the other occasion he was unable to do so, and a complete shut-down resulted. I would like to ask the author what would be the effect upon his cut-out device should the E.M.F. fall absolutely to zero, and also whether his rule as to the use of cut-outs applies to sub-station plant or only to generating plant. Mr. Pearce.

Mr. W. S. TOPLIS: I do not agree with the suggestion that the operating board should be placed outside the engine-room. It is very useful for the switchboard attendant to be able to see the running plant, as he is thus often able to detect a fault and operate a switch controlling the faulty section, without stopping to look at his instruments. Mr. Toplis.

Mr. A. E. McKENZIE: At one time we experienced at one of the Manchester generating stations considerable trouble with the Corliss valve gear on all the engines. This occasionally got out of order when the defective plant was running in parallel with other generators, and sometimes caused the faulty generator to take a momentary current, equivalent to 100 or 150 per cent. of its normal full-load current. The defect was only of short duration, and immediately it was removed the faulty generator took its share of the load. I believe that if this plant had been fitted with a reverse-current circuit-breaker it would have been cut out of action by its reverse current, and consequently we should have been obliged to re-parallel it. Mr. McKenzie.

Mr. EUSTACE THOMAS: There is undoubtedly a demand for a reverse-current circuit-breaker for alternating currents, and we ought not to have to rely upon the attendant. Breakdowns presumably do not often occur, and he may either take some time to locate the trouble, may get excited, locate it incorrectly, do the wrong thing, and cause further disturbance. This is very likely to happen, and people are more particular about avoiding temporary disturbances here probably than in any part of the world. Mr. Eustace Thomas.

A reliable attachment *can* be made, but it must *not* work instantly, and must *not* be set fine. This has not been properly understood, and engineers have not thought out the conditions, or they would see that quickness and fine adjustment are absolutely unnecessary, and in reality altogether inadvisable.

The device shown in Fig. G seems to do no more than that in Fig. B, and seems to have the disadvantage of being less simple, and less capable of adjustment for varying conditions. Both can be made to operate if the field should be broken.

Mr. E. W. COWAN: I think that the attitude taken up by Mr. Woodbridge in condemning reverse-current release gear because it would not deal with every condition which might arise in practice is a most unreasonable one. The contingency he cites is a remote one. He is probably quite right in expressing doubt as to the efficacy of any conceivable type of release gear under such conditions. The fact, however, that reverse-current release gear of the type described can Mr. Cowan.

Mr. Cowan. be relied upon to isolate a generator working in parallel with others if the prime mover pulled up or the field of the generator failed, even when accompanied by a considerable fall of pressure, is a justification for its use in the majority of instances. It seems to me impossible to dispute this view.

The criticism by Mr. Clothier of the danger of the exposed terminals at the back of the operating panel of the Poplar switchgear type is due to his misunderstanding the arrangement. The operating panel is absolutely safe to touch and work upon, both back and front, excepting when high-tension electrostatic voltmeters are fixed, and these are never used unless the customer specifies them. If architects would take into consideration the lay-out of the switchgear when designing their buildings, the problem of safe and fireproof switchgear would be more easily solved by the manufacturer. As matters are, we often have to be content with an arrangement which falls short of the ideal in respect of these much-to-be-desired elements.

Dr. Garrard condemns wholesale every type of release gear but his own wattmeter type, but I wonder whether Dr. Garrard is familiar with the difficulties of constructing a reliable wattmeter movement? I may remind him of a discussion which took place in London a few years ago upon a paper contributed by Mr. Mordey on "Capacity in Alternate Current Working." On that occasion Mr. Mordey described a wattmeter which he had devised for making his tests, and Dr. Sumpner pointed out in the discussion that he believed that if the wattmeter were used to test a cable with considerable capacity, the result would appear to indicate that the cable generated power instead of absorbing it.

Mr. Andrews. Mr. L. ANDREWS (*in reply*): I am not surprised to learn that Mr. Woodbridge's experience of reverse-current cut-outs have been of such a nature as to lead him to doubt their reliability. All the reverse-current devices I have seen in the United States are of a type which I should expect to give trouble. But I am convinced that perfectly reliable devices can be made to deal with all the troubles likely to arise with one exception, viz., that of the pressure falling absolutely to zero, but the only conditions under which such a fall of pressure is likely to occur in practice is that of a metal to metal short-circuit across the terminals or leads of a generator. Even under such drastic conditions the cut-out device would do no harm—it would merely remain inoperative. As to whether discriminating devices are likely to be tripped by a short-circuit on feeders causing heavy surging currents between generators, this depends to a great extent upon the sensitiveness of the cut-outs. If engineers insist upon cut-outs being adjusted to operate with a reverse current of, say, 5 per cent., there may be some risk of this occurring, but if discriminating devices are used, which will not operate with a reverse current of less than 25 per cent. of the full-load current, they need not fear trouble from this cause. I have used discriminating cut-outs for several years at Hastings, and during this time I have experienced many short-circuits on the feeders, etc., when generators were coupled in parallel, but I have never known a cut-out released under these conditions. Although the readings of ammeters may

Mr. Andrews.

often fluctuate between zero and their maximum reading, the design of generators must be very faulty if on a heavy overload they so far drop out of step as to take a reverse current of 25 per cent. of the normal full-load working current. If such generators are still used, one might hesitate to protect them by reverse-current devices, but it is certain that the majority of modern generators are not so defective, and discriminating circuit-breakers should certainly be used on these.

It is very interesting to note that Mr. Clothier's ideas have undergone such a complete change since he read his classical paper on switchgear before this section three years ago. The cubic capacity occupied by the gear Mr. Clothier now criticises as being too compact for 6,000 volts is about 180 cubic feet per 1,000 k.w., whereas when he wrote his own paper he apparently considered about 60 cubic feet per 1,000 k.w. ample space for a 10,000 volt three-phase scheme. The risk of shocks to attendants will in no way be minimised by allowing a greater space between phases. The only possible way of guarding against this danger *is* to construct the gear so that all live parts are thoroughly enclosed. This has been fully provided for in the arrangement shown in Fig. 1.

Mr. Orton has said that to close a switch electrically only entails the use of an additional solenoid, but it must be remembered that quite a small solenoid is all that is required for tripping a circuit-breaker, whereas a very large solenoid must be used to close a large circuit-breaker against powerful springs.

I may remind Dr. Garrard that in a paper I read in 1898 I drew attention to the fact that it is misleading to talk of an alternating reverse current, and he then suggested the word "discriminating" to designate circuit-breakers and other appliances used for discriminating between generating and motoring currents. According to Dr. Garrard's ideas of the theory of discriminating circuit-breakers, the device represented by Fig. G will not work. The device in question has, however, been submitted to many severe tests under practical working conditions, and has operated properly under them all. It is very evident from the remainder of Dr. Garrard's remarks that he has failed to understand the remarks on phase displacement in the paper. He states, for instance, that the introduction of inductance in the shunt circuit is liable to make the cut-out operate on a current lagging less than 90 deg. behind the voltage. It surely must be obvious that if the current in the potential winding is made to lag approximately 90 deg. behind the 'bus-bar E.M.F. by inserting a highly inductive resistance in series with this winding, and if the current in the series winding also lags nearly 90 deg. behind the E.M.F., the currents in the respective windings of the cut-out device must be approximately in phase with each other, and consequently the holding in pull on a highly inductive load will be much more pronounced than if no inductive resistance had been inserted in series with the shunt current. Replying to Dr. Garrard's inquiry as to the power consumption at full load in the device indicated by Fig. G, this may be made anything that is desired, but I generally prefer to use a device absorbing from 60 to 100 watts, as by that means a device is obtained that is very definite in its action.

Mr. Andrews.

Mr. Pearce cites two interesting cases that have come within his knowledge, in one of which the attendant had been able to deal with the fault without the aid of automatic devices, and in the other a complete shut-down resulted, entailing costly repairs. This is one of the strongest arguments in favour of the use of reverse-current devices, as it clearly shows that the practice of trusting to hand-operated switches cannot be relied upon. The fact that no troubles have arisen through using no automatic devices on the generators at the Shepherd's Bush station, is purely a negative argument, as it is quite recognised that generators as now made will often run for years without a serious breakdown. No one will deny, however, that breakdowns are always apt to occur at any moment, and the consequences of such breakdowns are usually so serious that every precaution should be taken. A case occurred in London a few weeks ago where two generators were coupled in parallel through fuses. The field failed of one of these generators, and the remaining generator took over the load and supplied current to the faulty generator. Both ammeters went hard over, but the carrying capacity of the fuses was so high that the remaining generator failed to blow them. The attendant, however, observed that both fuses were red-hot. One being considerably hotter than the other, he concluded that this was the faulty machine, and switched it out of parallel. Needless to say, this was the healthy machine, and the entire supply was interrupted.

I am unable to agree with Mr. Toplis that it is in all cases an advantage for the switchboard attendant to see the running plant. In some instances noises and other signs in the engine-room draw the attendant's attention to a fault before he might notice it on the instruments; on the other hand, these signs are always apt to be misleading.

Replying to Mr. McKenzie, I agree that a reverse-current circuit-breaker would have isolated the faulty plant when it took such a heavy current from the remaining generators, and I think that a generator that not only omitted to take its share of the load, but also robbed the remaining generators of current to this extent, ought to be cut out of circuit until the defect is remedied. If the remaining generators were able, even momentarily, to supply this heavy current without a serious drop of pressure, it is obvious that they would be better able to maintain the supply for a short time without the aid of this generator. Although the defect might only be of short duration, it might in that short time cause sufficient drop of pressure to make any synchronous motors connected at the time drop out of step.

I have to thank Mr. Thomas for his encouraging remarks, and I agree with his comment that device G is a modification of device B. One of the advantages of the latter arrangement is that the high-tension winding is not carried into the operating device, and I think it will be generally agreed that all the apparatus on the operating panels should be low tension. Although I have used device B for many years with complete success, I have found some advantages in using a simple differential solenoid as shown in Fig. G for the operating solenoid.

A vote of thanks to the author was unanimously accorded at the close of the proceedings.

GLASGOW LOCAL SECTION.

ARMATURE REACTION IN ALTERNATORS.

By JAMES B. HENDERSON, D.Sc., Member, and JOHN S. NICHOLSON, B.Sc.

(*Paper read at Meeting of Section, December* 13, 1904.)

The complete experimental investigation of the various elements in the armature reaction of any alternator is a very laborious problem, and is not one which can be tackled by the manufacturer for obvious commercial reasons. It has, however, great theoretical and practical importance, especially if for any machine considered the theoretical treatment is found to agree with the experimental results. Such an agreement would not only be interesting in itself, but would be a justification of any assumptions one had to make in order to be able to treat the complicated phenomena quantitatively.

In the present research the authors have endeavoured to make a comparison between the theoretical and experimental armature reaction of the 30-k.w. three-phase alternator in the James Watt Laboratories of the University of Glasgow. The alternator is one of the Electrical Company's standard type H. D. M., having rotating field poles and giving 50 $\sim$ per second at 750 revolutions per minute. The normal E.M.F. is 200 volts, and the current per phase winding is 87 amperes. Fig. 14 shows the details of the machine.

THEORETICAL.

When an alternator is running light, the E.M.F. in the armature is produced by that portion of the field flux which cuts the armature windings; hence, if we know the magnitude and distribution of the field flux and the distribution of the armature winding, it is a comparatively easy task to determine the E.M.F. in open circuit. When, however, the armature winding is supplying a current to a circuit of given power-factor, the E.M.F. at the alternator terminals is no longer produced by the same flux, but by a new flux, namely, that produced by the resultant of all the magnetomotive forces acting, and although we know these magnetomotive forces, our difficulty is to determine the resultant flux which they produce.

If for a moment we consider the fluxes from the point of view of the armature winding, the main field flux becomes a periodic function of

approximately sine form, the armature coils being generally distributed and the pole-pieces shaped so that the E.M.F. curve shall be approximately a sine curve. If we then consider the armature windings as a single coil, the main field corresponds to an alternating flux passing through the coil and waxing and waning according to a sine law. The armature current is also an alternating function whose form depends on the circuit supplied; but let us assume it is of sine form. The current is not in phase with the alternations of the field, its phase displacement relatively to the field depending on the power-factor of the circuit in which the current flows—the load and the armature windings. We know this power-factor, or can determine it. In addition to the main field flux, we have a local flux in the armature coils produced by the armature current. This we call the flux of self-induction or reactance flux of the armature. The armature current produces also a component magnetomotive force, which combines with the field magnetomotive force to increase or decrease the main field flux, and this component magnetomotive force varies with the power-factor of the load. Let us consider these various elements in detail.

The arrangement of the armature coils is shown in Fig. 1. A, B,

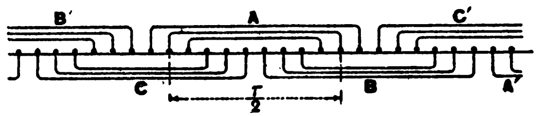


FIG. 1.—Arrangement of the Armature Windings.

and C are respectively elements in each of the three-phase windings, the armature surface being represented by a plane in the developed form. If we send a direct current through the phase winding A it produces a magnetomotive force normal to this plane whose distribution is represented by the right-angled curve in Fig. 2, the curve being

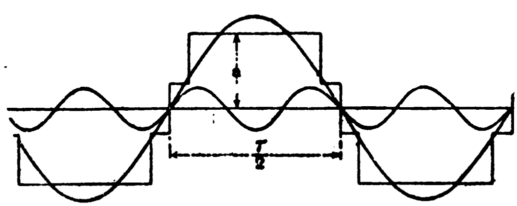


FIG. 2.—Distribution of Magnetomotive Force produced by a Direct Current in Phase Winding A of Fig. 1, showing also the Fundamental and the Third Harmonic of the Fourier Analysis of the M.M.F. Curve.

periodic—of period equal to double the pole-pitch. Let us denote this space period by τ while we reserve T for the period of the current. If an alternating current is sent through A, the magnetomotive force produced becomes periodic in time as well as place, but at every instant it is represented by a right-angled curve like Fig. 2, the horizontal dimensions of the curve remaining constant, but the vertical dimen-

sions fluctuating periodically with the current between certain definite equal positive and negative values. We shall follow Blondel's method of analysing such curves of magnetomotive force into Fourier series. The various terms in the Fourier series may be obtained mathematically or graphically, but in a case like this the former is much simpler.

Since the curve is symmetrically positive and negative alternately, it is evident that it can contain no even harmonics. Let us express this curve by a series of sines. Let a be the amplitude of the curve when the current is at its maximum.

$$a = \sqrt{2}\, n\, I_n/2,$$

where n is the number of turns in the element A and I_n the effective value of the current in each of these turns. I_n will, in many machines, as in ours, differ from the line current I, the relation between them depending on the grouping of the elements of the winding in series and parallel between the machine terminals.

The function which we have to express by a series of sines is—

$$\begin{aligned}
y &= a/3 && \text{from } x = 0 \text{ to } x = \tau/18,\\
y &= a && \quad ,, \quad x = \tau/18 \text{ to } x = 4\tau/9,\\
y &= a/3 && \quad ,, \quad x = 4\tau/9 \text{ to } x = \tau/2,\\
y &= -a/3 && \quad ,, \quad x = \tau/2 \text{ to } x = 5\tau/9,\\
y &= -a && \quad ,, \quad x = 5\tau/9 \text{ to } x = 17\tau/18,\\
y &= -a/3 && \quad ,, \quad x = 17\tau/18 \text{ to } x = \tau.
\end{aligned}$$

The wave-length is τ, and we must, accordingly, trace the variations throughout one wave-length. Any function $f(x)$ of wave-length λ if expressed in a series of sines, becomes

$$f(x) = \frac{2}{\lambda} \sum_{m=1}^{m=\infty} \sin \frac{m\pi x}{\lambda} \int_0^{\lambda} \sin \frac{m\pi v}{\lambda} f(v)\, dv.$$

Substituting for $f(x)$ the above constants, and for λ the wave-length τ, we get

$$y = \frac{2a}{3\pi} \sum_{m=1}^{m=\infty} \frac{1}{m} \sin \frac{m\pi x}{\tau} \left\{ 1 + 2\cos\frac{m\pi}{18} - 2\cos\frac{4}{9}m\pi - 2\cos\frac{m\pi}{2} \right.$$
$$\left. - 2\cos\frac{5}{9}m\pi + 2\cos\frac{17}{18}m\pi + \cos m\pi \right\},$$

which transforms to

$$y = \frac{8a}{3\pi} \sum_{m=1}^{m=\infty} \frac{1}{m} \cos\frac{m\pi}{2} \cdot \sin\frac{m\pi}{4} \left\{ \sin\frac{m\pi}{4} + 2\sin\frac{7}{36}m\pi \right\} \times \sin\frac{m\pi x}{\tau}.$$

When y is expressed in this factorial form we see at once that one factor becomes zero if m is odd, and another factor becomes zero if n is a multiple of 4. The only values of m we have to consider are, therefore, the odd multiples of 2, and these are the values which give us the odd harmonics.

Substituting the values 2, 6, 10, etc., for m we get

$$y = \frac{4a}{3\pi}\left\{2{\cdot}88 \sin \frac{2\pi x}{\tau} + \frac{2{\cdot}0}{3} \sin \frac{6\pi x}{\tau} + \frac{0{\cdot}653}{5} \sin \frac{10\pi x}{\tau} - \frac{0{\cdot}532}{7}\right.$$
$$\sin \frac{14\pi x}{\tau} - \frac{1{\cdot}0}{9} \sin \frac{18\pi x}{\tau} - \frac{0{\cdot}532}{11} \sin \frac{22\pi x}{\tau} + \frac{0{\cdot}653}{13} \sin \frac{26\pi x}{\tau}$$
$$\left. + \frac{2{\cdot}0}{15} \sin \frac{30\pi x}{\tau} + \frac{2{\cdot}88}{17} \sin \frac{34\pi x}{\tau} - \frac{2{\cdot}88}{19} \sin \frac{38\pi x}{19} -, \text{etc.}\right\},$$

each coefficient being repeated every ninth term, but with the sign changed. There are only five different coefficients, but four of them are repeated in every cycle of nine terms. We have now obtained the values of the fundamental sine wave of magnetomotive force and of all the harmonics by which the right-angled curve of Fig. 2 is to be replaced. The fundamental and third harmonic are drawn to scale in Fig. 2.

We have so far only considered one phase winding, but every one of the three windings has a similar magnetomotive force curve, and the resultant magnetomotive force will be got by adding all three together in their proper phase relations as regards time and space. Let us consider the three fundamentals for the windings, A, B, and C at any instant of time t. These are

$$y_{A1} = \frac{4a}{3\pi} \times 2{\cdot}88 \sin \frac{2\pi x}{\tau} \sin \frac{2\pi t}{T}$$

$$y_{B1} = \frac{4a}{3\pi} \times 2{\cdot}88 \sin \frac{2\pi\left(x - \frac{\tau}{3}\right)}{\tau} \sin \frac{2\pi\left(t - \frac{T}{3}\right)}{T},$$

$$y_{C1} = \frac{4a}{3\pi} \times 2{\cdot}88 \sin \frac{2\pi\left(x - \frac{2}{3}\tau\right)}{\tau} \sin \frac{2\pi\left(t - \frac{2}{3}T\right)}{T}.$$

Adding these together we get

$$y_{A1} + y_{B1} + y_{C1} = \frac{4a}{3\pi} \times 2{\cdot}88 \times 1{\cdot}5 \cos 2\pi\left(\frac{x}{t} - \frac{t}{T}\right).$$

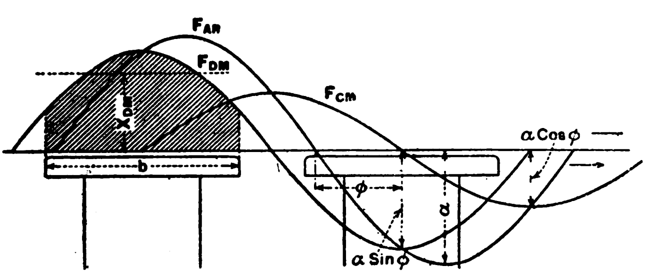
which represents a train of waves of wave-length τ, and amplitude half as great again as the amplitude of y_{A1}, moving forward with a velocity, τ/T—that is, with the velocity of the field poles. The three fundamentals combine, therefore, to give us a series of sine waves of magnetomotive force moving forward synchronously with the field poles, and therefore stationary over the poles. Similarly, we would find that the fifth harmonics combine to give us a sine magnetomotive force rotating at one-fifth of synchronous speed in the opposite direction to the rotation of the field poles. The seventh harmonic moves with one-seventh of synchronous speed, and in the same direction as the field poles, and similarly for the others. All the harmonics which are multiples of three disappear. Thus the fundamental, seventh, thirteenth, etc., harmonics revolve at their corresponding speeds in the same direction as the field poles, while the fifth, eleventh, etc., harmonics revolve at their corresponding speeds in the opposite direction to the field poles. Since all the harmonic magnetomotive forces move rela-

tively to the poles, they will induce currents in the amortisseurs, the field coils, and the pole faces, which will to a large extent annul any magnetising effect they might otherwise have. We will restrict our considerations for the present to the fundamental, which does not move relatively to the poles, and which produces, therefore, most of the armature reaction.

The position which this rotating magnetomotive force sine curve occupies relatively to the field poles depends on the power-factor of the circuit. If the current is in phase with the induced E.M.F., a little consideration of the components will show that the rotating sine curve must have its zero values over the centres of the poles, and in the general case, that the sine curves will lag or lead from this position by approximately the lag or lead of the current relatively to the induced E.M.F. The sine curve must always stand over any element of the winding at the instant that the current is a maximum in that winding, and the pole position at that instant is known relatively to the element of the winding, and, therefore, also relatively to the sine curve. We must now consider the general case of these waves lagging by an angle ϕ, relatively to the poles (Fig. 3). In order to separate the different magnetic effects produced by these magnetomotive force waves, we resolve the train of sine waves into two trains of sine waves, one of which has its maximum ordinates over the pole centres, and the other of which has its maximum ordinates midway between the pole centres. If a is the amplitude of the initial train of waves, the amplitude of the former component train will be $a \sin \phi$ and the amplitude of the latter component train $a \cos \phi$, and

$$a = \frac{4a}{3\pi} \times 2{\cdot}88 \times 1{\cdot}5 = \frac{2\sqrt{2}n\mathrm{I}_n}{3\pi} \times 2{\cdot}88 \times 1{\cdot}5.$$

We shall now consider these two sets of waves separately, and the remaining part of the theory will be divided into two parts, the first dealing with the set of waves having their maximum ordinates over the pole centres, and the second part dealing with the other set having their maximum ordinates midway between the pole centres. The former set we call the demagnetising waves $F_{D.M.}$, and the latter the cross-magnetising waves $F_{C.M.}$; both are shown in Fig. 3.



FAR = Fundamental M.M.F. Waves of Armature Reaction.
FDM = Demagnetising Component of FAR.
FCM = Cross-magnetising Component of FAR.

FIG. 3.

Demagnetising Component of the Armature Reaction.

The set of magnetomotive force waves which has the maximum ordinates over the pole centres will produce a direct magnetising or demagnetising action on the poles, depending on whether the current in the generator is leading or lagging. If the machine is running as a motor, the action would, of course, be the opposite of what it is in a generator.

Since the pole surface is not continuous, only that portion of a wave which stands over the pole will have any great effect on the flux in the gap. We must, therefore, find the average value of this portion of the wave which is shaded in Fig. 3. A simple integration gives us the area of the shaded portion, and, dividing by the length b, we get the average ordinate, the value being

$$\frac{\sin b\pi/\tau}{b\pi/\tau} \times \text{maximum ordinate.}$$

Let us call this the *demagnetising* magnetomotive force of armature reaction, ϕ being reckoned positive for lagging current, so as to give the demagnetising effect; a leading current will make ϕ negative and give negative demagnetisation or magnetisation. Let us denote the magnetomotive force by $X_{D.M.}$ We have then

$$X_{D.M.} = \frac{4a}{3\pi} \times 2{\cdot}88 \times 1{\cdot}5 \sin\phi \frac{\sin b\pi/\tau}{b\pi/\tau}$$
$$= \frac{4\sqrt{2}\, n\, I_n}{6\pi} \times 2{\cdot}88 \times 1{\cdot}5 \sin\phi \frac{\sin b\pi/\tau}{b\pi/\tau}.$$

In our generator $b/\tau = 0{\cdot}375$, $n = 27$, and since in this armature there are two paths for the current in each phase winding, I_n is one-half of I, the line current. Substituting these values we get $X_{D.M.} = 13{\cdot}7\ I \sin\phi$ ampere turns.

If now a current is taken from the armature, lagging or leading by 90 deg., the total armature reaction consists of the one set of waves $F_{D.M.}$, since the other set contains $\cos\phi$, and vanishes when $\phi = 90$ deg. In this case $X_{D.M.} = 13{\cdot}7$ I.

If, in addition to $X_{D.M.}$ we know the reactance of the armature, then from the no-load or magnetic characteristic we can construct the load characteristic for any value of the current. We might have obtained the armature reactance by calculation from the dimensions of the slots, but it was easier to determine it experimentally. Having removed the poles from the machine, an alternating E.M.F. of sine form, as tested by the oscillograph, was applied to the terminals of one-phase winding, and readings of the current and terminal voltage were simultaneously taken. From a number of experiments a mean reactance was obtained, $x = 0{\cdot}214$ ohm, and from the wattmeter readings in the same experiments the equivalent resistance was deduced: $r = 0{\cdot}030$ ohms.

Cross-magnetising Component of the Armature Reaction.

While the demagnetising component of the armature M.M.F. decreases the main field flux as a whole (or increases it when $X_{D.M.}$ is negative) and may be reckoned as so many ampere turns subtracted

from (or added to) the excitation of the field magnets, the cross-magnetising component shown by the curve $F_{C.M.}$ in Figs. 3 and 4 produces neither increase nor decrease of the field excitation as a whole, but increases the M.M.F. acting over one-half of the pole face, and diminishes the M.M.F. acting over the other half of the pole face, the increase and decrease being equal in magnitude. $F_{C.M.}$ thus tends to produce a local flux passing through one-half of the air-gap, then along the pole shoe, back through the other half of the gap, and completing its magnetic circuit through the iron of the armature. This local flux will be constant in magnitude, and will move along with the poles. It will therefore cut the armature windings, and produce an E.M.F. in them which we must investigate.

The shaded portion of Fig. 4 represents the distribution of $F_{C.M.}$ across the pole face, and since the gap is of uniform width, the flux distribution will be represented by a curve similar to $F_{C.M.}$; in fact, the shaded portion of the curve might represent on a certain scale the flux produced in the gap by $F_{C.M.}$ itself. The flux will not decrease at the edges of the gap so suddenly as is shown, but we can allow for that

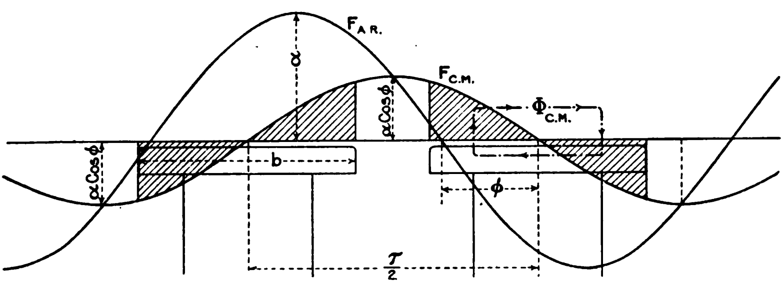


FIG. 4.—Diagram showing the Cross-magnetising Component of the M.M.F. of Armature Reaction. The shaded portions are effective in producing magnetic flux in the Air-gap.

later. The E.M.F's which this flux induces in the coils of one phase winding of the armature are represented by the curves shown in Fig. 5, the three sine curves, or portions of sine curves, being placed in their proper phase relationship, which is fixed by the position of the coils in the armature winding. The resultant E.M.F. is the sum of the three components in Fig. 5, and is drawn in the figure. To allow for the gradual change in the magnitude of the flux, which takes place at the edges of the gap, the resultant curve of E.M.F. has been rounded off at all the sharp corners, thus producing the E.M.F. curve shown shaded in Fig. 5.

The value of the effective E.M.F. has been determined by the polar curve in Fig. 5, and the equivalent sine wave is also drawn in the figure. It is evident from the shape of the E.M.F. curve that the third harmonic E.M.F. is a relatively strong one, and its effect is, of course, included in the equivalent sine wave; and, since the third harmonics are eliminated from the terminal E.M.F. when the windings are star-connected, we would expect the star voltage of the machine to be

greater than $\sqrt{3}$ times the line volts when $F_{C.M.}$ is great, that is, when the current is nearly in phase with the E.M.F.

Having obtained the equivalent sine wave, we can immediately calculate approximately the E.M.F. produced in the armature winding by the flux $\Phi_{C.M.}$ if we know the value of the maximum ordinate of one of the original sine waves.

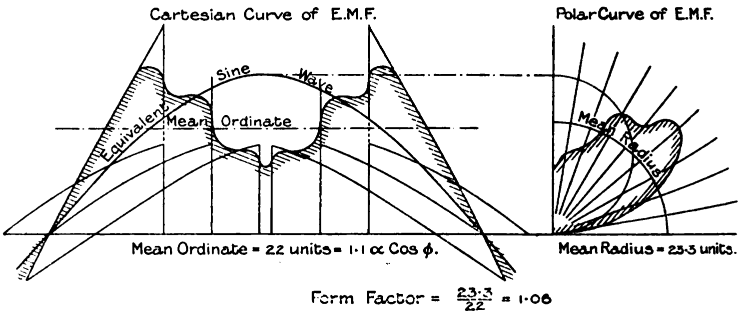


FIG. 5.—Curves of the E.M.F. induced in the Armature Windings by the Flux of Cross-magnetisation.

Let $X_{C.M.}$ = The average value of the M.M.F., $F_{C.M.}$ acting through the two half gaps.

$\Phi_{C.M.}$ = Flux produced by $F_{C.M.}$ through both halves of the gap.

$$\text{then } X_{C.M.} = 4\,a\cos\phi\int_0^{b/2}\sin\frac{2\pi x}{\tau}\,dx =$$

$$2\,a\cos\phi\times\frac{1-\cos\frac{b}{\tau}\pi}{\frac{b}{\tau}\pi} = 18{\cdot}3\ \mathrm{I}\cos\phi,$$

and $\Phi_{C.M.} = 0{\cdot}4\,\pi X_{C.M.} \div$ the reluctance of the two half-gaps.

$$= \frac{0{\cdot}4\,\pi\times18{\cdot}3\ \mathrm{I}\cos\phi}{2\times0{\cdot}5/8{\cdot}4\times12} \qquad \text{Breadth of pole-face } b = 16{\cdot}8 \text{ cm.}$$

$$\qquad\qquad\qquad\qquad \text{Length of pole-face } l = 12 \text{ cm.}$$

$$= 2320\,\mathrm{I}\cos\phi \qquad \text{Width of air gap } \delta = 0{\cdot}5 \text{ cm.}$$

Let $E_{C.M.}$ be the effective E.M.F. per phase winding induced by the fluxes $\Phi_{C.M.}$.

$$E_{C.M.} = [4\,n\,N\,\Phi\times\text{form factor}\times\text{breadth factor}\times10^{-8}]$$

The form factor and breadth factor are obtained graphically in Fig. 5.

$$\text{Form factor} = \frac{\text{Mean radius of the polar curve}}{\text{Mean ordinate of the cartesian curve}} = \frac{23{\cdot}3}{22} = 1{\cdot}06$$

The breadth factor =

$$\frac{\text{Area of the resultant E.M.F. curve}}{3\text{ times the area of the single E.M.F. curve}} = 0{\cdot}89$$

$$\therefore E_{C.M.} = 4\times54\times50\times2\,\Phi_{C.M.}\times1{\cdot}06\times0{\cdot}89\times10^{-8} = 20400\,\Phi_{C.M.}$$
$$\times10^{-8} = 20400\times23200\,\mathrm{I}\cos\phi\times10^{-8} = 0{\cdot}473\,\mathrm{I}\cos\phi\text{ volts.}$$

We have in $E_{C.M.}$ considered the equivalent sine wave, and in considering the effects it will produce on the line volts we must remember that it includes a relatively strong third harmonic.

In our generator we conceive our induced E.M.F. to be the resultant of two separate E.M.F.'s, each produced by its respective flux.

Let E_0 = Nominal induced E.M.F. or the E.M.F. which would be produced on open circuit by an excitation in the field magnets equal to the resultant excitation on load. Thus if X is the number of ampere turns in the field magnet coils, the resultant excitation $X_R = X - X_{D.M.}$ E_0 is in phase with the field poles, that is, it attains its maximum value at the instant when the pole centres are immediately under the centre slot of each group of slots containing that winding.

$E_{C.M.}$ the E.M.F. induced by the flux of cross magnetisation $\Phi_{C.M.}$ will be in quadrature with E_0, since $\Phi_{C.M.}$ is in quadrature with the main field flux. The E.M.F. induced in the phase windings is the resultant of E_0 and $E_{C.M.}$ Let us denote it by E_i. Since E_0 and $E_{C.M.}$ are always at right angles and E_i is their resultant, the diagrammatic representation of these three must always make up a right-angled triangle.

Let E = terminal E.M.F.
r = armature resistance.
x = armature reactance.
z = armature impedance.

E_i must also be the resultant of a combination of the terminal E.M.F. with the E.M.F. consumed by the armature impedance, that is, the resultant of a combination of E with $I z$.

Let ω = phase difference between the current I and the terminal E.M.F.

It must be remembered that the angle ϕ is the angle of phase displacement of the current relatively to the poles, and since E_0 is in phase with the poles, ϕ is the angle between I and E_0.

The vector diagram showing the relationship between the E.M.F.'s is shown in Fig. 6. If I, E, ω and z are known, as may be the case, then E_i is at once determined. We can then say that the point P (Fig. 6) must lie somewhere on the semicircle having E_i as diameter. If $E_{C.M.}$ is then known, E_0 is determined.

Theoretical Determination of Load Curves.

We are now in a position to determine any load curve from the constants of the machine and the magnetic characteristic, which, like the other constants, can be easily calculated from the dimensions of the machine.

A load curve gives us the terminal E.M.F. expressed as a function of the excitation for a constant value of the armature current. Let us take any value of X, the excitation, and try to determine the corresponding value of E when I and ω are given. Since the angle ϕ is unknown, $X_{D.M.}$ and $E_{C.M.}$ are both unknown, and our problem looks at first sight impossible. But let us think of the reverse problem for a moment, and instead of calculating for the load curve, the E.M.F.

corresponding to a given excitation, let us try to calculate the excitation which will give a certain terminal E.M.F. The fixed items are then E, I, ω, r and x. In Fig. 7, O A is drawn to represent E, making an angle ω with I. A B = I r is parallel to I, and B C = I x is perpendicular to I. O C is then the induced E.M.F. or E_i. This must be the resultant of the two components, E_o and $E_{C.M.}$, which are mutually perpendicular the one to the other, therefore the point P lies on the

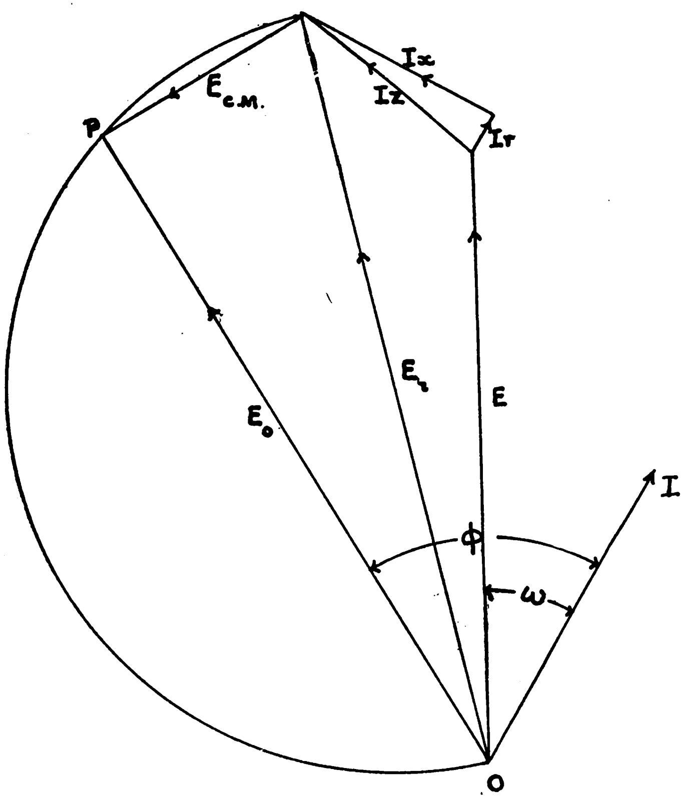


FIG. 6.

semicircle O P C. Suppose for a moment P is determined in position, produce O P to meet B C produced in the point D, then it is easy to see that the angle P C D = ϕ, and C D = $\frac{E_{C.M.}}{\cos\phi}$. But we found $E_{C.M.}$ = 0·473 I cos ϕ, therefore C D = 0·473 I, which is a constant for any load curve. In our diagram, therefore, A B, B C, and C D are constants, and if we mark along O A from A, a scale of terminal E.M.F. per phase winding, and join points on this scale to C and D, we can find for each point the corresponding value of E_o and of ϕ. Knowing ϕ, we can

calculate $X_{D.M.} = 13{\cdot}7\ I \sin \phi$, and knowing E_o, we get the corresponding value of X_R from the magnetisation curve, hence we find the values of $X = X_R + X_{D.M.}$ corresponding to the different values of E, and with these values we can plot the load curve. All the load curves in this paper have been calculated in this manner, and are represented by full lines in Figs. 9, 10, 11, and 12, and the experimentally determined values are represented by the experimental points alone. It is possible to assume values for E_o and obtain graphically the corresponding value of E, but the method we have described above is the one we have found most convenient after trying several others.

For the case when $\cos \omega = 0$, *i.e.*, the current lagging or leading relatively to the terminal E.M.F. by 90°, the above construction is

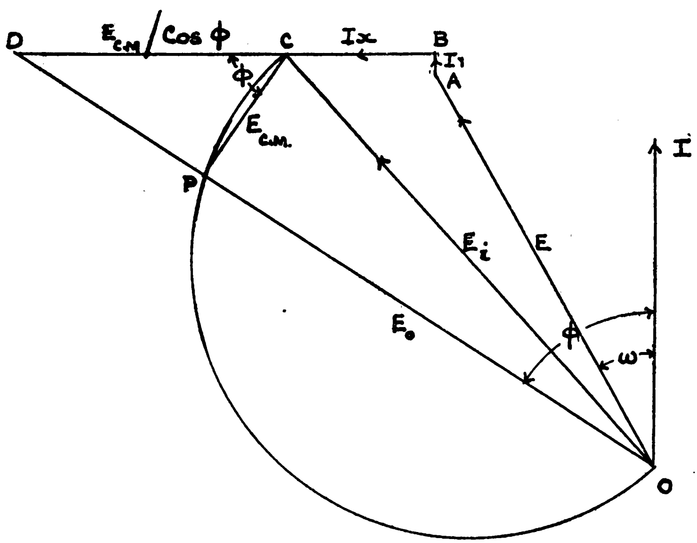


FIG. 7.

greatly simplified, since $E_{C.M.} = 0$. The load curves are then obtained by shifting the No-load curve along O X by the distance $X_{D.M.}$, and along O Y by the distance $I\,x$.

Those who are familiar with the Blondel-Arnold method of calculating the armature reaction will notice considerable differences in the above treatment. Blondel, instead of considering the E.M.F. produced by the resultant M.M.F. in the main field circuit, and combining it with $E_{C.M.}$, as we have done, considers that there are three E.M.F.'s acting. These are (1) the E.M.F. which would be induced on no load by the given excitation in the field coils; (2) the E.M.F. $E_{D.M.}$ which would be induced in the armature windings by the excitation $X_{D.M.}$ in the field magnets, which is in opposite phase to No. 1; and (3) the E.M.F. of cross magnetisation $E_{C.M.}$ This method has the great disadvantage that $E_{D.M.}$ cannot be calculated until you know the reluctance of the

field magnet circuit for the particular value of the flux under consideration. To overcome this difficulty, Blondel and Arnold make certain assumptions regarding $E_{D.M.}$, or its relationship to $E_{C.M.}$, relationships which may be justified by actual trial, but which have no direct physical meaning.* The method described in this paper, which has been developed by one of us, appears to us to be simpler and to contain fewer assumptions, and the physical meaning of each step is easily followed.†

Experimental Research.

In order adequately to test the theory we determined to obtain load curves of our three-phase generator at power-factors zero, 0·8, and unity for values of the current near half-load and near full-load, both when the current was lagging and when leading, and to carry these curves through as great a range of excitation as possible. The experimental difficulties were very great, but one by one they were overcome, and as the arrangements developed a higher order of accuracy was continuously aimed at.

A load curve is really a magnetic characteristic of the magnetic circuit of the alternator, in which the magnetisation is not produced by the excitation of the field magnets alone, but by the joint action of the field and armature currents; and if we wish to get good load curves we must employ all the precautions necessary for getting good magnetisation curves. We must carefully demagnetise the circuit to start with, and we must steadily increase the excitation and take corresponding values of the excitation and E.M.F. as the two rise together. This is comparatively easy to do on no-load or with a water resistance load for unity power-factor, but very great care is required to carry it out when the excitation is the resultant of the combination of three independent variables—the field current, the energy current in the armature, and the wattless current in the armature, each of which affects the E.M.F.

The inductive portion of the load was supplied in our case by two 9 k.w. rotary convertors coupled together. They are designed for 250 volts at the commutator brushes, and, as this E.M.F. had to be increased to over 400 volts at the highest portions of our curves, special arrangements had to be made for exciting the field magnets. These were divided into two groups on each machine, and the four groups were excited in parallel from a 250-volt battery. The convertors were only used to supply the inductive portion of the load, and therefore took no power from the alternator. The power to drive the convertors was supplied to the commutators of the convertors from the Corporation 500-volt supply mains through a regulating resistance. This resistance

* *Sammlung Elektrotechnischer Vorträge*, Vol. III., parts 1–3; "Beitrag zur Vorausberechnung von Ein—und Mehrphasenstromgeneratoren" von E. Arnold and J. L. la Cour; also "Die Wechselstromtechnik," Vol. IV., by E. Arnold.

† This method may not be new, but the authors have no means of making a search through the literature of the subject, as the back numbers of the Continental electrotechnical journals cannot be consulted in any library in Glasgow.

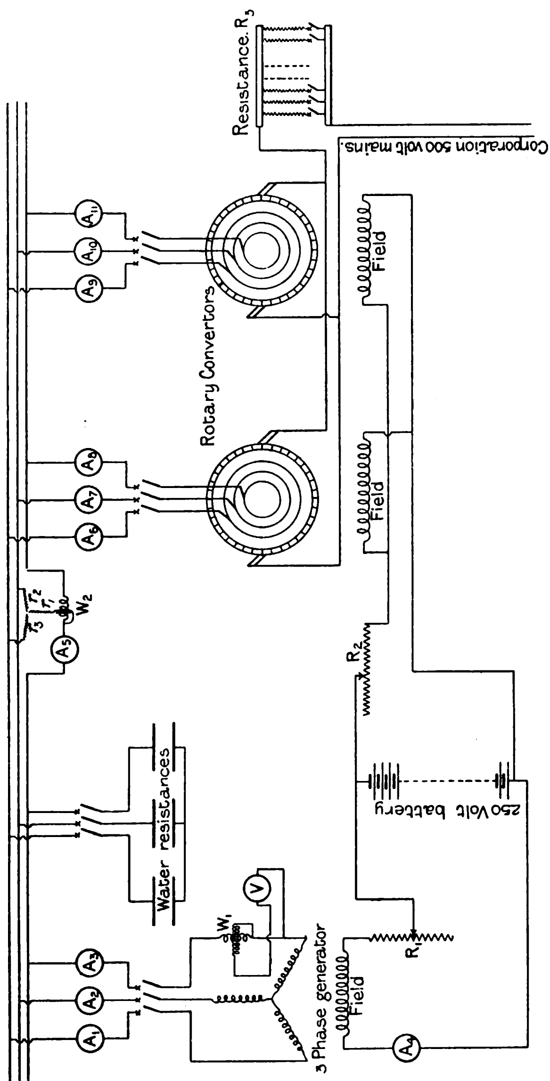


FIG. 8.—Diagram of the Apparatus and Connections used in the Experiments.

required both fine adjustment and large range of adjustment. The current and power passing between the alternator and the convertors were measured by an ammeter and wattmeter respectively, the reading of the latter instrument being kept at zero by adjusting the continuous current supplied to the commutators.

The output of the alternator was measured by another wattmeter inserted in one of the line wires, and an ammeter in each line enabled the current in all three phases to be adjusted to equality, and to be kept constant. A diagrammatic representation of the experimental arrangements is shown in Fig. 8. The generator is coupled by a flexible coupling to a 46-H.P. direct-current motor, which is driven from the Corporation 500-volt mains.

Method of Experimenting.

After starting the motor which drives the alternator, the magnetic circuit of the alternator was carefully demagnetised by reversals with ever-decreasing excitation. The generator was then excited so as to give 20 or 30 volts. The rotary convertors were then run up to synchronism with a very weak field excitation, their fields having previously been roughly demagnetised by reversals. To drive them at synchronous speed with such a weak field took a relatively large armature current—about 25 amperes for each machine. At synchronism they were switched into parallel with the alternator, and very often several trials had to be made before we succeeded in getting the machines to run together in parallel at such low excitation. The wattmeter reading was then brought to zero by adjusting the direct current supplied to the commutators, and throughout the experiments that followed a close watch had to be kept on this wattmeter reading when making any other adjustment, in order to prevent the machines getting out of step at the low E.M.F's.

In order to fix our ideas, suppose we consider the curve for $\cos \omega = 0{\cdot}8$ and 80 amperes, the current lagging behind the E.M.F. After getting the machines running together in parallel great care had to be taken in switching on the water resistance, or in altering the excitation as mentioned above, and if the machines did fall out of step the fields were demagnetised again and a fresh start made. By increasing the generator excitation and diminishing the convertors' excitation step by step (the reverse for leading currents), the wattless current was gradually increased, and, by moving the plates of the water resistance, the adjustment of the energy-current was kept approximately in step with the adjustment of the wattless portion until the final condition was reached, namely, 48 amperes wattless current and 80 amperes total current. When the desired condition had been reached, the E.M.F. and the field current were read off, a close watch being kept on the speed while the readings were taken. In the theoretical load curve for $\cos \omega = 0{\cdot}8$ leading, there is a vertical portion at low E.M.F's, which means instability when running in parallel at these E.M.F's. There is, therefore, no use trying to get points on this curve on the vertical portion. We found this experimentally before the theoretical curve had been calculated.

A Kelvin multicellular voltmeter was used for the measurement of the E.M.F.'s in conjunction with a small transformer of variable transformation ratio suitable for covering the range, and the ratio was altered so as to keep the indication of the voltmeter in the most sensitive portion of the scale. The speed was indicated by a tachometer, and was kept constant by adjusting the motor field current.

After getting one point on the load curves the generator excitation was increased by a definite amount, thereby increasing the armature current, which was then brought gradually back to its former value by increasing the excitation of the convertors and adjusting the water

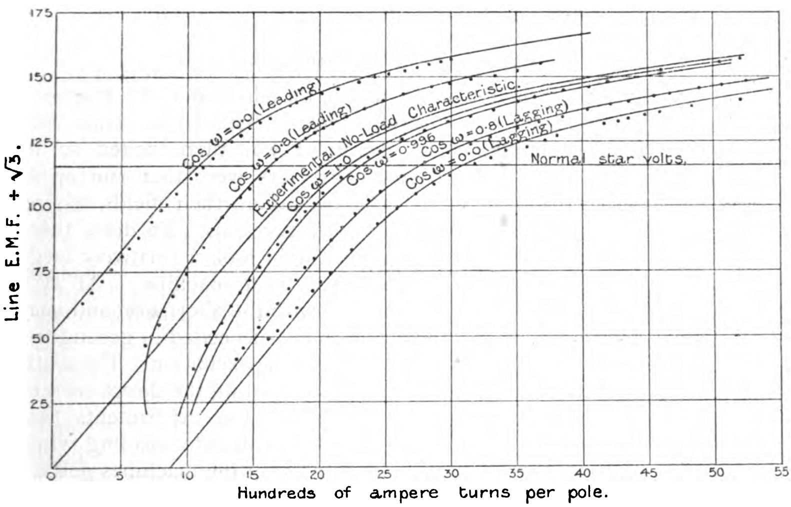


FIG. 9.—The full lines are the Theoretical Load Curves of the Three-phase Generator for 50 amperes, and external power-factors are indicated on the curves. The points show the corresponding experimental results, the line E.M.F. being measured.

resistance. Great care had to be taken not to overstep any adjustment of the excitation.

EXPERIMENTAL RESULTS.

The results of the experiments have been plotted in Figs. 9, 10, 11, and 12 side by side with the theoretically deduced curves. The experimental points, it will be seen, lie closely on smooth curves, but, in order not to confuse the diagram, these have not been drawn. The full lines are theoretical curves, and the points mark the experimental results. The deviations of the points from smooth curves are very small, and form a sufficient guarantee of the accuracy of the experimenting. The deviations of the experimental points from the theoretical curves are of a higher order of magnitude at some parts of the curves, so we cannot say that the agreement between theory and experiment is within the limits of experimental error. Let us examine the probable causes of these deviations.

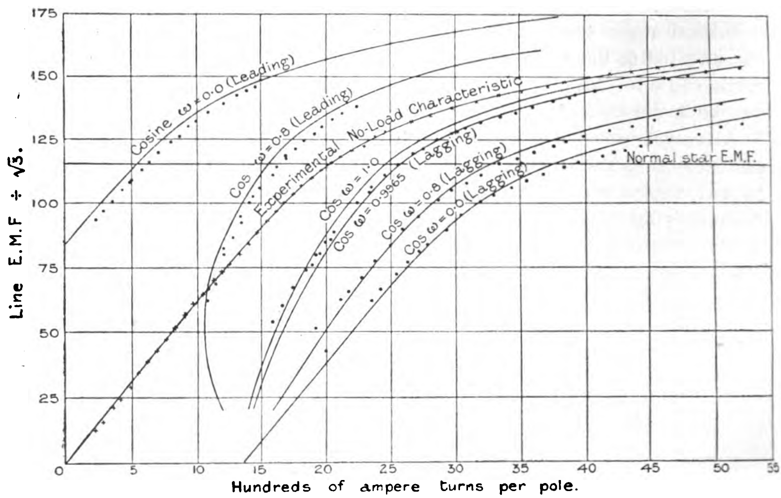


FIG. 10.—The full lines are the Theoretical Load Curves for the Three-phase Generator for 80 amperes and external power-factors, as indicated on the curves. The points show the corresponding experimental results, the line E.M.F. being measured.

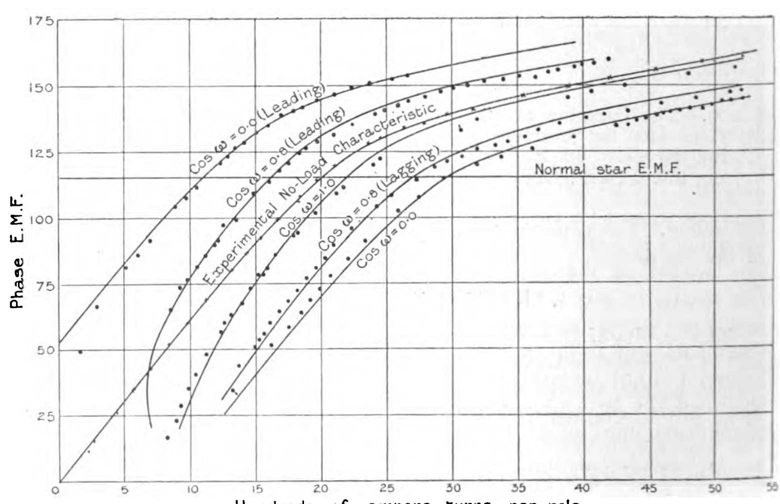


FIG. 11.—The full lines are the Theoretical Load Curves of the Three-phase Generator for 50 amperes and external power-factors as indicated on the curves. The points show the corresponding experimental results, the phase E.M.F. being measured.

The deviations are greatest in the curves for unity power-factor, and although the load for those curves consisted only of water resistances, still there was a certain reactance in the leads, in the ammeters, and in the iron plates used in the resistances, all of which would tend to give a power-factor less than unity, but less by an amount which could not be measured by a wattmeter method. The theoretical curve at power-factors near unity is very sensitive to small changes in the power-factor, and in Figs. 9 and 10 we have drawn theoretical load curves for power-factor 0·9965, which represents a lag of about 5 degs. Few of the experimental points fall below these latter curves, so it is possible to account for a large part of the deviations at unity power-factor by the small reactance of the circuit. The influence of the power-factor is not nearly so great at power-factors zero and 0·8.

Influence of the Third Harmonic in $E_{C.M.}$

In order to determine the influence of the third harmonic and its multiples, which are relatively strong in $E_{C.M.}$, we took two sets of experiments. In the first we read the E.M.F. at the terminals of one phase winding throughout all the experiments, including the no-load curve; in the second set we read the E.M.F. at the terminals of the machine when the windings were connected in star. One set of experiments might have sufficed if we had read the two E.M.F.'s simultaneously, but the second set was an after-thought to remove objections in our own minds.

In the calculation of $E_{C.M.}$ the equivalent sine wave was taken, and it therefore includes the harmonics. Taking the E.M.F. at the terminals of one phase winding therefore, we should get a real comparison between our theory and experiment. If we take the line E.M.F. we eliminate the effects of the harmonics which are multiples of three from the experimental points alone, while the effects still remain in the calculated value of $E_{C.M.}$ In the theoretical curves in Figs. 9 and 10 therefore, the values of $E_{C.M.}$ are too large. An increase of $E_{C.M.}$ at unity power-factor moves the theoretical curve to the right, so that the deviations in Figs. 9 and 10 are really too small; but it is evident from the two sets of experiments that the third harmonics do not influence the results to any very considerable extent. The E.M.F. curve of the generator is a good smooth sine curve on no-load when taken from the terminals, but if taken from the terminals of one phase winding it shows a slight distortion due to the third harmonic. The difference between the two no-load curves in Figs. 9 and 11 gives a measure of these harmonics on no-load.

Effect of Saturation of the Iron.

The theoretical curves for 50 amperes deviate very slightly from the experimental ones, and the agreement is closer than is called for in practical designing. The agreement is not quite so good in the curves for 80 amperes, and this fact forces us to consider what effects the armature current strength will introduce that have not already been

considered. The chief of these is the effect of the saturation of the iron in the armature teeth. Let us study first the effect produced by the cross-magnetisation, which will cause the iron in one half of the gap to approach saturation before that in the other half. This inequality in the two halves of the gap will be electrically equivalent to a motion of the centre lines of the poles towards the unsaturated side, that is, it corresponds to an increase of the angle ϕ when the current is lagging, and to a decrease of ϕ when the current is leading. Such an alteration in the angle ϕ will in every case move the theoretical load curve to the right, or nearer the experimental curve. This will apply to the curves for power-factor's unity, and 0·8, in all of which $E_{C.M.}$ appears. If we increase the saturation far enough we ought to get over this want of symmetry, and, although some of the curves seem to agree better at the highest values than they do nearer the bend of the curve, it is very doubtful if the cause can be thus explained.

In calculating $E_{C.M.}$ we have considered only the reluctance of the gap, and have neglected the reluctance of the iron part of the circuit. Let us examine in how far we are justified in doing so in our particular case, especially at high values of the E.M.F. We found $\Phi_{C.M.} = 2{,}320 I \cos \phi$ and to take an extreme case, let $\phi = 0$, which is possible if the current is a leading one. Taking the current as 80 amperes, we get $\Phi_{C.M.} = 18{,}560$. This flux has to be subtracted from the flux in one half-gap and added to that in the other. The uniform flux in the gap necessary to give the normal terminal E.M.F. of 200 volts is 0·96 megalines. The average variation of the flux over one half-gap due to $\phi_{C.M.}$ is therefore about 4·5 per cent. at normal E.M.F., but the variation near the pole-tips is about double the average—let us say 9 per cent. The pole shoe covers seven teeth, each having a section of 69 square cms. The induction in the teeth at normal E.M.F. is about 14,000, the length of a tooth is about 3 cms., the permeability at this induction is about 1,600, therefore the average reluctance of the teeth is equivalent to an increase of the gap by $\frac{3}{1600}$ cms., or 0·019 mm., or 0·4 per cent. of the gap length. A 9 per cent. increase of the induction at 14,000, however, carries us over the bend of the B-H curve, and gives us 2·5 times the former increase of the gap, or 1 per cent. If we take the value of the induction 16,000, which would correspond to a star E.M.F. of about 130 volts, we find that the reluctance of the teeth is equivalent to 0·095 mm., or 2 per cent. of the gap length. A 9 per cent. increase at 16,000 would correspond to an increase of the gap by 0·285 mm., or 5·7 per cent. These increases in the equivalent length of the gap may not be great in themselves, but they produce a much more potent influence in the increase of the angle ϕ. The increase of the equivalent gap decreases the value of $E_{C.M.}$, but the load curve is not very sensitive at high values to small variations in $E_{C.M.}$ A decrease in $E_{C.M.}$ would displace the theoretical curve to the left.

In the curves for zero power-factor the cross-magnetisation plays no part. The deviations in these curves may be due to the saturation effects, but there are other effects present, all or any of which may play a part. The field flux for these curves is excited partly by the armature-

current, and we have, therefore, a different distribution of the exciting windings on the magnetic circuit to that present when the no-load curves were obtained. This will cause different leakage and slightly different distribution of the flux, and according as the armature current lags or leads the leakage flux will be increased or diminished.

Influence of the Form of the Current Waves.

Throughout the theoretical portion we have assumed that the current wave is of sine form, but distortion may take place through a distortion of the E.M.F. curve due to $X_{C.M.}$ or it may take place due to the external load. In such a case we have to analyse our current wave into its harmonics and deal with each harmonic separately. Each of

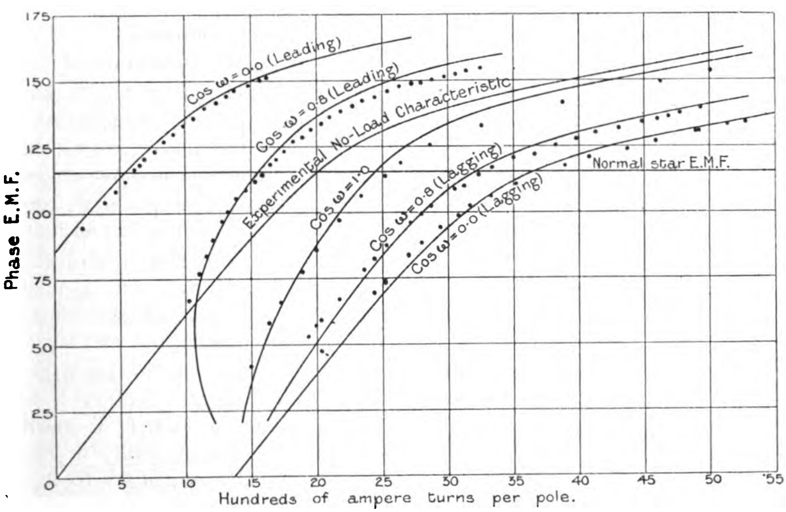


FIG. 12.—The full lines are the Theoretical Load Curves of the Three-phase Generator for 80 amperes and external power-factors as indicated on the curves. The points show the corresponding experimental results, the phase E.M.F. being measured.

these harmonics produces a M.M.F. in the armature windings, the shape of the M.M.F. curve produced by every harmonic of the current being the same rectangular form shown in Fig. 2, the time period of the M.M.F. varying, however, with the particular harmonic of the current which we consider. Each of these rectangular curves can be analysed into its harmonics like those we have considered in the paper. We have seen that the seventh harmonic M.M.F. waves produced by the fundamentals of the currents in the three windings revolve at one seventh of synchronous speed in the same direction as the poles. If, then, we increase the frequency of the current to seven times the fundamental frequency the seventh harmonic M.M.F. waves would then revolve synchronously with the poles. The seventh harmonic in the current wave has seven times the frequency of the fundamental, there-

fore the seventh harmonic M.M.F. waves which this seventh harmonic of the current produces will revolve synchronously with the poles. Similarly the thirteenth, nineteenth, etc., harmonic M.M.F. waves produced by the respective thirteenth, nineteenth, etc., harmonics in the current wave, will all revolve synchronously with the poles. The fifth, eleventh, seventeenth, etc., harmonic M.M.F. waves produced by the respective fifth, eleventh, seventeenth, etc., harmonics in the current wave will all revolve at the same speed as the poles but in the opposite direction to the poles.

The M.M.F. waves which are stationary over the poles will produce their full effects, as they are not subjected to the damping influence of the amortisseurs. But since in the majority of cases the harmonics of the current are small compared with the fundamental, it is evident that

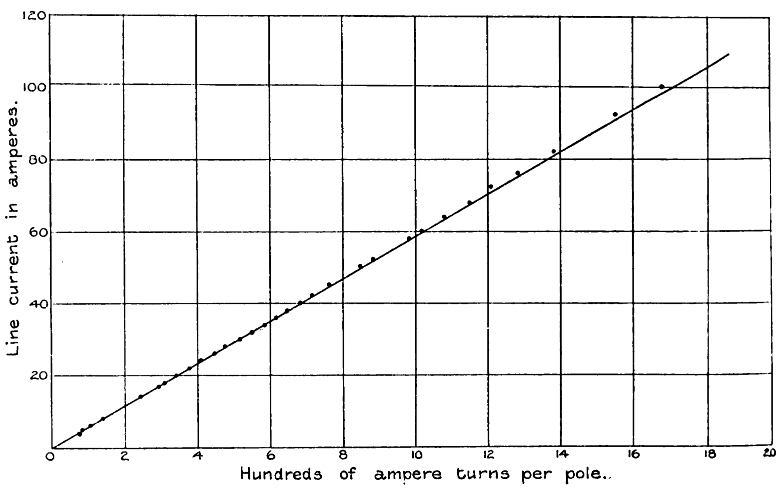


FIG. 13.—The full line is the Theoretical Short-circuit Characteristic of the Three-phase Generator. The points show the corresponding experimental results.

the harmonic M.M.F. waves which they produce will be of a still smaller order of magnitude when compared with the fundamental M.M.F. waves produced by the fundamental of the current.

It is interesting to note here that when the current wave is not of sine form and we wish to consider its magnetising action in the armature, we ought to find the value of the fundamental of the current wave and not the equivalent sine wave which includes all the harmonics. We have dealt in the experimental part entirely with effective values of the currents, that is, with equivalent sine waves, and therefore the magnetic effects we have calculated would be slightly too great if the current wave were a distorted one.

Influence of the Poles on the Armature Self-induction.

There is one other possible condition which may have potent effects on the result in particular cases, namely, the presence of the main field

flux affecting the flux of armature self-induction. We have taken account of the main field flux as a whole, but we have neglected the consideration of the minor fluxes which go to produce some of the higher harmonics. It is possible to imagine in certain cases one of these local fluxes being interlinked with one or more sets of the armature slots, and varying at a rate corresponding to the variation of the armature current, so that the main field really supplied the E.M.F.

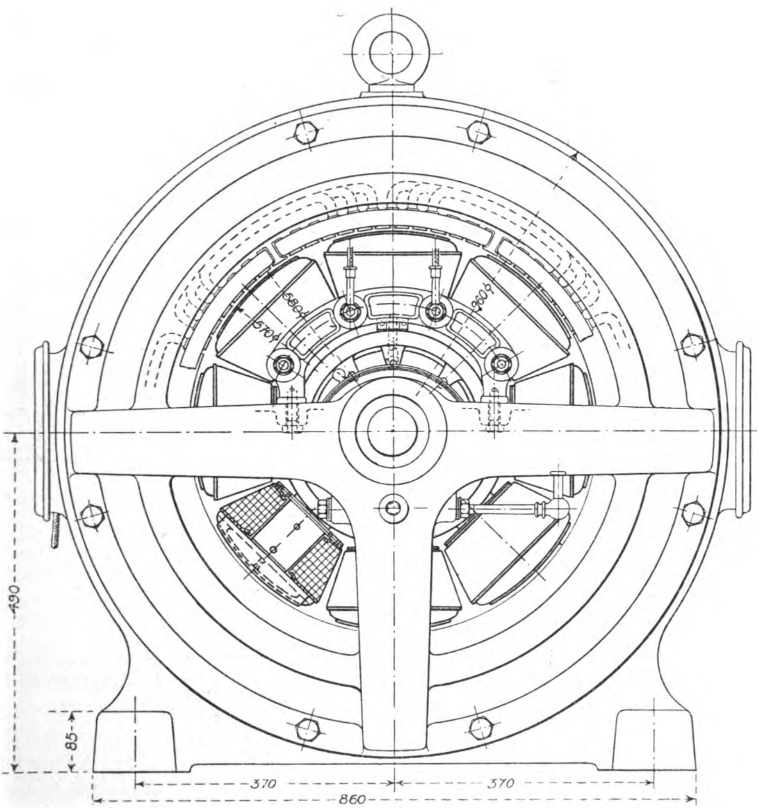


FIG. 14A.

necessary to overcome a portion of the self-induction of the armature. There can be no great influence of this nature present in every case, but there may be minor ones present at particular power-factors. We ought really to study the flux distribution relatively to the armature conductors when the current is varying at the maximum rate, and this will vary considerably at different power-factors. The full consideration of this and all the many other minor effects would require long researches to themselves, even if investigation were in every case possible.

As the oscillograph can be used to throw still further light on the

phenomena discussed in the previous pages it is now being used for a further research.

Short-circuit Characteristic.

The short-circuit characteristic expresses the armature-current in short-circuit as a function of the field excitation. We can deduce the theoretical short-circuit characteristic in a manner similar to that which

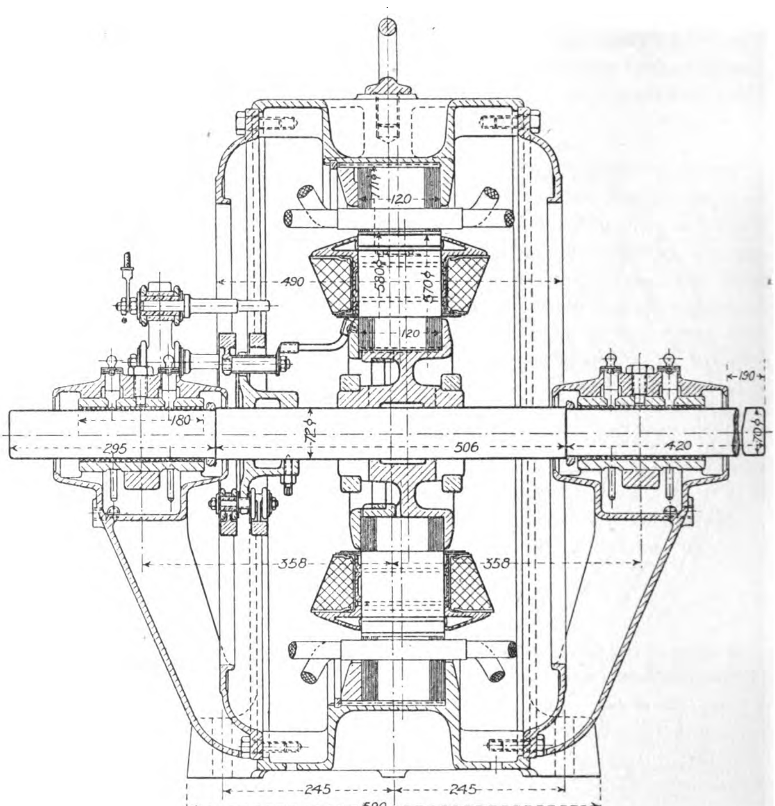


Fig. 14B.

we have adopted for the load curves, because each point on a short-circuit curve corresponds to a certain point on a load curve—the point for which the terminal E.M.F. is zero. In Fig. 7, therefore, for a short-circuit curve, OA will be zero, AB, BC, and CD will all remain proportional to I, and E_0, or OP becomes almost parallel to and equal to BC, and is also proportional to I, and ϕ is approximately 90°. All the lines in the diagram are then proportional to I, and, since all the values of E_0 with which we have to deal in the short-circuit curve lie on the straight portion of the magnetisation curve, and are proportional to X_R,

it follows that X_R must be proportional to I. The excitation $X_{D.M.}$ is also proportional to I, therefore the field excitation X must be proportional to I, and the short-circuit curve must be a straight line.

In our particular case r the resistance of each phase winding plus the connections up to the short-circuiting point, was 0·036 ohms., $x =$ 0·214 ohms., $E_{C.M.}/\cos\phi = 0·472$ I. Constructing the diagram of Fig. 7 gives us $E_0 = 0·2155$ I. From the magnetisation curve we get $E_0 =$ 0·0625 X_R, therefore $X_R = \frac{0·215}{0·0625} I = 3·44$ I. $X_{D.M.} = 13·7$ I, therefore $X = 17·1$ I is the equation to our short-circuit characteristic. A comparison between this curve and the experimental short-circuit curve is shown in Fig. 13.

Summary of Results.

The theoretical load curves of the three-phase generator, which have been calculated in the paper by a novel method, employing, like Blondel's, a Fourier analysis of the armature M.M.F.'s, agree closely with the experimental load curves, the agreements being within the limits required in matters of design. Owing to the special precautions which were taken with the experimental work, the experimental errors have been reduced to a small fraction of the maximum deviation between the theoretical and experimental load curves, and the discrepancies between theory and experiment, although small in themselves, are yet definite in direction.

The deviations which have been anticipated by considering the imperfections or the incompleteness of the theory are at least qualitatively such as are found to exist.

The authors desire to express their thanks to the Carnegie Trust Commissioners for a grant for special instruments used in this research.

DISCUSSION.

Professor F. G. Baily: I think Dr. Henderson's curves are almost able to defy criticism, because, no matter what the severe mathematician may say about his theory, it brings out the right results. I was doubtful at first as to whether the amortisseur coils would behave with such complete success, but there is very little doubt that they have almost completely eliminated the higher harmonics, although Dr. Henderson does not give the curve of actual electro-motive force. Professor Baily.

Dr. Henderson: On no-load there is a slight deviation on the phase voltage, due to the third harmonic, but on the line voltage there is none. Dr. Henderson.

Professor Baily: The higher the harmonic the more effective will the amortisseur coils be, because the higher the periodicity the greater is the inductive effect and the greater the effect on the field magnets in obliterating the variation due to the current. It would be very interesting to know whether the coils gave large indications of reversing currents or obliterating currents, and to know what value these currents amount to. One might think that they would amount to a good deal, because on examining the curves it will be Professor Baily.

Professor Baily.

found that they differ very much from the sine curves, and a great deal of the variation in some figures would have to be obliterated somehow or other. We cannot possibly get a resultant sine curve when an E.M.F. anything like figure 5 is interposed even to a very small extent. Dr. Henderson has assumed in the diagrams that they were pure sine curves, and he has taken an average sine curve in order to deal with them in their polar co-ordinates, but that, of course, is only an approximation, as one could see, and I suppose the whole balance must be taken up by the eddy currents in smoothing out the little eccentricities. Almost the only point which strikes me is the bold elimination of these difficulties, trusting to the eddy currents doing their work much more efficiently than one would have expected from one's own inner consciousness.

Mr. Mavor.

Mr. HENRY MAVOR: It is quite impossible in the workshop to attempt any such investigations as Dr. Henderson has put before us this evening, and the assistance we get from such investigators as Dr. Henderson is very welcome. I would like to ask him, if possible, to make these experiments on a larger machine. Professor Baily has pointed out the doubt arising in one's mind as to whether, in looking at these results, they do not appear almost too good. We are quite familiar with the fact that cross-magnetising effects are comparatively of small importance on small machines, and if we have to deal with machines three or four times the pole area radial width we shall probably find that the divergence between calculation and experiment will be much greater. At the same time the close agreement between the experiment and the theory is very promising and gives confidence in the results so far as they applied to such machines, and it is to be hoped that Dr. Henderson will be able to pursue his very valuable investigations into the larger sizes. There is one little point that I should like to ask Dr. Henderson. In dealing with the induction and reactance of the armature coils he speaks of removing the pole shoes. That is the proper thing to do to get the reactance of the coils alone, but the reactance in the actual working of the machines is complicated by the presence of the poles, and these were not present while the experiment regarding the reactance was being carried out.

Mr. Hird.

Mr. HIRD: I think the methods described by Dr. Henderson seem so perfect that there is little to criticise in them. I have failed to get any light on the point as to how the ordinates of the square curve in Fig. 2 are arrived at, and I should feel personally obliged if Dr. Henderson will give us some information on that point. It is quite easy to see how to get the harmonics, but I fail to see how the square curve is arrived at.

Dr. Henderson.

Dr. HENDERSON: The value of the ordinate "a" is given on page 466 under Fig. 2, the factor $\sqrt{2}I$ gives the maximum current, the remainder of the expression being half the turns per element A of Fig. 1, and there are three steps in the curve corresponding to the three coils in the element.

Mr. Hird.

Mr. HIRD: After that I can only add my congratulations to those already given to Dr. Henderson for the wonderful agreement he has obtained between the theoretical and the experimental results.

Dr. Henderson.

Dr. HENDERSON (*in reply*): I may say with regard to Professor Baily's remark about the action of the two fundamental M.M.F. waves, that, as a matter of fact, there are six fundamental M.M.F. waves, or two for each phase winding, and I can show them diagrammatically on the board in their relative positions at a given instant of time. The three waves moving in the opposite direction to the poles combine to give no resultant, while the three moving with the poles are in the same phase and add their effects. In the expressions for Y_{A1}, Y_{B1}, Y_{C1}, on page 468, which represent the fundamental stationary waves, it will be noticed that each expression contains the product of two sines. Each can, therefore, be resolved into the difference of two cosines. If this is done it will be found that one cosine term represents a train of waves moving at synchronous speed in one direction, while the other represents an equal train of waves moving at synchronous speed in the opposite direction. On adding the six terms together, it will be found that the three terms containing the one cosine disappear, while the three containing the other cosine are all equal, and can be added together arithmetically, the result being given on page 469. The constant 1·5 in that result corresponds to three times one half, the maximum ordinate of the moving wave being one half of the maximum ordinate of the stationary wave. The mathematical part of the paper is cut down to the minimum, and it might perhaps have been better if the physical interpretation of the phenomena, to which I have confined my descriptions to-night, had been added to the paper, but the exigencies of space forbade it. Professor Baily is astonished at the damping out of the effects of the higher harmonics in the M.M.F. curve, and, personally, I am surprised that they play such a small part. It would also be very interesting to find what currents were passing through the amortisseurs.

Mr. Mavor suggests experimenting with a larger machine, and the results will undoubtedly be interesting. The results depend, however, only on the ratio of the pole breadth to pole pitch, the harmonic analysis of the armature reaction being independent of the size of the machine, so that in any large machine with a similar winding and the same ratio of pole breadth to pole pitch and equal saturation of the iron the results should be similar.

Regarding the experimental determination of the armature impedance, if the poles are not removed when determining it, the result obtained is a combination of the true impedance with cross magnetisation, which varies for different positions of the poles, but if the poles are placed concentric with the elements of the winding in which the current is passing, then the impedance differs very slightly from that obtained with the poles removed. In such an experiment if there were no amortisseurs the field coils would have to be short-circuited during the experiment.

The experiments were begun to determine in how far these M.M.F.'s, in the analysis of the total armature M.M.F., which theory indicates should be stationary over the poles can account for the effects obtained in practice, and the results have been most satisfactory. Many difficulties were met with, not only difficulties of experiment, but also

Dr. Henderson.

of theory which was free from objections, in obtaining a method for determining the theoretical load curves. These difficulties were all tackled as they arose and overcome. In conclusion, I have to thank you for Mr. Nicholson and for myself for the interest you have taken in the paper, and I trust the results may be of use to many.

LEEDS LOCAL SECTION.

CONDENSING ARRANGEMENTS IN CENTRAL STATIONS.

By J. D. BAILIE, Associate Member.

(*Abstract of Paper read January* 19, 1905.)

In installing condensing plant, sufficient attention does not always seem to be given to the question of the most suitable type, or to the general arrangement of the condenser itself and its accessories, and the paper was compiled with the object of describing the leading forms of apparatus employed for the purpose, and promoting a discussion on the subject.

EJECTOR CONDENSERS.—Probably the simplest form of condenser in general use is the ejector. It comprises a conical water nozzle leading to a condenser tube, usually vertical, and contained in an outer shell which serves as a steam space. The cooling water is delivered to the nozzle under a pressure of some 7 lbs. to 9 lbs. per sq. inch, so giving it a corresponding velocity of discharge from the nozzle and in the condenser tube. The two types of ejector condensers in general use are the Korting and the Ledward. In the former, the condenser tube is perforated with a number of rings of holes, drilled obliquely so that the exhaust steam meets the central moving column of water at a suitable angle. In the Ledward ejector, the condenser tube takes the form of a series of cones.

JET CONDENSERS.—In the jet condenser the water is delivered into the exhaust steam space in the form of spray, and a pump is employed to overcome the pull of the vacuum. This may be in one and serve as a water and air pump combined; or there may be two separate pumps, one, connected to the top of the condenser, being used solely for abstracting the air and vapour, and the other, connected to the bottom, for the condensed steam and cooling water.

In some condensers the exhaust steam and the cooling water are arranged to flow in the same direction; these are known as parallel current condensers. In others the steam and the water are arranged to flow in opposite directions; these are described as counter current condensers. In the latter type both the steam and the water have their greatest temperatures at one end and their lowest at the other. In the former the cooling water must leave at a temperature lower than the lowest temperature of the steam, whereas in the latter

the water may leave at a temperature only slightly less than the highest temperature of the steam, which means that more cooling water must be used with parallel than with counter current condensers to achieve the same result. The counter current principle is frequently applied also to surface condensers.

THE EVAPORATIVE CONDENSER.—This consists of a stack of corrugated iron tubes with radial flanges exposed to the air, over which water trickles. The quantity of cooling water required is comparatively small, and, in practice, may be from one-half to two-thirds that of the steam condensed, the loss being due to evaporation. The condenser tubes being exposed to the air, the condensation of the steam is chiefly due to radiation. The usual tube surface allowed is, say, two-thirds of a square foot per lb. of steam. If the tubes are contained in a casing and cold air driven or drawn in among them by a fan, the cooling surface may be somewhat reduced. An air pump is provided for producing a vacuum in the tubes and for passing the condensed steam back to the hot well. A sump is provided below the condenser for catching such of the cooling water as is not evaporated in its transit among the tubes. This type of condenser may be erected on a roof, so saving ground space, but this of course introduces the question of draining the exhaust steam pipe, and the effect on the vacuum of the upward flow of the steam.

THE SURFACE CONDENSER.—Surface condensers are made both horizontal and vertical, and the advantage of their use is in keeping the condensed steam separate from the cooling water, so rendering it suitable for passing back to the boilers. Frequently a feed-water heater is interposed for raising the temperature of the condensed steam before it is fed back to the boilers, and this may be either separate from the condenser or combined with it.

The air and circulating pumps may be driven in any convenient way, either direct from the engine, by separate engine, or by electric motor. One objection to driving the pumps from the main engine is that, in the event of a breakdown of the pumps, the engine has to be stopped whilst the pumps are being disconnected; another is that when both air and circulating pumps are driven from the engine, the inequality of the work done by the two pumps tends to affect the governing—this is especially objectionable with alternating-current plant. Driving the pumps by a small separate engine has its advantages; they are then independent of the main engine, and can be started up before it, so enabling the main engine to be run up on the condenser.

Circulating pumps may also be either of the reciprocating, screw, or centrifugal type. The first-named are usually the more efficient, especially when new, but require some attention when running. Where the source of water supply is more than, say, 10 ft. to 12 ft. below the level of the pump, the reciprocating pump, owing to its power of lifting, is more or less imperative. The screw pump is sometimes used where the suction head is very low indeed, and the delivery head does not exceed, say, 10 ft. When the suction head does not exceed say, 10 ft. the centrifugal pump is to be preferred, which, though not so

efficient as the reciprocating type, maintains its initial efficiency, has no valves to be damaged by impurities and foreign matter in the water, runs smoothly, and delivers a constant stream of water, and lends itself admirably to being driven direct by an electric motor.

There are many more or less efficient forms of air pumps to be had, and probably the "Edwards" air pump is one of the best known in this country. The action is direct and mechanical, and, owing to the form of the plunger and chamber, the water is displaced quietly and smoothly.

Another excellent air pump is the "Parsons" compound single-acting air pump, which may be used either as a wet or as a dry pump; in the latter case a small quantity of water is let into the pump chamber by means of a pipe and valve—which water both primes, lubricates, and fills up clearances. Water only is used as a lubricant. In the event of the circulating water pump failing whilst the air pumps are in use there would be no trouble from flooding, as they would be working under ordinary conditions.

One frequently hears that the maintenance of high vacuum involves the use of abnormal quantities of cooling water and cooling surface, abnormally large capacity air pumps, prohibitive initial and working costs, etc. However, with properly designed condensing arrangements, and cooling water at a moderate temperature, there is no difficulty in maintaining a vacuum of 28 inches. All that is necessary, assuming the cooling water to be at a moderate temperature, is that the type of condenser adopted be selected intelligently and with due consideration of the local conditions.

A great deal may be done by the use of properly designed and efficient air pumps, such as those described above. With the steam turbine the gain per inch at the higher vacuum is so great that a vacuum closely approaching the barometric column is aimed at, and obtained in regular working by not depending upon the main air pump alone to abstract the air and vapour. One well-known firm of condenser builders use with surface condensers a small auxiliary air pump, whose function is to deal only with the air and vapour, the main air pump dealing with the condensed steam. Probably the simplest and most efficient method at present in use for the purpose is the Parsons Vacuum Augmentor. It comprises a pipe passing from the main condenser to a small auxiliary condenser; the pipe is fitted with a cowl at its top end, to prevent water in the form of condensed steam falling into it from the main condenser tubes, and, in a contracted portion of the pipe is placed a small steam jet, which draws from the main condenser the air and vapour left by the main air pump, and delivers it to the auxiliary condenser, where it is condensed and led thence to the common air pump. A water seal is provided to prevent return of air and vapour to the condenser. The injection water for the auxiliary is obtained from the main supply by means of branch connections, so that the same circulating pump suffices. The jet compresses the air and vapour, reducing its volume, so that the vacuum in the air pump may be considerably less than that in the condenser, which means that the air pump may be smaller. There are no moving parts in this arrangement, the minimum

of attention is necessary, and the steam consumed by the jet is but small, being about one per cent. of the total full-load consumption of the plant. A vacuum within 0·33 in. of the barometer has been attained.

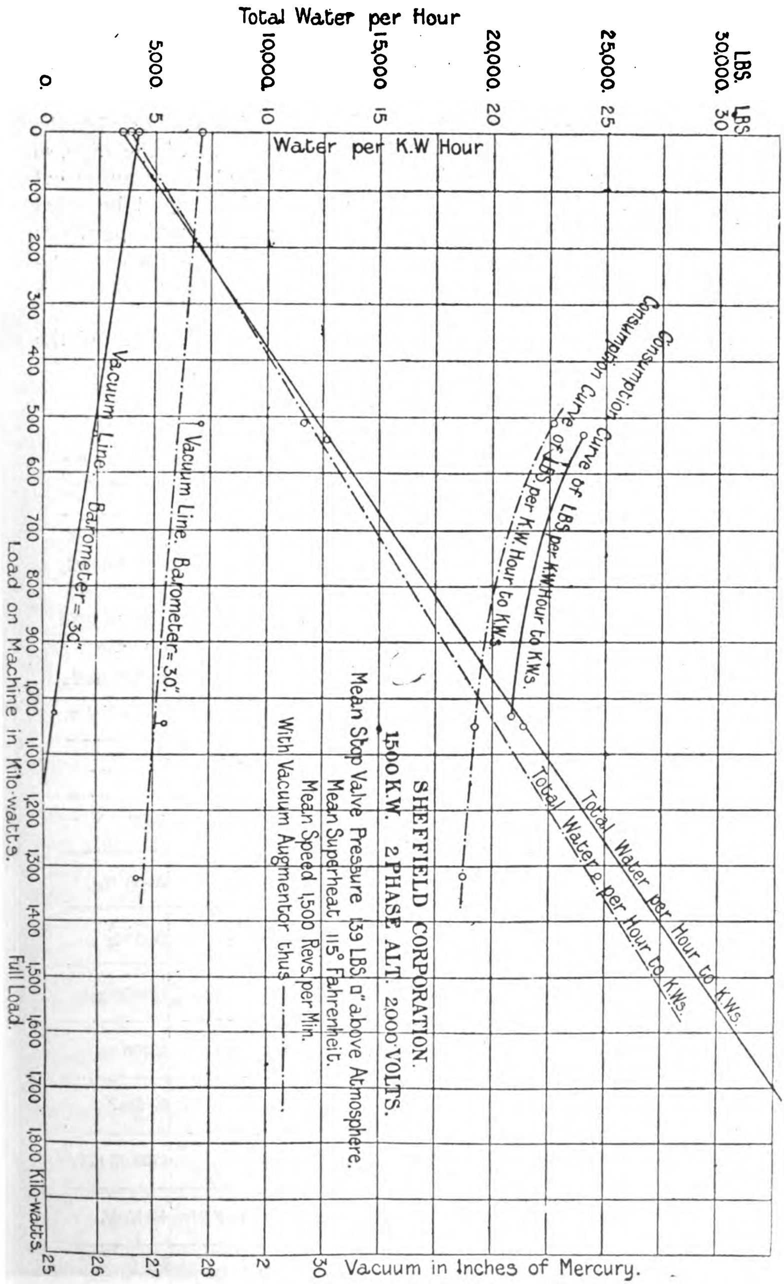
Whether to adopt one large common condensing plant for a station, or a number of smaller ones, is a question concerning which no hard-and-fast rule can be given. The former saves in initial cost and attendance. If, however, several condensing plants be installed, it means that break-down of one, or even two of them, does not necessarily seriously affect the economical running of the engines, as, by means of inter-communication pipes and valves, it can be arranged for any engine to run on another condenser.

COOLING WATER.—Before deciding upon the type of condenser to adopt, one of the most important considerations is the water supply available. Its quantity and temperature, at various times and seasons, should be ascertained, as well as its quality. Fortunately for many central lighting stations the quantity available is greatest, and its temperature lowest, in the winter-time, when loads are heavy. Where there is always an ample supply of cooling water to be had, the average and the maximum temperature should be ascertained, as upon these will depend the proportions of the condensing plant. Whatever the conditions as to quantity and temperature, the water should be analysed, in order that its composition and probable effect on the condensers, pumps, etc., may be known. If cooling towers are used, the surrounding atmosphere should be taken into account, for since the same water is used again and again, it may take up impurities from the atmosphere, chemical action resulting, and so converting what was originally harmless water into a corrosive medium.

Many devices have been adopted for re-cooling the injection water, including ordinary open reservoirs, reservoirs with spraying arrangements, cooling towers, etc.

The general principle in all cooling towers is to spray or otherwise split up the warm water by distributors and baffling devices into small particles which become cooled by contact with a current of air. It is usual to deliver the warm water to the tower at a considerable height by means of a circulating pump, from whence it falls by gravity through the various baffles, into a sump immediately below the tower, from which it is pumped back to the condensers and used over again—the process being repeated indefinitely. In some cases the air is circulated through the baffles by means of a fan, in others by natural draught. The former type are compact, but a certain amount of power is absorbed in driving the fan, and, in general, the author prefers the natural draught towers, which, if properly arranged, will satisfactorily cool the water.

These towers are usually some 60 feet to 80 feet in height, and, as the water is delivered to them at, say, 25 feet to 30 feet above the ground, the top 35 feet to 50 feet forms a chimney for inducing the draught of air through the baffles, and, at the same time, it discharges such vapour as escapes at a sufficient height to prevent its being objectionable. The quantity of vapour discharged is also less, as some condensation takes place in the chimney. The baffles take various forms, such as grids,


Total Water per Hour
LBS.
0. 5,000. 10,000. 15,000. 20,000. 25,000. 30,000.
0 5 10 15 20 25 30 LBS.
Water per K.W Hour
Consumption Curve of LBS per K.W. Hour to K.Ws.
Consumption Curve of LBS per K.W. Hour to K.Ws.
Total Water per Hour to K.Ws.
Total Water per Hour to K.Ws.
Vacuum Line. Barometer = 30"
Vacuum Line. Barometer = 30"
SHEFFIELD CORPORATION.
1500 K.W. 2 PHASE ALT. 2000 VOLTS.
Mean Stop Valve Pressure 139 LBS. above Atmosphere.
Mean Superheat 115° Fahrenheit.
Mean Speed 1,500 Revs. per Min.
With Vacuum Augmentor thus
0 100 200 300 400 500 600 700 800 900 1,000 1,100 1,200 1,300 1,400 1,500 1,600 1,700 1,800 Kilo-watts.
Load on Machine in Kilo-watts.
Full Load.
25 26 27 28 29 30 Vacuum in Inches of Mercury.

laths, drain pipes, etc., their function being to ensure thorough breaking up of the water.

SUCTION AND DISCHARGE PIPES.—Where practicable, a supply pipe should be led from the canal, or other source, to a sump, and this pipe should have a fall so that the water will gravitate from the canal to the sump. A penstock should be fitted to the sump end of the pipe, so that the canal water can be shut off when desired, for cleaning out the sump, etc. The sump end of the suction pipe should be fitted with a foot valve and strainer, the former to prevent anything which may have fallen into the sump being drawn into the suction pipe, and the latter to assist the pump by keeping the suction pipe, when once filled, charged with water.

Though they should, of course, be properly fitted, there is not the

CONDENSER TESTS.

STATION.	HULTON COLLIERY.	NEEPSEND WORKS.	WEST BROMWICH.	M'CHESTE
TYPE OF CONDENSER.	Ledward Ejector	Parsons Surface.	Parsons Surface.	Jet. Wet & Dry Pumps.
TYPE OF AIR PUMP.	———	Parsons 3 Thr'w C'mp'nd	Parsons 3 Thr'w C'mp'nd	Parsons 3 Thr'w C'mp'd
TYPE OF CIRC. PUMP.	Gwynne Centrifugal.	Gwynne Centrifugal.	Gwynne Centrifugal.	Gwynne Centrifugal.
PUMPS DRIVEN BY.	B.T.H. Motor	B.T.H. Motor.	B.P. Motor.	E.C.C. Motor.
COOLING SURFACE.	———	3000 Sq. Ft.	1000 Sq. Ft.	———
TEMPERATURE of COOLING WATER.	67·6° F.	65·8° F.	51° F.	82° F.
VACUUM AT CONDENSER.	26·17" Hg.	27·7" Hg.	28·2" Hg.	26·66" Hg.
BAROMETER.	30·05" Hg.	29·9" Hg.	29·8" Hg.	29·5" Hg.
COOLING WATER PER HOUR.	48100 gals.	88500 gals	50000 gals	178500 gals
STEAM COND'NS'D PER HOUR.	6996 lbs.	29530 lbs.	10000 lbs.	35700 lbs.
COOLING WATER PER LB. OF STEAM.	68·75 lbs.	30 lbs.	50 lbs.	50 lbs.?
TOTAL POWER GENERATED	314·4 K.W.	1610 K.W.	446 K.W.	1822·03 K.W
POWER USED BY PUMPS.	22·5 K.W.	37·4 K.W.	9·17 K.W.	40 K.W.
% age TOTAL P'W'R USED BY PUMPS.	7·1 %	2·3 %	2·05 %	2·2 %
HEAD AGAINST CIRC. PUMP.	47·5 Ft.	20 Ft.	15 Ft.	40 Ft.

same need of extreme care in the arrangement of the discharge pipes. They may also be of smaller area, and the velocity of the flow in them may be higher. It is good practice to water seal the end of the discharge pipe, when, assuming all joints are tight, some of the power for driving the circulating pump may be saved. By doing this a syphon action is set up when the pump is running, so eliminating the head due to the difference of level between the suction and discharge, leaving only the head due to pipe friction, plus, of course, that due to the condenser, etc. This method has been adopted at the Carville power-station, with the addition of a small motor-driven pump to extract the air from the pipes.

ORIGINAL COMMUNICATION.

THE MAGNETIC PROPERTIES OF SOME ALLOYS OF IRON AND SILICON.

By THOMAS BAKER, M.Sc., 1851 Royal Exhibition Scholar.

The investigation of these formed part of a research on the influence of silicon on the mechanical and physical properties of iron.

The alloys on which the determinations were performed were prepared by melting Swedish wrought-iron in specially prepared crucibles, and adding the required amount of silicon, in the form of ferro-silicon, to the "clear melted" iron. This method of preparation, though much longer than that of melting the iron and ferro-silicon together, yields a material much purer with respect to carbon and manganese, a condition of great importance, since small amounts of these elements exert a very marked effect on the properties of iron. The ingots were cast $1\frac{3}{4}$ inches square and afterwards rolled down to $\frac{7}{8}$ inch round bars, and were found on analysis to have the following chemical composition :—

TABLE I.

Ingot No. ...	691	728	745	723	782
Combined Carbon	0·044	0·038	0·038	0·040	—
Silicon	0·024	1·020	2·903	4·885	7·470
Manganese ...	0·036	0·079	0·061	0·072	0·210
Aluminium ...	0·010	0·016	0·069	0·061	0·050
Sulphur	0·030	0·038	0·041	0·027	0·011
Phosphorus ...	0·014	0·019	0·018	0·021	0·019
Iron (by diff.) ...	99·842	98·790	96·870	94·814	—

The mechanical properties of these alloys, after having been subjected to a temperature of 950° C. for 40 hours, and then allowed to cool over 170 hours, are included in Table II.

TABLE II.

Silicon per cent.	Elastic Limit tons per sq. in.	Maximum Stress tons per sq. in.	Reduction of Area per cent.	Elongation per cent. on 2 in.
0·024	19·87	22·62	71·5	47·0
1·020	20·04	27·18	76·2	40·2
2·903	21·29	33·90	60·0	35·5
4·885	coincides with maximum stress	41·46	Nil	Nil
7·470	coincides with maximum stress	21·06	Nil	Nil

From these results it will be seen that additions of silicon up to 3 per cent. have little effect on the mechanical properties of iron provided the material has been well annealed. The alloys containing 4 per cent. and upwards of silicon are hard and brittle, and require very great care in machining to avoid fracture of the bars.

The effect of silicon on the microstructure of iron is simply to increase the size of the crystals which go to make up the mass of the iron; this at least holds good in the case of the alloys containing up to 7·5 per cent. silicon, which is as far as the series has been investigated. The microstructure of the iron to which no addition of silicon has been made is shown in Fig. 1; it consists of allotrimorphic crystals of ferrite with a few small areas of cementite (Fe_3C) (due to the small percentage of carbon present) scattered throughout the mass. Fig. 2 is the structure of iron containing 2·16 per cent. silicon, and consists of allotrimorphic crystals of a solid solution of iron and what is probably a definite silicide of iron.

In order to study the magnetic properties of the alloys it was necessary to adopt a method which would not involve the making of another set of specimens; and as the alloys were in the form of bars, the Bar and Yoke method was accordingly adopted.

The yoke made of Swedish wrought-iron was in halves, and so constructed as to be of uniform cross-section throughout (Fig. 3). Each half had a semicircular groove cut in it with a radius equal to that of the bars employed (0·492 cm.), so that when the halves of the yoke were bolted together the bar was in as good contact with the yoke as possible. The length of the magnetising coil was 12·2 cms., and consisted of 526 turns, with a mean radius of 1·399 cms., wound on a brass reel, so that the bars could be rapidly interchanged; the secondary, wound over the middle of the primary, consisted of 121 turns.

The magnetising current was controlled by a series of shunts, and its strength measured by a galvanometer of the D'Arsonval suspended coil type, very carefully graduated with a Kelvin standard centi-ampere balance, so that one scale division was equal to 0·1 unit

magnetic field. The value of the magnetic induction was determined ballistically by a very excellent Crompton bi-filar ballistic galvanometer the deflections of which were calibrated by a specially wound standardising coil, the primary of which was 82·2 cms. long and consisted of 1,254 turns, with a mean diameter of 2·95 cms. The secondary, wound over the primary equidistant from the ends so as to be in as uniform a field as possible, consisted of 2,392 turns. The mutual induction of these coils was found by calculation to be 0·0002887 henry.

The magnetisation curves were determined by the method of reversals, but in the case of the hysteresis curves a step-down method suggested by Dr. Hicks, F.R.S., was employed in preference to the

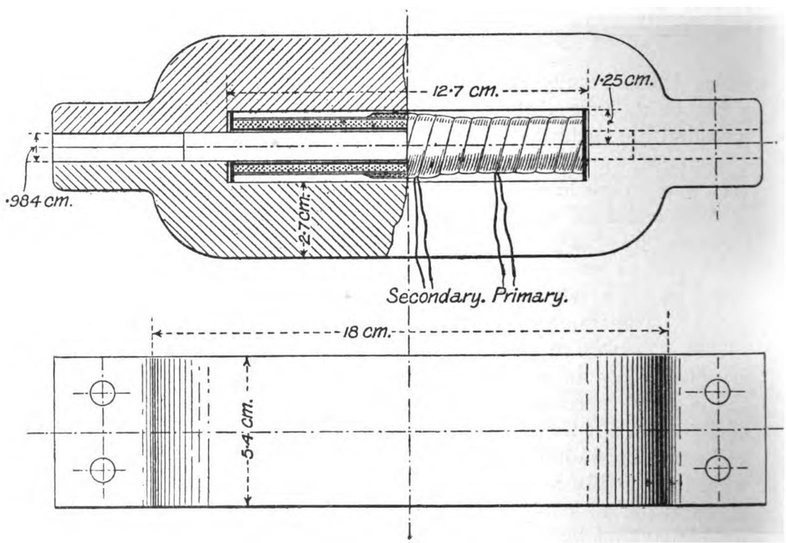


Fig. 3.

customary step-up one. The test is brought to its maximum induction, then the current suddenly reduced, and the change in the induction observed; this method of procedure is continued right round the cycle. In order to bring the magnetic field from its maximum value through zero to a small negative value, a reversing key is employed so arranged as to bring an adjustable resistance in series with the magnetising coil when the current is reversed. This, though a step method, is free from the fault of the ordinary step method where the errors are cumulative, inasmuch as an error made in any reading does not affect the following one, and if of any moment can readily be detected when plotting the curve. The hysteresis losses were calculated from the areas of the cyclic curves as determined by a planimeter. From the constants of the coils already given, if :—

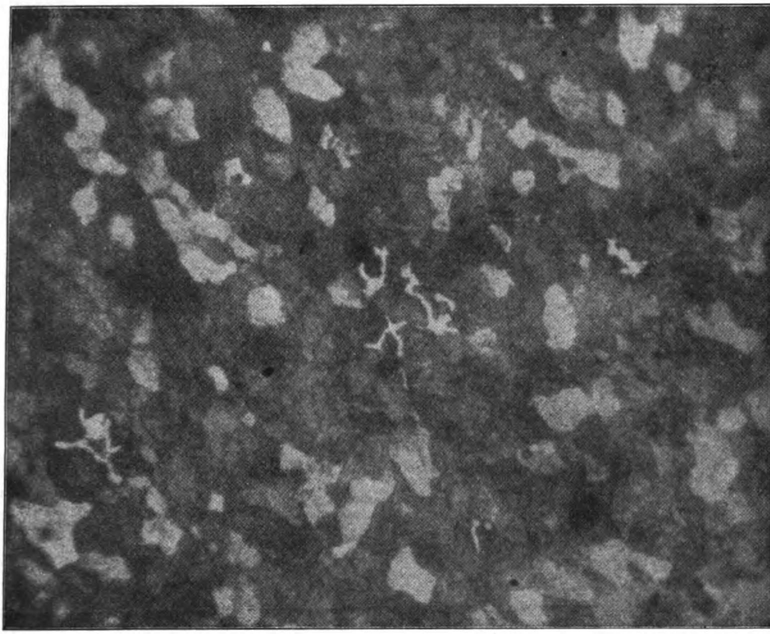


FIG. 1.—Microstructure of Alloy 691. Magnification, 360 diameters. Mottled appearance is due to a thin film of oxide over the surface of the section. Silicon, 0·04 per cent.

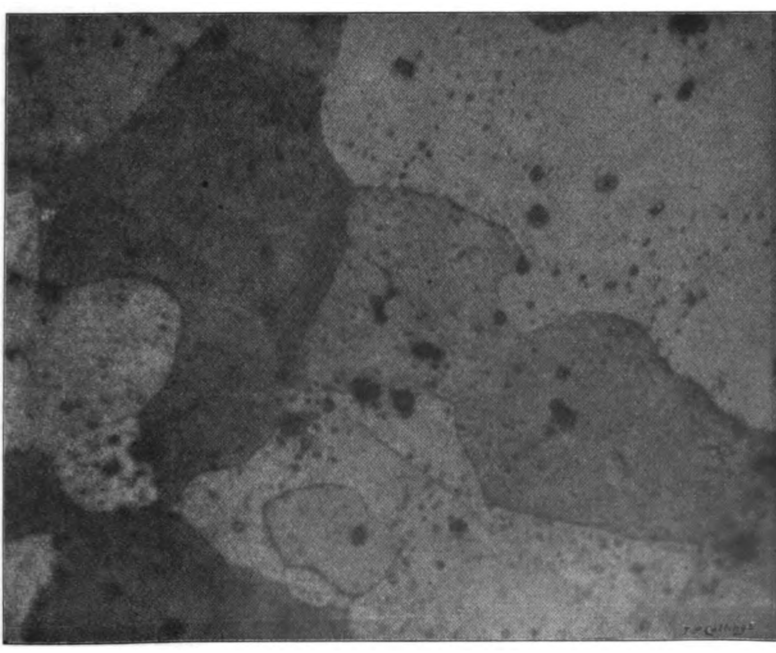


FIG. 2.—Microstructure of Alloy 722. Magnification, 360 diameters. Silicon, 2·16 per cent.

θ = throw of galvanometer for a primary current change of C amperes,

C = primary current change in amperes,

θ^1 = throw of galvanometer for current change of C^1 amperes in standard coil,

d = diameter of iron core,

the following formula for the magnetic induction was obtained :—

$$121 \frac{\pi}{4} \left\{ Bd^2 + 48{\cdot}7\,C \right\} = 288700\,C^1 \frac{\theta}{\theta^1}$$

$$B = \frac{288700}{\frac{\pi}{4} \times 121} \cdot \frac{C^1}{d^2} \cdot \frac{\theta}{\theta^1} - 48{\cdot}7\,C$$

The correction (— 48 C) for the air space between the bar and the magnetising coil was neglected in every case, as in no determination would it exceed 50 lines per sq. cm.

The determination of the value of H is by no means so definite ; the magnetising force along the axis of the test-bar may be regarded as constant and equal to H. That through the yoke, which is of uniform cross-section, will also be almost constant and equal to, say, H^1. Then

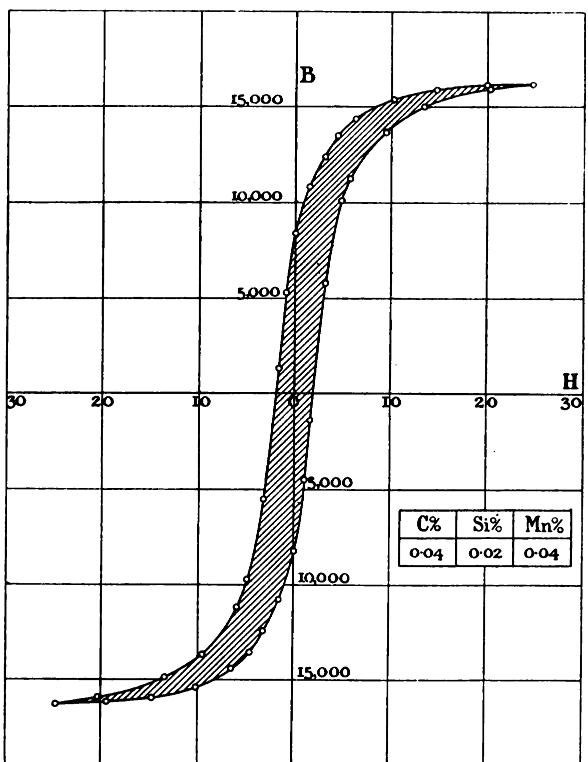


FIG. 4.

taking the path in the axis of the test-bar as 13 cms. and that in the yoke as 20 cms., the following formula is arrived at :—

$$13\ H + 20\ H^1 = 4\pi \frac{526}{10} C$$

$$H = \frac{210 \cdot 4}{13} \pi C - \frac{20}{13} H^1$$

$$= 50 \cdot 85\ C - 1 \cdot 5\ H^1$$

(where C is the current in amperes passing through the coil). The latter correction ($- 1 \cdot 5\ H^1$) was neglected, as it did not exceed our unit for the maximum induction determined.

All the results obtained are on specimens of the alloys annealed at the same time, so as to have the heat treatment identical. The magnetic induction and permeability of alloys containing 0, 1, 3, 5, and 7 per cent. silicon are given in Table III. The somewhat erratic behaviour of the alloy containing 4·89 per cent. silicon, the results of which should have fallen between those containing 2·9 and 7·4 per cent. silicon, is to some extent explained by the fact that the ingot was rolled at too low a temperature, in consequence of which the crystalline

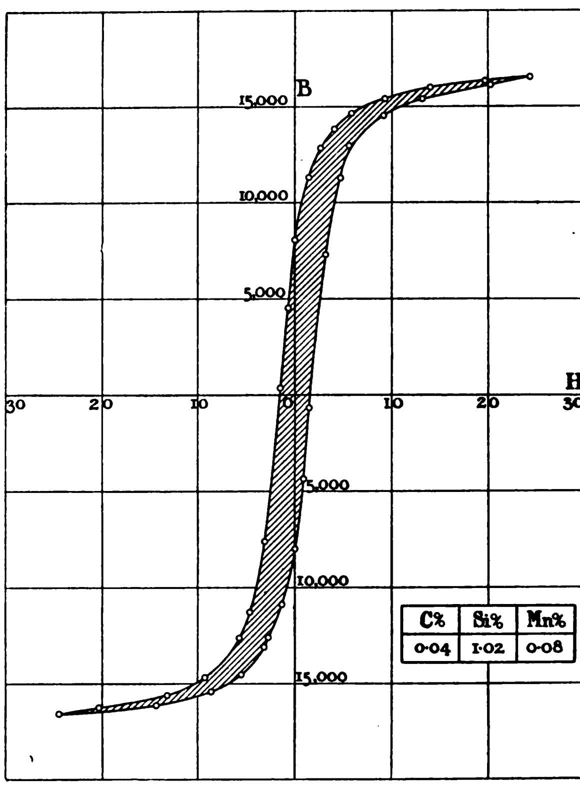


FIG. 5.

structure, upon which annealing had little or no effect, is very much distorted.

TABLE III.

Silicon % ...	0·024		1·026		2·903		4·885		7·47	
H	B	μ	B	μ	B	μ	B	μ	B	μ
2	5,000	2,500	5,500	2,750	6,500	3,250	6,000	3,000	8,600	4,300
4	9,300	2,325	10,250	2,562	11,000	2,750	10,700	2,675	11,750	2,937
6	11,600	1,933	12,500	2,083	13,000	2,166	12,750	2,125	12,500	2,083
8	13,300	1,662	13,900	1,737	13,800	1,725	13,600	1,700	12,800	1,600
10	14,300	1,430	14,650	1,465	14,400	1,440	14,000	1,400	13,000	1,300
12	14,900	1,241	15,300	1,275	14,750	1,229	14,400	1,200	13,300	1,108
16	15,650	978	15,800	987	15,100	945	14,600	912	13,750	859
20	16,000	800	16,200	810	15,500	775	14,750	737	14,050	702

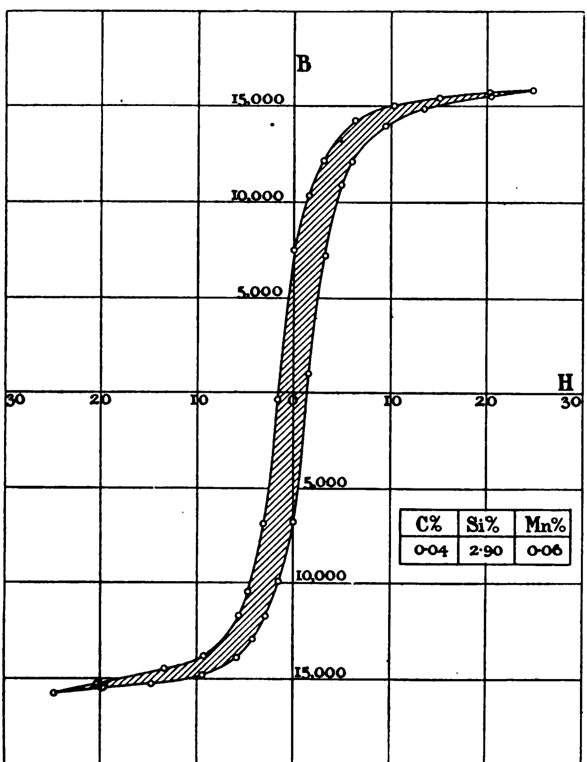


FIG. 6

Table IV.

Silicon per cent.	Maximum Induction. H = 25.	Permeability. H = 4.	Retentivity.	Coercive Force.	Energy dissipated per Complete Cycle.
0·02	16,250	2,325	8,350	1·8	10,550 ergs per cc.
1·02	16,500	2,562	8,060	1·7	8,978 „ „
2·90	15,750	2,750	7,320	1·5	8,081 „ „
4·89	14,900	2,675	7,270	1·2	6,110 „ „
7·47	14,450	2,937	8,900	1·0	5,613 „ „

Table V.

	H	B	
		Ring.	Yoke.
Magnetisation Curve.	2	2,250	2,250
	4	7,750	7,500
	6	10,200	10,000
	8	11,500	11,500
	10	12,350	12,600
	12	13,050	13,250
	14	13,550	13,650
	16	14,000	14,100
	18	14,250	14,300
	20	14,550	14,600
Down-coming Curve.	18	14,500	14,500
	16	14,400	14,250
	14	14,200	14,050
	12	14,000	13,800
	10	13,700	13,500
	8	13,250	13,200
	6	12,650	12,650
	4	11,750	11,750
	2	10,300	10,500
	0	8,000	8,400
	− 2	2,500	3,250
	− 4	− 7,500	− 6,250
	− 6	−10,250	− 9,800
	− 8	−11,600	−11,500
	−10	−12,600	−12,500
	−12	−13,350	−13,050
	−14	−13,800	−13,500
	−16	−14,200	−13,850
	−18	−14,500	−14,200

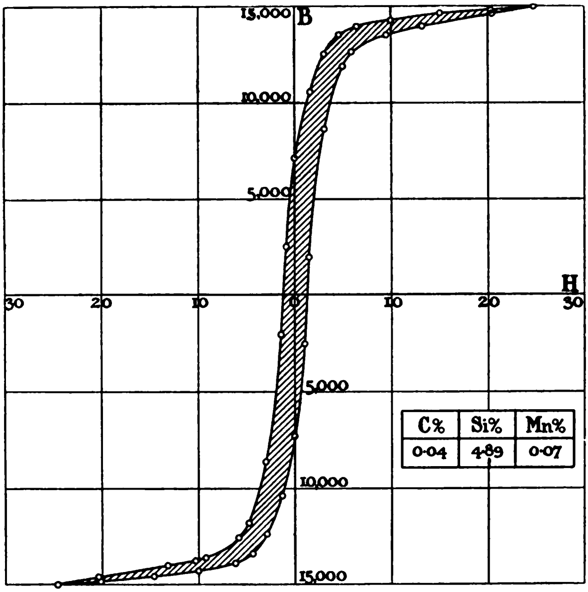

15,000
B
10,000
5,000
H
30
20
10
0
10
20
30
5,000
10,000
15,000
C%
Si%
Mn%
0·04
4·89
0·07

FIG. 7.

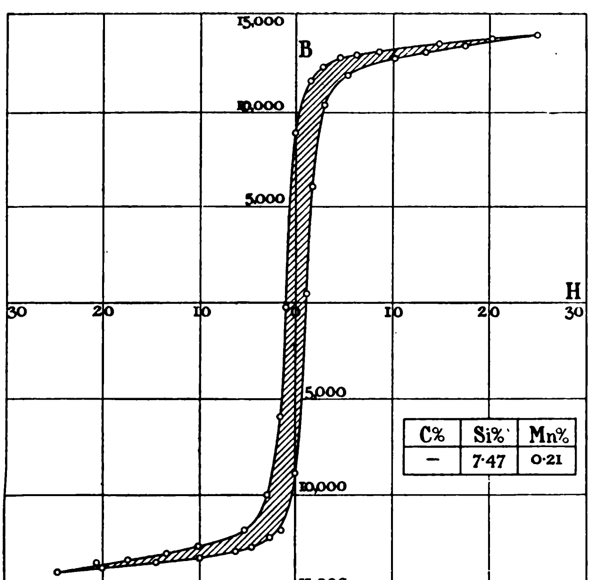

15,000
B
10,000
5,000
H
30
20
10
0
10
20
30
5,000
10,000
15,000
C%
Si%
Mn%
—
7·47
0·21

FIG. 8.

The presence of silicon in iron renders the iron more permeable, but lowers the maximum induction. The results obtained for the magnetic permeability of the alloys are not so high as those of Messrs. Barrett, Brown, and Hadfield; this, however, may be due to different heat treatment. The coercive force and energy losses due to hysteresis when iron is subjected to a magnetic cycle are very much reduced by the presence of silicon in the iron, as is shown in the hysteresis curves of the alloys (Figs. 4, 5, 6, 7, 8). A brief summary of the results obtained are embodied in Table IV.

In order to test the trustworthiness of the "bar and yoke" method, castings of a ring and bar were prepared of identical composition, heated to a temperature of 1,000° C. and allowed to cool in air. The ring after machining was perfectly sound, and had the following dimensions:—Outside diameter, 13 cms.; inside diameter, 12 cms.; depth, 2 cms.; the bar was of the dimensions of those employed in the research. The results of the comparison are given in Table V.

The agreement between the results obtained by the two methods is fairly good; the greatest difference occurs in the small negative magnetic fields in the down-coming curve, where small changes in the magnetic field produce great changes in the magnetic induction. In consequence of this, the coercive force as measured by the two methods is slightly different, the bar and yoke giving the higher result. In the preceding experiments the ring gave a coercive force of 2·2, and the yoke one of 2·60; so that in the determination of the magnetic properties of the alloys, the value of the coercive force in all probability is over-estimated. As all determinations, however, were made under similar conditions, they are strictly comparable.

The gradual reduction of the value of the coercive force and hysteresis loss by the addition of silicon to iron can hardly be accounted for by the removal of ferrous oxide, since this is brought about by a very small percentage of silicon, whereas the improvement in the magnetic properties continues with increasing silicon. A more probable explanation, and one which is supported by the results of the above experiments, has been given by Professor Arnold, and Dr. Hicks, F.R.S. (*Nature,* April 17, 1902), namely: "Those elements which give iron high permeability and low coercive force are those which cause it to crystallise in large crystals." The size of the crystals, which go to make up the mass of the iron, increases as the percentage of silicon in the alloy rises. It is well known that the presence of tungsten and chromium increase the coercive force and hysteresis loss in iron, and these are elements which favour fine crystallisation.

The work embodied in this paper owes its success to the many facilities possessed by University College, Sheffield, and to the sympathy and advice heartily tendered by Dr. Hicks, F.R.S., and Professor Arnold in every portion of the work. The thanks of the author are also due to Mr. J. C. Sheldon, A.I.C., for much valuable assistance in plotting the curves and making the drawings accompanying this paper.

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"Alloys of Iron and Silicon." R. A. Hadfield, Journal of the Iron and Steel Institute, 1882, No. 2, p. 222.

"On the Electrical Conductivity and Magnetic Permeability of various Alloys of Iron." W. F. Barrett, W. Brown, and R. A. Hadfield. Transactions of the Royal Dublin Society, vol. viii. part ix., also vol. viii. part i.; also Journal of the Institution of Electrical Engineers, vol. 31, p. 674.

"Recherches physiques et physico-chimiques sur l'acier au carbone." Carl Benedicks.

NOTICE.

The following publications can be obtained from the Secretary, or from Messrs. E. and F. N. Spon, Ltd., 57, Haymarket, S.W.:—

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// JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 34. 1905. NO. 172.

Proceedings of the Four Hundred and Eighteenth Ordinary General Meeting of the Institution, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday Evening, February 9, 1905—Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on January 26, 1905, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

James Fiddes Brown.
David J. Gadsby.
Arthur C. Devey.
Matthew McA. Gillespie.

From the class of Associates to that of Associate Members—

Percival F. Crinks.
Fred. Walton Green.
Herbert Turnbull.

Messrs. C. K. Falkenstein and W. J. Grey were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Members.

Alfred Falconer Ball.
Thomas Tomlinson.
Alexander Wyllie.

Associate Members.

Wm. Maxwell Hubert Butcher.
Tom Arthur Cunliffe.
Frederick William Davies.
Percy J. Hood.
Stanley Thomas Land.
Robert Wallace McClay.
Alfred Jacques Makower.
John Place.
Theodore Schontheil.
Robert John Shaw.
Robert Joseph Smith.
George Bransby Williams.
Frederick William Wyand.

Students.

C. S. Atkinson.
Thomas Morgan Barlow.
Harold Fuller Barnes.
Richard Gordon Barrett.
William F. Birch.
John Boyd Blake.
William Paul Brennan.
Edward John Dent Buckney.
William C. C. Chappel.
Douglas P. Dunn.
Oscar Faber.
Kenneth Byres Findlay.
Benjamin Stevens Fowler.
Charles Harrison.
Walter Lawrence.
Cecil Reid McGowan.
John Huie McMinn.
John Alexander Marshall.
Wm. Herbert Steele Mitchell.
Victor Muirhead.
Clifford Thomas Pressland.
Thomas Alexander Pudan.
Carl Price Richards.
Thomas Allan Rose.
Wilfred Holme Russell.
George Tingley.
Frank Lester Newall Tuck.
Conrad Vandermin.
Miguel Vela.
I. Douglas Whittall.
Geoffrey Hutton Wilson.
William Dennis Wivell.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. C. S. Vesey Brown, R. K. Gray, G. Semenza, G. O. Squier, Sir Charles Todd; to the *Building Fund* from Messrs. A. A. Crawford, H. W. W. Dix, C. F. Farlow, D. Henriques, W. R. Rawlings; and to the *Benevolent Fund* from Messrs. H. Alabaster, Major P. Cardew, V. K. Cornish, D. Henriques, H. A. Irvine, W. R. Rawlings, R. Robertson, W. E. Russell, M. J. E. Tilney, Captain R. F. Willis, and C. E. Winter, to whom the thanks of the meeting were duly accorded.

The discussion on Messrs. Booth and Kershaw's paper was concluded (see page 371), and the following paper was read:—

THE VALUE OF OVERHEAD MAINS FOR ELECTRIC DISTRIBUTION IN THE UNITED KINGDOM.

By G. L. ADDENBROOKE, Member.

(*Paper read February* 9, 1905.)

The electric transmission of power on a large scale has been carried out chiefly in districts in Europe and America where abundant water power was available and the cost of coal was considerable; that is, in districts where, as we should expect, the economic margin was greatest.

A great number of these transmissions consists of a long line of mains carrying the current to some point or area where distribution takes place.

In the United Kingdom the problem will usually be rather different. In the first place, except in a few instances which I do not propose to deal with specifically, the source of power will be coal. Now coal is portable, and, notwithstanding what has been said by some people, it is my view that in nearly all cases it will pay better to place the central station near to what may be called the "power centre of gravity" of the district to be served and bring the coal to it, rather than to place the central station at a greater distance from its work in order to save freight on coal.

It may be pointed out that progress is likely to accentuate the soundness of this argument, since improved methods of generating power, such as the use of gas engines necessitating less coal per kilowatt-hour output, will make the cost of carriage on the coal relatively less important, while the cost of carriage itself is more likely to decrease than to increase.

Consequently, whenever we have a power-station operated by steam or gas engines and using coal as its source of energy, as will be the case with nearly all the power-supply stations in this country, I think we may take it that such a station should be situated somewhere near the centre of the district it is intended to supply, except in special cases which we need not consider to-night.

That it is cheaper to generate power from coal on a large scale and in a central station than when the same total amount of power is generated by scattered units has become almost an axiom. If transmission cost nothing it would be sufficient to set down such a central station in any suitable district, and within a moderate period—if it were not over-capitalised and were economically administered—its supply would displace all other sources of power in the district. But this is not the case practically; transmission costs money and involves

losses, and consequently there arises the important engineering problem of how to make this capital cost and loss a minimum, seeing the smaller it is made the larger will be the field to the central generating station.

In supplying light the electrical engineer is dealing in a luxury and convenience, and the business is not altogether governed by ordinary commercial considerations. But in supplying electric energy for power purposes the whole question is purely one of economics and commercial desirability. No manufacturer will displace his present method of power supply unless he can see a distinct economic gain. Moreover, it will not usually be sufficient that the cost of central supply shall be equal to the cost of local generation; this will turn the scale in the case of extensions of old and for new works, but to displace existing means, some gain, including interest and depreciation on the capital to be expended, must usually be shown over the existing method. Moreover, it is desirable that the difference between local generation costs by existing methods and the power-supply company's price should be such that manufacturers are tempted to change over fairly quickly and without waiting till plant breaks down or exceptional business demands additional power. On the other hand, during the early days of an enterprise, it is difficult to produce as cheaply as when a large demand has been created and matters have settled down to a routine.

For these reasons, therefore, and for many others which I will not attempt to enumerate, it is desirable if electric power supply from central stations in this country is to make way rapidly, that the costs of generating the energy and the costs of distribution between the power-house and the terminals of the consumers' motors should be kept as low as possible, and this apart from the general proposition that it is for the good of manufacturers first and the country generally afterwards that power in its most convenient form should be generally available over large areas at the lowest prices commercially compatible with a fair return on the capital needed.

The cheap production of power within the central station itself is a part of the question I do not propose to deal with directly in this paper, as I expressed my views on this subject in an article in *Engineering* which appeared in its issue of the 3rd of June, 1904, and what I say here accords with the deductions then made. I shall consequently here assume that everything is done in this way which experience and the state of our knowledge dictates. All that it is necessary to say here is that to secure good results the greater the amount of power which can be supplied from a given station the better, and also the better the load factor the lower the average price which can be charged. It is also obviously important to get a good load on the station quickly after it is ready to start.

Now in order to get a large load in a central station, to get such a load quickly and to obtain a good load-factor, unless the station is situated in an exceptionally rich district, it should be in a position to invite customers from as large an area as possible, because it is not likely that manufacturers within range of the mains will change over

the whole of the arrangements in their establishments to electric driving immediately; they will usually commence by adopting electric driving tentatively for some particular purpose, and gradually extend as they find it convenient and economical and can afford to make the change. Moreover, it is most desirable that power companies who have been granted, by the wisdom of Parliament, concessions over large areas should not be compelled to confine their operations during their earlier years to the richest corners of their districts, but should be in a position to afford a supply of energy over a wide area from the start. All this means that from the power companies' point of view, from the manufacturers' point of view, and from the public and national point of view, as soon as a power company is in a position to supply electric energy in an area it is desirable that it should have the largest mileage of mains radiating from its station which engineering and finance will permit, and should be in a position to take mains in any new direction required with the greatest facility and economy.

On the Continent and in America, outside considerable towns electric transmission has almost universally been carried out by means of overhead mains—that is, by the most economical and simplest method which could be devised. Unfortunately, until recently in this country officialism and to some extent public sentiment have prevented the employment of this method on any scale, with results which appear to me the more deplorable the more I think of them; but within the last year it is pleasing to be able to record that more enlightened views have slowly begun to permeate official quarters, and a disposition has reluctantly been manifested to meet the necessities of the situation, so that applications for permission to erect overhead mains in suitable localities are likely to be favourably met in Whitehall. It is therefore possible to consider systems of transmission of this character in the United Kingdom, and it becomes of practical importance to see how they compare with underground transmission, and what difference their employment may make in the policy and procedure of power companies.

For this purpose I have had prepared two tables showing approximately the cost of overhead conductors and underground mains employed under similar conditions. The figures must be regarded as approximate, as so much depends in any given case on the amount of work to be done, the locality and other conditions, but still they are near enough for the present purpose. The costs in both cases are for high-class work, and this applies particularly to overhead work, as it is the experience of users both on the Continent and in America that to secure the best results such work must be solidly and carefully constructed and with due consideration of the requirements to be met; in fact it would astonish many electricians in this country to find how much thought and care is being put into the construction of such lines abroad, into the manufacture and design of insulators, the arrangement of the conductors on the poles, the poles themselves and arms and their erection, lightning conductors, cradles, and all the other necessary details which go to make up a properly built overhead transmission line.

OVERHEAD MAINS.—THREE-PHASE CABLES.

On the Basis of One per Cent. Loss per Mile.

Particulars of the Conductors.			Carrying Capacity in Kilowatts at the following Voltages.				Capital Cost of Line complete per Mile, allowing 45 Poles per Mile at the following Voltages.				Capital Cost of Line complete per Kilowatt conveyed per Mile at the following Voltages.			
Size of Cables.	Section of Cables in Square Inches.	Weight per Mile in lbs.	6,000.	8,000.	10,000.	12,000.	6,000.	8,000.	10,000.	12,000.	6,000.	8,000.	10,000.	12,000.
3— 7/18	3 ·01292	3 × 266	81	152	225	323	£211	£230	£245	£260	52/-	30/-	22/-	16/-
3— 7/16	3 ·02299	3 × 468	143	254	400	545	£259	£275	£290	£305	36/3	21/8	14/6	11/3
3— 7/14	3 ·03588	3 × 730	222	394	615	887	£301	£316	£332	£347	27/-	16/-	10/9	7/10
3—19/16	3 ·06247	3 × 1270	382	680	1,060	1,526	£402	£418	£433	£448	21/-	12/4	8/2	5/10

UNDERGROUND MAINS.—THREE-PHASE CABLES.

On the Basis of One per Cent. Loss per Mile.

Particulars of the Conductors.			Carrying Capacity in Kilowatts at the following Voltages.				Cost of Main complete laid and jointed at the following Voltages.				Cost of Line complete per Kilowatt conveyed per Mile at the following Voltages.			
Size of Cables.	Section of Cables in Square Inches.	Weight per Mile in lbs.	6,000.	8,000.	10,000.	12,000.	6,000.	8,000.	10,000.	12,000.	6,000.	8,000.	10,000.	12,000.
3— 7/18	3 ·01292	3 × 266	81	152	225	323	£490	£540	£678	£800	120/-	71/-	60/-	49/6
3— 7/16	3 ·02299	3 × 468	143	254	400	545	£520	£635	£725	£870	73/-	50/-	36/-	32/-
3— 7/14	3 ·03588	3 × 730	222	394	615	887	£545	£660	£772	£950	49/-	34/-	25/-	21/-
3—19/16	3 ·06247	3 × 1270	382	680	1,060	1,526	£760	£858	£957	£1,050	40/-	25/-	18/-	13/9

An inspection of these tables will show that at 6,000 volts between conductors on a 3-phase system the cost of the overhead conductors is about half that for underground cables. If the voltage is increased, the cost of the overhead line only rises slightly while the cost of the cables increases more rapidly, so that at 10,000 volts the cost of underground is nearly three times the cost of overhead transmission. I may say that in the costs for the overhead transmission a fair sum is included for extra poles and networks when crossing roads or places to which the public have access.

In these tables the costs of overhead conductors have been calculated on the basis of a single set of conductors of the sizes mentioned on a pole, but when more power is wanted a second set of conductors can usually be added at the cost for copper, insulators, arms and erection only, whereas to lay a second cable will cost practically as large a sum as the first. Herein lies one of the great advantages of overhead distribution; the current can be taken to a works and a small-sized set of conductors put up to commence with at half the capital outlay required for laying a cable of similar section. If the use of electric energy should afterwards be discontinued for any reason, the poles and wires can be recovered and are still worth a great part of the original value, as they can be used elsewhere. If, as is probable, the use of current is extended, another set of larger conductors can be easily and cheaply erected. With a cable, if an underground main is discontinued it is worth nothing to take up. To lay a full section cable in the first instance means locking up a great deal of capital unremuneratively, possibly for some years; on the other hand, to lay a small cable first and a second larger one later is very expensive. In the first case, viz. with overhead conductors, the capital required will only be half that required in the second, and moreover a good proportion of it will remain in a liquid form. Thus by employing overhead conductors a power company is sinking a much smaller amount of capital in extending its mains widely during its early stages.

Besides the lower cost per mile of the distributing system with overhead mains, which in many cases would make the difference whether a supply can be given profitably or not, it is important to observe also that if it will pay to carry the current a certain distance by underground mains, it will pay to carry the current nearly twice as far with overhead conductors; which means that a power-station can economically supply current over about three times the area which would be possible with underground transmission at 6,000 volts, and if the pressure is increased the relative areas show a still greater difference. For this reason not only has a power-station with overhead transmission a much larger area from which it can pick up customers and thus acquire a load quickly, but it will later on have a larger area to draw customers from, so that the advantages of additional size and concentration are also secured in a greater degree.

It is also important to bear in mind that overhead conductors are, comparatively speaking, free from the complicated phenomena known as resonance effects, which give a good deal of trouble on underground circuits and have been a cause of much anxiety.

Notwithstanding all these obvious advantages, the attitude of the high authorities at the Board of Trade has until recently been so adverse to the use of overhead conductors in this country as to amount to a practical prohibition, and this has led to a timidity amongst engineers in proposing their use and has produced an impression amongst local authorities and the public that they are dangerous, unsightly, and liable to faults. I therefore propose to devote attention to these points for a short time, as in the negotiations I have had with the Board of Trade it was on these grounds that I found most opposition and reluctance to move.

Overhead conductors are not now new. For nearly twenty years high-pressure overhead transmissions for series arc lighting have been used on an immense scale in America and elsewhere, and under conditions which would seem extraordinary to many English engineers. Such circuits, moreover, still continue to be used outside great towns. Again, it is now more than twelve years since multiphase transmissions at high pressures came into use, and they have since multiplied enormously outside this country. In Mr. Bell's book on "Power Transmission" there is a list of power transmission plants in America all at pressures above 10,000 volts, the list apparently being carried up to the year 1901. The horse-powers of these various transmissions aggregate 170,000 H.P., and as plants employing over 10,000 volts pressure must be in the minority, it is a fair deduction to make from this and other sources of information that at the present date there must be in America over 500,000 H.P. transmitted electrically by overhead conductors. Notwithstanding this we come across no long lists of accidents, we do not hear of any legislative prohibitions of this method of transmitting energy, there are no signs that overhead conductors are being given up; on the contrary every week we hear of fresh projects, including such undertakings as the long-transmission lines in California and the running electrically of the whole of the railways in Switzerland, Norway and Sweden, the power for which would in most cases be almost entirely conveyed overhead.

I have also made inquiries amongst several of the leading firms on the Continent as to what accidents have come to their notice, and the invariable reply has been that so far as the general public was concerned they could not recall any, though they could usually mention one or two cases of accidents to employés, almost invariably, however, from transgressing simple rules as to not working on circuits with the pressure on.

A little thought will show that with a properly constructed route of overhead mains, with stout poles 40 to 50 yards apart, the chances of the mains falling are infinitesimal, far less than in the case of telegraph wires, which are much smaller in diameter and are run habitually in spans double the length and are strained up much more tightly than in the case of tramway conductors.

The most legitimate objection which can be taken to overhead transmissions is from the æsthetic point of view, but against this should be placed to their credit the fact that they would be the means of abolishing much smoke and dirt and heavy coal traffic on the roadways;

they would render brighter, cleaner, and more cheerful numberless homes; and by bringing a convenience of town life into more open districts would be a powerful aid in the important object of repopulating the country which is at present so much to the fore.

One of the most desirable objects at the present time is the extension of tramways into the country and the construction of inter-urban tramways, in which so much has been done in the United States. Now in that country the whole of the feeder work, an expensive item in tramway construction, is carried out by means of overhead mains.

Abroad, overhead power-transmission mains freely traverse much of the finest scenery, and yet tourists and others do not find them overpoweringly objectionable. Overhead power mains are not more unsightly than heavy telephone or telegraph transmissions which we already have with us on a large scale, and as they would more often traverse the open country they would be still less in evidence. A great deal can also be done to remove their obtrusiveness by simply painting the creosoted poles green or grey. Abroad most of the poles are not creosoted, but when preserved at all are treated by another process. It is the deep dead black of the creosoted pole which attracts the eye and makes the ordinary telegraph pole an obtrusive object in a landscape. The adoption of any means by which the tone of the pole was brought as nearly as possible to that of its surroundings would make an immense difference in this respect.

We now come to the third objection to overhead transmissions, namely, that they are liable to faults. To meet this the best way is to appeal to facts. Overhead transmissions have been used on a large scale for many years in other countries, as alluded to above. Are such transmissions being given up? Do we hear that industries supplied with power in this way find the interruptions a serious source of loss? I can find no evidence to this effect, particularly where the mains are well erected, and it must be remembered that a great deal of overhead transmission is over very rough ground in hilly and mountainous districts where the chances of accident are the greatest. The fact that towns like Rome, Berne, Milan, and Buffalo, to name only some which come into one's head at once, are and have been for several years dependent on electric energy supplied over distances of from 15 to 30 miles by overhead mains is clear proof of their reliability when well erected and looked after. It is a point in favour of overhead transmissions that any fault which may occur can be so easily located and quickly remedied. The risks of wilful damage are much less than is supposed. There is a wholesome dread amongst people generally of electric shocks, and to do effective damage without personal risk requires an elaboration of means and an amount of knowledge which is possessed by few and can only be used with the most deliberate criminal intent. Any tendency in this direction is further restrained in several countries on the Continent by special provisions in the penal code.

The real danger of interference with overhead transmission is from lightning, which the pioneers of this class of work sometimes found exceedingly troublesome; but the question of lightning protection has

received much study, and overhead transmissions are freely used now in countries where thunderstorms are so much more frequent than in this country that I do not think we need have any fear under this head in the United Kingdom when proper precautions are taken. Besides this it must be recollected that practice and experience in those countries where overhead transmissions are used are continually producing improvements and developments, particularly in lightning protection and in the character and forms of insulators, and this progress will continue. There is no more question of finality here than in any other branch of engineering.

Having now dealt with the advantages of overhead electric power transmission and having met the chief objections which have been made to its use, we are now brought to the consideration of what is the practical position supposing transmission of this character is proposed in this country.

I have already mentioned that the Board of Trade are now prepared to consider favourably proposals of this character.

As regards passing near buildings and the class of district through which overhead wires may be taken. The Board of Trade have passed a series of routes in a case in which the writer is acting as engineer which go as far in these respects as I think engineers can reasonably ask for at the present stage. The Board, however, still appear to have objections to high-pressure transmissions being erected along the sides of roads. Consequently, as things stand at present, way-leaves will usually have to be obtained for carrying the mains across the neighbouring fields.

In choosing a line of route, particularly in parts of the country where there are hedges and trees, it is very desirable that as far as possible it should be clear of these, should be accessible, should not have too many angles, and should be within sight of roads or good footpaths in order that the line may be inspected easily. An inspector should patrol all the main routes at any rate once a day. If a line is in sight from a good road it is obvious that this inspection is easily and quickly made, and should a fault occur it can be traced by an observer from a gig, motor car, or bicycle provided with repairing plant, and is easily remedied.

In many districts there will not be much difficulty in obtaining way-leaves, but it is in this matter exasperating to be at the mercy of an occasional cantankerous landlord or set of trustees, and it would be a great facility if there were in this country a set of laws and regulations such as have been made in several foreign countries giving undertakers the power to obtain such way-leaves by a form of compulsory arbitration when it can be shown that the project is of public utility. It is, I understand, only occasionally that this power has to be put in force; the fact that this right of appeal exists in case of necessity is as a rule sufficient to enable an amicable arrangement to be made privately.

In this country such powers can at present only be obtained by a specific Act of Parliament, which is far too cumbrous and expensive a method of procedure and is moreover impracticable, since it is only as a project develops that the routes of a great part of the mains can be definitely settled.

To show the arrangements and formalities necessary to obtain such rights, a translation of the Swiss law relating to electric transmission is appended to this paper. I have to thank Mr. E. G. Cruise for procuring it for me and kindly making the translation together with some extracts of the General Law on Expropriation which bear on the subject. It will be seen that an undertaker desiring to obtain way-leaves and finding difficulty in doing so by private arrangement can at any time appeal to the Communal authorities, who must, after deposit of the plans, at once appoint a tribunal consisting of three members, which acts immediately and must give its decision without undue delay. The procedure is thus prompt and inexpensive. I also append a translation of the Italian Law and Rules on the same subject to which I would direct special attention, as the whole is so clearly and comprehensively drawn up. For this I have to thank Mr. C. A. Baker.

I am aware that so far no such rights of expropriation have been granted in this country even to the Post Office, but I would ask whether the importance of electric communication is not now so great to the community as to warrant such an innovation, which would, by assisting in opening up the country, ultimately, I feel confident, prove to be in the interests of the landowners themselves.

Coming now to the more technical aspects of the question. When once the primary objections of the Board of Trade to granting permission for overhead distribution were overcome, I found a disposition on the part of the Board's electrical adviser to treat the question of regulations and rules in a broad-minded and fair spirit, and a disinclination to make specific rules at present, so long as the work is properly done in accordance with the best practice abroad, and until further experience has been gathered. This policy seems a very wise one, and I trust it will be met in a fair spirit by any engineers who may be contemplating overhead transmission. Now that a beginning has been made, it is most desirable that the use of overhead mains should go forward on right lines, and that work of this character should not be further hampered or delayed by unnecessary restrictions or misunderstandings.

So far, I have dealt chiefly with the branch of the overhead distribution problem, which for the moment concerns us most directly, but there is another aspect of it of nearly equal importance to which I will now allude—I mean the distribution of electrical energy at voltages suitable for lighting, running motors, heating, and other direct uses. As is well known, overhead distribution for these purposes is and always has been widely practised both on the Continent and in America, nevertheless hitherto in this country, through official opposition, prejudice and other similar causes, almost nothing has been done of a similar character. To give an idea of what has been done in this direction, the best way will be to quote from a report which the United States Government Department of Commerce and Labour has recently published, in which a number of data and statistics relating to work of this character are given, and extracts from which have been recently published by the *Electrical Review*.

There are, it appears, in the United States 1,892 towns or villages

having a population of more than 2,400; of these, 1,511 have electric supply stations, and out of these 1,511 stations, 1,100 are in towns or villages having between 2,500 and 5,000 inhabitants. Now, how many such towns or villages in this country have electric supply? Not more than a dozen or two, although in the United Kingdom there are nearly 1,000 similar places. When we come to look into the reasons for this, we find that in the United States, out of a total of 125,000 miles of feeding and distributing networks, 117,000 or 93 per cent. of the whole are overhead, there being in all the United States only 8,124 miles of underground circuits.

There is no reason why, with overhead distribution and a cheap system of wiring, such as is in use everywhere except in this country, all these smaller centres of population and the outskirts of the larger towns should not enjoy the benefits of electric light and power supply, which they are hardly likely to do for another twenty years without this system of distribution.

It is, further, worth pointing out that most of the local distribution abroad is at 110 volts. With 240 volts, such as we use largely here, the problems involved are much simpler, greater areas can be covered, and the sections of copper required lend themselves very well to being carried overhead. I myself carried out the lighting of a town in the Colonies some years since in this way, and was quite surprised, when I came to work out the mains, to note with what facility effective distributing arrangements could be made, and what distances it was possible to go by a little scheming without awkward drops in voltage, and without many regulating appliances. The advantages which would be secured by the smaller towns in this country, where gas is usually expensive, and, in most cases, of inferior quality, by obtaining a system of electric supply would be very great, and the general adoption of electric lighting in the smaller class of houses would, I am sure, be an important factor in improving the general health of the population, one of the most deleterious factors in modern life being, in my opinion, the spending of evenings in small rooms lit by gas.

It ought to be the function of power companies to feed these towns and villages by lines of overhead mains radiating 12 to 15 miles in all directions round each power-station, and the undertakings being in groups, economic and effective administration would be secured, and the disadvantages and lack of efficiency of separate small undertakings would be avoided. To do such work effectively, however, and in the best, safest, and neatest manner, some powers of obtaining way-leaves such as I have alluded to above would be a great advantage.

That there is a large field open for future work in this direction, no one who has studied the question will, I think, deny, and it is very much allied to the question of inter-urban electric railways, in which so much progress has already been made in the United States. The feeders for these railways are invariably overhead, and the same line of poles, if not the same circuits, should serve for the lighting and power supply of the districts through which an inter-urban railway passes.

Before closing, I should like to say a few words on another class of overhead electric distribution which is fast attaining an important

place, namely, distribution at very high pressures such as 50,000 volts. The continued success and extension of such installations warrants our now considering them not only as local enterprises specially arranged to meet some unusual set of conditions, but as being of general application. When electric distribution is effected at these voltages, not only do we get into a new order of phenomena electrically, but economically also. The capital cost of the line comes down to from 1s. 6d. to 2s. per kilowatt conveyed per mile, with a loss of, say, $\frac{1}{5}$th of one per cent. per mile. From what I have seen in the Colonies I feel sure that such long-distance transmission lines will be of great value there and in India, and will open up many possibilities. The opportunities for advantageously employing them will probably not be many in this country, except perhaps in connection with railway work, but it is very desirable that every facility should be given for their erection wherever they would be clearly advantageous, for the sake of the practical example they would offer and the experience which would be gained in operating them on the spot.

The carrying out of work of the character I have been dealing with in this paper would not only be a direct benefit to the electrical industry, and to those who had placed at their doors an important facility of modern life, but it would have a great indirect effect on the electrical and allied industries of the country in the following way.

This method of electric transmission is being used in our Colonies and by the other nations we trade with, and will be to a still greater degree in the future. Being well acquainted with the conditions in some of our Colonies myself, I feel assured from personal observation, that overhead electric transmission of power and light will be one of the most potent factors in developing new industrial and mining regions in the next twenty or thirty years. If work of the same character is carried out on a considerable scale in this country, our engineers and manufacturers will have examples of it at their doors, will be able to take part in its development and improvement, and will be able to carry out such work not only here, but in other countries also. If, on the other hand, there should be no such work here, it is, in my opinion, idle to expect that manufacturers in this country will be able to develop serious industries in this direction in competition with rivals who have opportunities of keeping closely in touch with the work, and can see more clearly what is needed, and learn from direct observation and inspection what is good and what is faulty, and the best manner in which improvement can be made.

I have now touched on the salient points which are raised by the title of my paper. It is not easy to present them in an interesting and concrete form, but I shall be well satisfied if I have succeeded in doing anything which will contribute towards putting this matter on a better basis in the United Kingdom.

I have not attempted in this paper to give any detailed description of systems of overhead distribution, because for some time at any rate we shall only be able to follow what has been done successfully in other countries, of which frequent descriptions have appeared in the technical press, my object being to bring the subject forward in its general lines

with the view of promoting discussion and eliciting the opinions of other members. I, however, give in an appendix a list of references to descriptions of overhead distribution and works on the subject which may be useful. I wish to draw special attention to a pamphlet on the subject recently issued by the American Institute of Electrical Engineers, a copy of which has been kindly sent me by Mr. C. O. Mailloux, of New York. Some time since, the Committee of the Institute appointed a Commission to inquire into the practice with regard to high-pressure transmission. This Commission formulated a large number of queries which were forwarded to the engineers in charge of such plants. Replies had been received to date from 46 installations, and the pamphlet consists of an epitome of these replies, which cover the whole field of high-pressure overhead transmission, and give the views and recommendations of experienced men on almost every point, including poles, arms, spans, conductors, insulators, leading in, effect on other conductors, lightning arresters and numerous other practical points. No one who proposes to attempt such work in this country should be without a copy of this pamphlet, which I presume will be public property before long.

APPENDIX.

Italian Law for the Transmission to a Distance of Electric Currents.

(*June* 7, 1894.)

Art. 1. Every proprietor is required to permit the crossing of his lands by electrical conductors, either overhead or underground, which may be required to cross the same, having permanent or only temporary right to be used for industrial purposes.

The faces towards the streets or public squares of houses, courts, gardens, orchards, and the store-houses adjacent to houses are exempt from this.

Art. 2. Whoever demands such right of way must do all things necessary in making use of it and must eliminate every danger through carelessness of workmen. He may also be required to make use of work already put in hand by the proprietor and adapted to be used, compensating the proprietor for the expense that he has incurred, and contributing proportionally to the cost of maintenance.

Art. 3. Way-leave must also be granted for the conductors to cross canals or aqueducts or other works employed for such use, provided that no impediment or diminished utility is caused to the proprietor.

Art. 4. Where it is necessary, in the route of the electrical conductors, to cross public streets or rivers or streams or to touch the exterior of houses facing the streets or public squares, the special laws and regulations concerning streets and water and the directions of the competent authorities must be observed.

Art 5. Applicants for way-leaves for electrical conductors must prove their ability to erect the same and demonstrate the value and the use of the conductors for industrial purposes; they ought, besides, to demonstrate that the way-leave requested and the manner of making

use of the same is the most convenient and the least prejudicial to the lands used, having regard to the circumstances of the surrounding lands and to other conditions of the route and the place to which the electrical energy is to be conducted.

ART. 6. Before undertaking the construction of the conductors, the applicant for way-leave ought to correspond with the proprietor of the lands to be crossed, proposing an indemnity to compensate him for the diminished value of the land which will be caused by the actual work to be done and by the subsequent operation of the same. In estimating compensation the land is to be considered as it exists and without deduction for the crops it bears and with super-valuation of one-fifth. The undertaker ought also to compensate the proprietor for immediate and subsequent damage owing to the intersection of the land or other deteriorating effects, as well as for the privilege of entering upon the land for the purpose of operating and for the maintenance of the electrical conductors.

ART. 7. Where way-leave for the conductors is requested for a period of not more than nine years, the agreed indemnity for the value of the land will be reduced by one half, but when this period expires, the land must be restored to its original condition at the expense of the undertaker who obtained the way-leave for the conductors. Undertakers obtaining temporary way-leave can before the expiration of the term make it permanent by paying the remaining half with the proper interest from the day when the way-leave was granted. When the first term has expired no account will be taken of what may have been paid for the temporary way-leave.

ART 8. The undertaker must, in every case, observe that the conductors are, and must be, arranged with due regard to the legal and special regulations in regard to the matter and the special directions that are, and may be, established to regulate the working of telegraphic and telephonic communication.

ART 9. Disputes which may arise in the application of the present law will be treated according to the usual custom, either before the magistrates or in the courts. All the proprietors of lands on which it is desired to place conductors can be convened before one judicial authority; in this case the Magistrate of the place in which the land is situated, subject to the largest contribution towards the State, will be the competent authority.

Rules for putting into Effect the above Law of June 7, 1894.

ART. 1. Electrical conductors for industrial use, by the law of June 7, 1894, No. 232, are all those destined to conduct energy to a distance by means of electric currents, except telegraph and telephone conductors, which are regulated by the law of April 7, 1892, No. 184.

ART. 2. When for the preliminary study of a projected installation of electrical conductors it is necessary to enter upon any lands and it is not possible to obtain the consent of the proprietor, the person wishing to establish the conductors can obtain from the Royal Magistracy of the Province in which are situated the lands to be entered upon an authorisation of access. He should be able to prove to the said

authority the justification of the request for permission and present a demand in which are indicated:

(*a*) The period of time during which he intends to study the matter.

(*b*) The lands to be entered.

(*c*) The necessary details to furnish an exact idea of the importance of the installation.

ART. 3. The Magistrate, having ascertained the justification for the permission, will authorise the applicant to enter upon the lands for the purpose of studying the project, by suitable decree. In this decree the names should be indicated of the persons to whom it applies and the length of time that it is to remain in force.

ART 4. Persons who have occasion to avail themselves of such authorisation must make use of it in the manner least prejudicial to the proprietor of the lands, and will be called upon to restore any damage to the property. In regard to inhabited dwellings, the Mayor will fix, at the suggestion of the parties, a time and method when the permission can be exercised. When it is necessary to enter within the enclosures of a public railway it will be necessary to obtain also the permission of the Administration responsible for working the line.

To assure the payment of the indemnity, the Magistrate can demand the deposit of a suitable sum of money from the applicant.

ART. 5. The conductors of an electrical installation, which cross public streets, railways, rivers, streams, canals, telegraphic or telephonic lines for public service, or which come near to these lines, or if they pass them, or if they are near a public monument, must not be constructed without the previous consent of the competent authority unless the work has already been declared of public utility. Additional conductors can be placed without the said consent, but ought to be notified to the same authority at least ten days before putting the work in hand. Subsequent enlargements or alterations of the conductors can be made without notification, except the case that may arise under Art. 13.

ART. 6. The demand for a consent or the notification in regard to an installation is to be addressed to the Royal Magistracy of the Province, in which the installation is to be located. When the conductors cross public works or property comprised in the areas of two or more Provinces, the demand for the consent or the notification is to be addressed to the Minister of Agriculture.

ART. 7. When for the working of an installation, the consent of the competent authority is wanted in the terms of Art. 5, the undertaker, besides the proof of his right to ask for a compulsory way-leave, must produce to the same authority:

(*a*) A complete drawing of the installation with particulars of the conductors and of their supports, with an indication of the crossings of the public streets, railways, rivers, streams, and canals, and with a distinction, the telegraph and telephone lines near to the projected conductors.

(*b*) A description of the installation, indicating its character, whether alternating or direct current, the maximum effective values of the differences of potential and the strength of current in the conductors, the nature and section of the same, and the system of insulation.

The applicant must also give a definite address and a period within which he undertakes to have the installation in working order.

ART. 8. The authority indicated in Art. 6, or, where necessary, the public administration interested, in the project presented by the applicant in the sense of Art. 7, gives its consent that the installation may be set to work by the conditions of the present rules, under the responsibility of the undertaker for any damages that may result from the system adopted and with express reserve as to the opposition of interested parties in the sense of Arts. 5 and 6 of the law.

ART. 9. The notification to be made by the terms of Art. 5 must be accompanied by a general description of the installation.

ART. 10. The undertaker will be required to operate the installation and the electric conductors under his own responsibility, using all possible means of safety and safeguards from danger and such apparatus as is found advisable by science and practice, observing, besides, the following general conditions :

(1) For aerial conductors, he must adopt all the known precautions to prevent breakage of the conductors and the danger arising therefrom, having regard to their weight and potential.

(2) Conductors between which a difference of potential exists must be placed in such a way that one cannot, by falling or by elongation, come in contact with another ; in the cases where this condition cannot be conveniently fulfilled, some special arrangement ought to be adopted either in the supports or in the contrivance, to assure the suspension of the conductors or to render breakage as little dangerous as possible to persons and things, independently of the insulation of the conductor itself.

(3) Conductors supported externally to the houses must be so arranged that no persons, other than those connected with the service, can touch them, either near the roofs or the supports. In the open places they must not be supported at a less height than six metres from the ground, in special cases greater heights may be necessary. The competent authority may concede placing of the conductors at a lower level than that above indicated, only in the case of electric tramways, and in such other cases in which it is possible to show not only the absence of danger, but also the practical necessity to place them at a lower level, for the use to which they are destined. On the *façade* of the houses, the conductors must be away from the possible reach of the hand of a man standing at a window, or on the sill of it, or on a balcony, or on a terrace, or on the roof.

(4) In wells and in shafts of mines and in similar excavations, in the tunnels of railways, tramways, or ordinary roads, in subterranean passages or cuttings and everywhere where the local conditions prevent putting into effect normal rules, the conductors must be rendered harmless from contact in the most efficient manner that is possible.

(5) For conductors in contact with the earth the terms of Art. 11 are to be observed.

(6) The poles, supports and other bearers for the conductors on which there is a dangerous potential, must be provided with suitable defences to prevent any one approaching the conductors without the aid of a portable ladder or some similar means.

(7) All the supports ought to be arranged in the manner least prejudicial to the proprietor of the property, and such as present the necessary resistance in themselves and in their points of attachment.

(8) Suitable means of protection must be adopted where there is any danger of contact between the conductors of energy and the telegraph or telephone lines, in case of the breakage of either.

(9) Special precautions must be taken in localities where the conductors might be touched by the agents in the service of the telegraph or telephone departments.

(10) In the arrangement of underground conductors, possible contact causing accumulations of gas must be avoided, and likewise damage to other conduits of gas, water or similar things.

ART. 11. In the electrical conductors, as indicated in Art. 1, it is permissible to make connection with the earth at one point only of the circuit, but the circuit must always be entirely metallic, and, in the point of contact with the ground, have perfect joints and the section not less than would be required in a well-proportioned installation in which there was no connection with the earth. Such precautions must be observed as science and practice may be able to suggest to avoid possible damage owing to the earth connection.

ART. 12. In regard to the works of public interest and to rivers, streams, and canals, besides the conditions of Art. 10 and those in force by the ordinary laws, the following rules must also be observed, as also any special rules which, during the construction of the conductors or in operating them, may be determined, case by case, by the competent authority, that is to say the Administration interested:

(a) *For railways and for tramways on their own grounds.*

(1) Where possible, the conductors of electrical energy should not be laid along the railways, nor on the lands of the railway authorities, nor across the internal courtyards of the stations.

(2) The electrical conductors ought to cross the railways at a right angle, and at a height of not less than seven metres from the level of the rails, modifications being allowed in the case of aerial conductors for electric tramways.

(3) The supports of the electrical conductors ought to be arranged at such a distance from the lines that, falling, they cannot encumber the railway, and where this condition cannot be observed, they ought to be secured in such a manner as to prevent them falling on the lines.

(4) The trenches for underground cables must be not less than one metre in depth, measured from the top to the bottom of the trench, and must be well formed as required by the conditions affecting the safety of the railway. Such trenches ought to be constructed so that any repairs can be effected without touching the surface of the railways.

(5) For crossing underneath the railways with electrical conductors, where possible, separate trenches from those used for other conduits—water, gas, etc.—should be employed.

(6) The undertakers for the electrical conductors may be compelled, when crossing a railway, to make use of any passages already con-

structed under the railways, and that may be suitable for the purpose, conductors for electric tramways being excepted; this will be done under the supervision of the railway administration, and subject to future interests.

(b) *For the public roads away from dwellings, for rivers, streams, and canals.*

(1) Along the public roads, the banks of rivers, streams and canals, on which there exists, or will have to be placed, telegraphic or telephonic lines for public service, the installation of electrical conductors is absolutely prohibited. This can be modified by previous agreement with the Minister of Posts and Telegraphs, with the object of adopting the best arrangement for both undertakings, in a manner the least objectionable to the undertaker of the electrical conductors. For crossings, the conditions of (a), 2nd par., shall apply, with such modifications as the circumstances warrant.

(2) The conditions which are promulgated from time to time by the competent authority must be observed in regard to the interpretation of the laws and regulations concerning streets and water.

(c) *For public streets and squares.*

Respecting the way-leaves for the streets and public squares, and the attachments to the *façades* of houses, the conditions prescribed by the municipality or other competent authority must be observed.

ART. 13. The installation of electrical conductors for which compulsory way-leave is obtained, must be constructed in the manner least prejudicial to the proprietor of the lands traversed, and also in regard to any other purpose to which the land may be put. When along the route of the conductors there exists another electrical conductor or a telegraph or telephone line—unless a judicial decision has been made known—the reasonable necessities for the smooth working of both undertakings ought to be agreed, or the views accepted of the party having the best title to claim pre-eminence in public service, or if such titles be equal, then by reason of priority of existence. When the decision arrived at necessitates the removal or the modification of the conductors, the expense is to be borne by the party making the alteration necessary.

ART. 14. The proprietor of the land must not do anything to diminish the value of the way-leave or to render it more difficult, nor transfer the right of way to any other position than that originally fixed. The same obligation applies to the undertaker. If, however, the original arrangement has become more serious to the proprietor of the land, or if it hinders him in doing certain works, repairs or improvements, he can ask the undertaker to modify his arrangement of conductors, offering him a place equally convenient for his operations. This the undertaker must not refuse. Where the undertaker finds that a similar alteration would benefit him and no damage to the land would result, it is to be permitted.

ART. 15. The way-leave for the conductors does not convey to the undertaker the soil on either side under or over the conductors, or the

relative supports, or the wall to which they may be hung. The Government taxes and other charges on the land still remain against the proprietor of the land.

ART. 16. The way-leave includes the installation and the use of the maximum number of conductors, and of the maximum section of the same that the undertaker has notified to the proprietor of the land when he made known his right of way, according to Art. 5 of the law of June 7, 1894, No. 232. It is thus within the right of the undertaker to follow the planned arrangements for the supports, and to place the conductors notified without asking further consents, and without obligation to pay further indemnities except always for the reinstatement to be done owing to damage caused whilst placing the conductors.

ART. 17. The fact that conductors are arranged in a certain way does not entitle the undertaker to exemption from the new regulations; such regulations may be modified to meet the conditions of the existing works on the representation of the undertaker.

ART. 18. In the cases foreseen by Art. 9 of the law of June 7, 1894, No. 232, it is the duty of the competent authority to permit the temporary execution of work in the manner and under the conditions that are deemed necessary to meet the public requirements and the rights of the proprietors of the lands.

ART. 19. The control of carrying into effect the law of June 7, 1894, No. 232, and of the present rules, in all cases the responsibility of the undertaker, concerns the Minister of Agriculture, who will look after it, where necessary, in accord with the other Administrations publicly interested. To the same Minister the magistrates will give immediate information of the consents granted for the installation of electrical conductors, and of the notifications received according to Arts. 8 and 9 of the present rules.

ART. 20. In regard to electrical conductors already existing, the proprietors who intend to claim rights against undertakers will not be able to enforce modifications of the arrangements of the conductors until the matter has received proper judicial consideration.

EXPROPRIATION IN SWITZERLAND IN THE CASE OF ELECTRICAL WORKS. (See pages 15 to 17 in Swiss Document.)

Swiss Federal Law. June 24, 1902.

The following clauses extracted and briefly translated from the General Law of above date relating to electrical undertakings, set forth the special provisions made for Expropriation.

Art. 43. The Federal Council has the right to grant powers of expropriation to the owners of electrical undertakings, as also to the consumers.

Art. 44. Branches of trees endangering the transmission lines are to be removed on demand of the undertakers, and in case of dispute as to the necessity for removal, indemnity, etc., the Local Authority designated by the Government of the Canton will give a final decision within eight days. The costs of such proceedings to be to the charge of the undertakers.

Art. 45. Electrical transmission and distribution installations are defined as (1) The electrical conductors, whether aerial or underground and their accessories, and (2) Transforming stations and their accessories.

Art. 46. The right of expropriation can be exercised for private property and also for railways, but in the latter case the electrical works must not interfere with the working of the railway, and ample space must be allowed for the operation of the railway and its telegraph lines, etc. The public land and highways of the Cantons and Communes can be utilised, and rights of expropriation granted over same for the transmission and distribution of electrical energy. When such employ of the public land of a commune for distribution of electrical energy is demanded, the commune can, to protect its own legitimate interests, refuse or restrict the demand, save in the case where the energy is required for the electrical working of a railway. The government of the Canton can be appealed to within twenty days of such refusal or restriction, and its judgment can be further appealed against before the Federal Council within a further twenty days. The decision of the Federal Council will be final. Electrical undertakings cannot claim the use of the public land without also respecting the other uses to which the land may be applied.

Art. 47. Expropriation rights can be demanded by the undertakers of electrical installations or by consumers both for the absolute acquisition of the property or for its conditional possession whether permanently or temporarily.

Art. 48. The indemnity is to consist, according to the circum stances, of a capital sum or an annual charge. With the consent of the two parties, the indemnity can be made to include repairs for damage to crops or other damages similarly caused by alterations to the electrical constructions. If no arrangement in this respect has been made between the parties, the demands for indemnities which may be made during the working of the undertaking can, in case of dispute, be settled by the ordinary procedure.

Art. 49. Save the exceptions mentioned in Articles 50 and 54 herewith, expropriation will take place in accordance with the provisions of the Law of May 1, 1850.

Art. 50. To obtain powers of expropriation, the undertakers must address their demand to the Government Survey Department of Electrical Works, submitting a plan of the works and of the land required. The Federal Council will then grant expropriation rights if, within a period of thirty days from the deposit of the plans, there is no opposition. In case of opposition, rights will not be granted if the projected works can be modified without grave technical difficulties involving too great cost, or without danger to public safety. Where alterations appear necessary, a new application for expropriation rights can be proceeded with on the demand of the undertakers of the works, or of the party to be expropriated.

Art. 51. As well as depositing the plans with the Survey Department mentioned, plans are to be deposited in the communes affected, so that every one concerned can note same. The deposit of the

plans and demand for expropriation rights shall be made public, and in addition personal notification shall be sent to every one concerned. If expropriation is demanded for particular property owners only, the provisions of Articles 18 and following of the Expropriation Law of 1850 will apply.

Art. 52. As soon as the Federal Council shall have given their decision and approved the plans, the Estimating Committee (Article 54) will, if required by either party, meet and discuss the indemnity to be paid.

Art. 53. After approval of the plans, the works can be proceeded with, even though the Estimating Committee may not have finished their inquiry, or though the indemnity be not yet paid. Guarantees for such payment shall, however, be forthcoming, and in case of dispute the Estimating Committee will fix the amount of the guarantees.

Art. 54. Each Canton shall supply an Estimating Committee of three members, the Federal Court, the Federal Council, and the Government of the Canton each nominating one member and two deputies for each member.

The decisions of the Estimating Committee can be appealed against before the Federal Court in accordance with the provisions of the Expropriation Law of May 1, 1850.

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Discussion at Meeting of February 23, 1905.

(*Mr. W. H. Patchell, Vice-President, in the Chair.*)

Mr. Gavey.

Mr. J. Gavey: I think the time is very appropriate for the consideration of the subject dealt with by Mr. Addenbrooke, and I trust the result of the paper will be that local authorities, who so strongly oppose the erection of open wires throughout the United Kingdom, may be induced to take a more liberal view of the requirements of the electrical industry. Before dealing specially with the way-leave question, I should like to ask the author one or two questions in relation to some of his statements. In reference to the cost of overhead wires, I observe that his charges appear to be fairly reasonable, but there are one or two points on which I am not quite clear. One of them is the varying cost of the erection of wires of the same type to provide for varying voltages. It is possible that his answer may be the answer to one or two other questions which I propose to suggest. It may be that this extra cost is due to the additional precautions which have to be taken in carrying these high-pressure wires across roads where naturally some special steps will have to be taken to protect the public. In a thickly populated country like the United Kingdom, roads are very numerous, and some measure of protection will have to be afforded in crossing those roads, either by taking the wires underground, by enclosing them in cages, or enclosing them in lattice girders where the pressure is abnormally high. I should like also to ask whether the cost he quotes includes the cost of way-leaves, which although, as a rule, they are paid for annually, might be represented as capitalised in estimating the cost. Again there is the question of possible diversions. If you erect long lengths of overhead wires across private property, you may unfortunately now and then have a dispute with a landowner, and you have to remove your wires. This is an item which comes under the head of maintenance, but it must be provided for as it represents interest on capital. One other point that has occurred to me is this. It appears as though provision were only made for running three wires on each main line of poles. Would those three wires suffice to maintain a continuous and uninterrupted service day and night, year in and year out? With open wires, however well erected, it is necessary to provide for the removal of faults. Members of the Institution who joined in the Italian visit will probably remember the great installation of the Edison Company at Milan, where they transmit, I think, about 13,000 H.P. a distance of some 20 miles. This transmission is by means of overhead wires. Eighteen wires are provided in three groups of three—one series of nine wires being on one side of the poles, and the second series on the other. I was told by our friend Mr. Semenza, that normally the

energy was transmitted over the whole of the eighteen wires, but to effect repairs and carry out alterations the energy was switched over to nine of the wires on one side of the poles, the other nine being left absolutely free and available for repairs. Repairs, of course, must always be necessary. Contingencies will arise which cause faults, the breakage of insulators by stone-throwing, the piercing of insulators by the high pressure, pieces of iron and copper thrown across the wires, and other difficulties. Provision must be made for effecting these repairs whenever the faults arise. Mr. Gavey

With regard to the underground work, I see it is stated that to increase the number of conductors, it is necessary to repeat the original expenditure. I take it that in carrying out underground work, along main lines at all events, spare ducts would be laid at a comparatively small additional cost, into which additional conductors could be drawn from time to time as the need arose. On the question of erecting overhead wires along highways as compared with their erection on private property, I think there are two or three points that are worthy of consideration. Most of the highways in England—the most important of them, at all events—carry telegraph and telephone wires. Three-phase transmission on open wires causes a good deal of inductive disturbance, both electrostatic and electromagnetic. When we were in Italy, I found that on the Valtellina Railway, the disturbance to the telegraph wires was so great that the authorities had had to double the wires on the railway, in order to overcome the disturbance; and so as to avoid doubling them throughout the whole length they had established repeaters at each end of the line, at, of course, some considerable cost. I admit that the conductors which carried the energy, both the high-pressure conductors of some 20,000 volts and the feeding conductors of some 3,000 volts, were erected so unsymmetrically that no one could have expected anything else but extreme disturbance. On the line from Milan to Lake Maggiore, however, the company had erected the transmission conductors far more symmetrically. They were equally spaced throughout, and moreover they had taken the precaution of revolving the conductors in the same manner as telephone conductors are revolved, so that the disturbance on the neighbouring telegraph and telephone wires was reduced to a minimum. But I am afraid if overhead conductors were carried along our main roads in the immediate neighbourhood of telegraph and telephone wires, considerable disturbance would arise. On the other hand, by encouraging private way-leaves certain advantages will be obtained. The lines will be freer from accidental disturbances, due to interferences by mischievous people, such as I have already referred to. A route relatively free from trees can be selected, so that interruptions due to falling boughs or falling trees may be avoided. There is just one other point which perhaps may not generally be known. In carrying wires across cultivated fields in smoky neighbourhoods, it is well to avoid localities in which luxurious vegetation exists, because the smoky acids acting on the copper generate salts, which are rather destructive to crops.

On the general question of way-leave difficulties, I may say that this has been with me for forty years. Some thirty-five years ago,

Mr. Gavey. when the State acquired the telegraphs, every man objected to a pole in his neighbourhood. After a time this objection quieted down; but I am sorry to say that within the last few years there has been an extraordinary recrudescence of obstruction to the erection of open wires. Local authorities, private individuals, and all those who think they are interested in the matter, have assumed that in all cases wires can be placed, and should be placed, underground. They pay no regard whatever to the cost involved. If you tell them that under certain conditions it is impossible to lay wires underground—for instance, in the case of long trunk telephone wires—they quietly assume that you are not telling the truth, and they oppose you without the slightest consideration, either to the question of cost or to questions of efficiency. I only hope that our friend Mr. Addenbrooke will succeed in removing some of the objections that are raised by local authorities to the erection of overhead wires, and I am sure most of those who are interested in the progress of electrical work will join me in that wish.

Mr. Portheim. Mr. R. S. PORTHEIM: The paper is particularly interesting to me because my firm is intimately associated with the North Wales Electric Power scheme, which depends in its very essence upon the use of overhead wires. With underground cables that scheme would be, commercially, absolutely impossible. This is the first instance in this country in which a power company has been started on a scale of any magnitude with free power, as far as the Act is concerned, as to the use of overhead wires, provisional only on the Board of Trade consent. The energy will be transmitted by overhead wires at 10,000 volts along public roads, and the total power to be transmitted from the water-power station at Snowdon will be 10,000 H.P. Some 60 miles of mains are now being completed. The total cost of the overhead wires in the North Wales scheme, having three or four feeders to a radius of 40 miles from the power station, is under £40,000, and the same cost in underground cables would have been nearer £150,000. With regard to the erection of these wires, we have found the Board of Trade exceedingly courteous and very reasonable indeed, considering it is a pioneer installation. A railway is also being constructed by the North Wales Power Company, the 10,000-volt feeders for which will run parallel to the trolley, and the only stipulation made by the Board of Trade is that the feeders for the railways shall be two pole-lengths' distance from the trolley. The Board is of opinion that unless this is done, if a high-tension feeder breaks, the lives of the passengers in the train will be endangered. On the Valtellina railway, in spite of the fact that it has run for over two years and there are 20,000 volts on the same poles as the trolley wire, there has not been one single accident of any kind. In the North Wales scheme it has been found better to give a few pounds for private rights-of-way than to run the mains over the public roadway. For a small consideration the Welsh farmer does not seem to object to having poles on his ground. It is better than running the risk of trouble with the post-office through interference with the telegraph and telephone wires. As regards crossing public roads, permission has been given to the

North Wales Company to do this at 10,000 volts, and no trouble is anticipated. Mr. Portheim.

Mr. C. P. SPARKS: While we are very much indebted to Mr. Addenbrooke for reading this paper, I should like to refer to the remarks he has made with regard to the Board of Trade. On p. 513 of the paper he tells us: "Unfortunately, until recently in this country, officialism and, to some extent, public sentiment have prevented the employment of this method on any scale"; and again on p. 516: "Notwithstanding all these obvious advantages, the attitude of the high authorities at the Board of Trade has until recently been so adverse to the use of overhead conductors in this country as to amount to a practical prohibition." I think Mr. Addenbrooke has taken a wrong standpoint with regard to the Board of Trade. Together with every other nation we started this business by distributing with overhead conductors. The Act of 1882 required the express consent of the local authority before overhead wires could be used. Owing to the fact that the provisions of the 1882 Act were very stringent, undertakings were started without any parliamentary powers at all; and the earliest transmissions, principally in towns, were by means of overhead conductors. By section 4 of the amended Act of 1888, the Board of Trade were empowered to require that the use of any of the overhead lines should be discontinued: that is to say, if they were of opinion that they were disadvantageous to the public, or interfered with the post-office, they could call upon the undertakers to discontinue the use. The work up to then had been principally in towns, and here in London a considerable amount of work had been done by means of overhead wires. The Board of Trade, not for the reason that the work was badly done or was especially dangerous, prohibited the use of the conductors in a town. When one considers the settled state of this country, I think they were perfectly right to have prohibited the use of overhead wire in such an area as London or in similar towns. But the real veto on the use of overhead conductors outside towns has not been the Board of Trade at all; it has been the local authority. Under the Act of 1888 local authorities alone had the veto. Mr. Sparks.

Coming to the Power Bills referred to by Mr. Portheim, in most cases the local authorities' veto is removed; and one can go to the Board of Trade if the local authorities' consent is unreasonably withheld, and obtain permission to put up overhead wires if a proper system is proposed. The Power Bill people appear until recently to have ignored the extreme importance of low distributing capital cost, and now we have a paper read by Mr. Addenbrooke throwing the blame of this matter on to a public department, which I do not think is quite fair. It has been suggested in Parliamentary Committee, and in the technical press, that possibly the interests of powerful people in the way of cable-makers has something to do with the delay in starting overhead transmission. Obviously their interests are directly opposed to the use of overhead conductors.

One word with regard to way-leaves. I am glad to hear Mr. Portheim has met with such little difficulty with regard to the matter in North Wales. No doubt the factor of obtaining way-leaves is the

Mr. Sparks. great difficulty in the use of overhead conductors, because if you manage to complete way-leaves for two-thirds of your line, somebody who has held out will ask exorbitant terms. Further, there is always the possibility of way-leaves falling in, when the terms that would be asked for renewal of the way-leaves will in many cases make them prohibitory. The solution is to work along the main roads, avoiding if possible the roads selected by the post-office.

I have some little experience with regard to overhead conductors, having been connected with the Grosvenor Gallery Station in 1885 and 1888. Recently I have erected overhead lines for three-phase transmission at 3,000 volts, the pole route being 8½ miles, and the conductors some 17 miles of three conductors. This route is not all in one straight line, but radiates some 4 to 5 miles from the centre. In one case we had to supply four collieries, and in the other six. The transmission line is erected on private property, but we had to cross the public roads of two local authorities, a railway company's line three times, and the post-office and Telephone Company's lines on many occasions. In all these cases crossings were negotiated without difficulty, and the requirements of the post-office and the other authorities easily met. With regard to the loss in transmission, I would like to ask Mr. Addenbrooke to give us the basis for his calculation on p. 514, which he tells us is based on 1 per cent. loss per mile. Is that energy loss, or is it pressure loss? If the latter, it affects the value of the calculation to a considerable extent.

Mr. Bloemendal. Mr. A. Bloemendal: I should like to say a few words on what I think is a very interesting and, at the same time, a very important paper. Unfortunately I have not had the same experience that Mr. Portheim has had with regard to overhead high-tension mains, and I think I may be permitted to quote an instance which occurred a short time ago in connection with a plant I was carrying out for my own company. Although it was only a small matter, it shows how some engineers, and even electrical engineers, think about the dangers in connection with overhead transmission lines.

I was installing a power plant working with continuous current at 500 volts. In connection with this a lighting plant was put down for 105 volts pressure, and we had to carry the current from the generating station over a distance of about 500 yards to a private house which was to be lighted up. We were obliged to cross a private telephone line belonging to another company, which connected this house with a station belonging to this other company. Of course, I never for a moment anticipated any difficulty, and I put up bare wires for this 105 volts for the 500 yards' distance. Imagine my surprise when, after a short while, I was requested by the company owning the telephone system to remove the bare wires and replace them by rubber insulated ones. It is true that for two or three poles the lighting wires were run parallel with the telephone wires, and I offered to put the ordinary nets between the two systems. I further offered to insert at the end of the telephone system the well-known protection fuses which have been used all over the Continent, to protect telephone wires from lighting or power wires; but all this without

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success. We had to take the bare wires down, and replace them by insulated wires, which of course increased the cost quite unnecessarily. Naturally, the insulated wires offer no protection at all, because after a time even the best rubber wires coated with rubber-proof compound corrode on exposure to the atmosphere. On account of an apparent safety in adopting an insulation which is worthless, an actual danger now exists.

I should also like to refer to another case. I was asked to report and advise upon the conversion of electric driving of all the machinery in five pits belonging to a colliery company. I suggested using 3,000 to 4,000 volts pressure, building one central station at one pit, and taking the overhead wires to the four other pits. Unfortunately, the powers that be viewed this scheme with very great disfavour, and we were not allowed to run the bare wires because they passed through two or three villages on their way to the pits, and consequently the scheme had to be abandoned because underground cables were prohibitive on account of the cost. This involved necessarily not only a loss to the electrical industry, but, as Mr. Addenbrooke very properly points out, also a national loss.

I have, during the last few years, been engaged in carrying out a great many high-tension plants in the North Bohemian coalfields. There it is the usual practice with companies owning two, three, or more pits to erect one central station, and to take overhead wires with any voltage between, say, three to twelve thousand across to their pits without the slightest difficulty. The only restriction the authorities make is that at the crossing of the roads the high-tension wires should be encased in a network in case of breakage, and when telephone or telegraph systems are crossed, that the instruments which lie at the end of that part of the line which is being crossed shall be protected by the well-known telephone or telegraph fuses.

A few words with regard to the breakage of overhead wires. I have made it a point when visiting high-tension central stations which transmit their energy over any great distance (and I have seen a good many of them in Italy, Switzerland, the South of France, and Austria), to inquire from the managers of these stations as to the occurrence of breakages in the overhead wires, and over a number of years during which I have annually travelled through these countries, I have never come across a single case where a breakage occurred which involved any damage to man or property. I myself in my experience only know of one single case in which a breakage actually did occur, and that was on a high-tension plant which I installed some years ago, working at 11,000 volts in one of the mountainous districts of Hungary. The wire broke during a tremendous snowstorm and gale, and also in this case no damage was done. It may be of interest to state that on each insulator I had fixed a copper earthed loop, so that when a breakage did occur, the wire at once became earthed and consequently harmless.

With regard to what has been said before, I should like to mention the case of a plant which I designed, and which has now been running for nearly three years, working at 21,000 volts. It carries the current for a distance of thirty-four miles and supplies a town in Styria with

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current for traction, lighting and power purposes. With this plant only three overhead wires, and no reserve wires of any kind were used, and, as far as my information goes, during the whole of this time only one single interruption has occurred, and that was caused by a sparrow flying into one of the lightning conductors on the line and making a short. I should also like to say a few words with regard to the difficulties experienced during thunderstorms and from lightning. I have never found any great difficulties occasioned by the effects of lightning. In a high-tension plant of the kind I have mentioned, working at 21,000 volts, I have found it entirely sufficient to use the well-known Wurtz type of lightning-arresters, and although the districts in which this plant is working is subject to very severe and heavy thunderstorms no damage in consequence of lightning has occurred to the machinery. Along the line at a distance of about three-quarters of a mile from one another I have used the well-known Horn type of lightning-arrester, but I found that it was necessary to have these arresters fitted with magnetic blow-outs. The Horn type without these blow-outs was first tried, but they were not successful. The great advantage of overhead transmission lines in a plant of this description is of course to be found in the fact that resonance is practically nil. If underground cables had been used (assuming for a moment that these be practical for such a high voltage) we should not have been able to do without static interruptors. I may mention that in this plant no static interruptors were used, and I have experienced no difficulty in switching the transformers on or off, although a great number of transformers are used in the various substations.

Mr. Addenbrooke mentioned in his paper the trouble caused by wilful breakage of the insulators. I experienced this difficulty with this plant when it was first started up, and it might be of interest to state that instead of resorting to the police for the protection of the line, we appealed to the schoolmaster and to the parson, with the result that after a fortnight or so, we had no more complaints to make of wilful damage done to the insulators.

The Chairman.

The CHAIRMAN (Mr. W. H. Patchell): It is evident that Mr. Bloemendal has had considerable experience on this subject, and I should be glad to have information particularly on one point: With regard to the earthed lightning-arresters, did he dead-earth them, or did he earth them through a resistance, and if so, what was its value?

Mr. Pooley.

Mr. F. POOLEY: Mr. Addenbrooke has certainly made out a very excellent case for overhead distribution with high-tension feeders. My first impressions on looking through the table of costs were that he had made the case too favourable to overhead conductors, but after verifying them I began to think, What is going to become of us poor cable-makers? We have either got to shut up shop, or else, if we want to take any share in these big overhead schemes, we must turn our insulating machines into stranders. However, I do not agree with the author that we are going to lose the distributing cables. When we once get inside a town, where most of the consumers will be located, we are always sure to have to supply underground. I do not think it is very likely to happen that we shall have to do the actual house-to-house

Mr. Pooley.

connection with overhead wires. Therefore as cable-makers, instead of looking on this overhead bare feeding as an enemy, we must look upon it as rather providing us with a fresh field which we should not otherwise be able to touch, owing to the fact that unless this cheap power were available, small towns such as these power schemes will serve, will have to do without electric light undertakings altogether. I do not think we shall follow American and Continental practice in this direction. With regard to way-leaves, I think quite enough has been said on that point, but I should like to say that some years ago I had to negotiate a way-leave for a single pole. In that case the occupier of the land required £10 per annum rental, apart from the sum that had to be paid to the owner of the land for legal permission to erect it. I do not think it is due to the fact that we use underground cables that we are behindhand in the application of electricity in this country; it is much more due to the fact that here we have certain vested interests to compete with, such as gas companies. We cannot go anywhere at all in this country to put in electric lighting but what we find that gas is there before us, whereas abroad, especially in America, they build the electric lighting station before they build the town probably. Consequently electric supply companies have nobody to turn out, and they have a much better chance of doing the turning-out themselves if competition should afterwards crop up. Then again, legislation is against us here, though, from Mr. Addenbrooke's paper, that appears to be in a fair way to be removed. The danger from overhead circuits is very easily prevented by two or three simple devices which most people are acquainted with. With regard to faults on overhead conductors, I do not think they are any more likely to occur than are faults on underground mains, and even if we should get a greater number of faults overhead it would not cause anything like the same dislocation to the supply as it does if the fault is underground, because it takes very much less time to localise and afterwards to repair. The unsightliness of overhead distribution is really, I believe, the serious objection to its adoption. The post-office have had a very good example of that recently, where they wanted to take a line through Epping Forest, but owing to the sentimental objection to the unsightliness of the poles they had to go round by a longer route. With regard to the painting of creosote poles, I think that is an impossibility. Once a pole has been creosoted, if it is attempted to coat it with paint the result is far from pleasing; the creosote comes through, and you get probably all the colours of the rainbow, and a most disgustingly mottled appearance generally. If it is desired to put up wooden poles and to get a pleasing result, the best thing to do is to use pitch-pine, which can, of course, be painted any colour and made to look extremely neat. Square poles look very well in pitch-pine, and although they are more expensive, their life probably is quite as long as that of a creosoted pole. With regard to the cheaper system of wiring which Mr. Addenbrooke mentioned, I hope he has some other explanation than that of the use of cheaper material and cheaper workmanship. At the present time there is certain wiring work put in which is cheap enough for the poorest consumer, and nasty enough to do incalculable damage

Mr. Pooley. to the electric lighting industry generally. I therefore hope he has in his mind some cheaper system which may be thoroughly well put in at cheaper rates than the cheap and nasty work often put in under existing systems.

Mr. Gaster. Mr. L. GASTER: I agree with the author that the subject dealt with in the paper is of vital importance for cheap electric distribution of power and light, and I should like to hear whether he has anything to say regarding the comparative merits of copper and aluminium for long transmission lines. From the large number of plants brought before my notice in the United States and on the Continent, and even in this country, in which aluminium wires for power transmission purposes have been employed, covering considerable distances and with varying voltages up to 60,000 volts, it appears that aluminium is likely to compete with copper in the future for such purposes.

Taking the actual ruling prices of copper and aluminium, it is claimed that for equal conductivity and efficiency an economy of about 10 to 15 per cent. can be obtained by using aluminium wires.

In view of the great portion of the total cost for power distribution which is attributed to the transmission lines, particularly with the increase of distances over which power has to be distributed, it might be worth while to consider the merits of aluminium wire as compared with copper wire. Although the resistance of aluminium is 60 per cent. greater than that of copper having the same sectional area, it is, however, more than made up by the difference in the weight of the two metals, the specific gravity of aluminium being 2·65, that of copper 8·93; so that for equal conductivity only about half the weight of aluminium is required. This involves a great saving in the cost of transport, and also a large economy in poles and supports in the case of aerial lines. The tensile strength for equal conductivity is about three-quarters that of copper wires, which I think for larger wires leaves ample margin of safety. I am given to understand that the almost insurmountable difficulty of jointing aluminium wires has now successfully been overcome by several effective methods.

For further particulars regarding aluminium, its production, properties, and use, as well as the use of aluminium alloys, I may refer to the papers by Mr. W. Murray Morrison and Professor E. Wilson, *Journal of the Institution of Electrical Engineers*, vol. 31, p. 400 and p. 321, respectively.

From the experience gained by the use of aluminium, it appears that it can be used equally as well as copper if the proper precaution is taken of inspecting the wire before being erected, and erecting it with due consideration of its properties. As far as I have been able to gather at present, the use of bare aluminium wires in underground conduits has proved to be economical and satisfactory. Owing to improved methods in the manufacture of aluminium, and to the increased output, the price may fall below the present rate of 1s. 3d. per lb., so that I think it possible to regard aluminium as likely to prove an efficient substitute for copper in the construction of overhead mains for power and light distribution.

It would be useful to hear whether the author has had any ex-

perience in the use of aluminium wires, and what prospect exists for their use in future. Mr. Gaster.

Mr. A. W. HEAVISIDE (*communicated*): The main point of Mr. Addenbrooke's interesting paper, as I take it, is, Why retard the development of the electrical industry by vetoing the cheaper and more efficient overhead conductors for any purpose in electrical distribution? The main objections to their use appear to be, first, the idea that they are dangerous to life; secondly, the sentimental objection to their presence in the public field of vision; and lastly, but most important, the practical difficulty in getting way-leaves from public bodies, and to a lesser degree from private owners. Mr. Heaviside.

The author has stated the case fully and carefully argued it out; nevertheless, perhaps I may be allowed to make the following remarks, which are entirely personal to myself: I agree that it is axiomatic to say that centralised production of electric energy on a large scale, the more continuous the better, leads to great economy in the cost of manufacture—all things being equal. Then, having produced the power, it has to be distributed, and, as pointed out by Mr. Addenbrooke, the distribution is greatly cheapened and facilitated by the use of overhead conductors.

I must endorse this view thoroughly as an undisputed fact, and as it is too late in the day to bring illustrations in proof thereof, such arguments would only waste the time of the meeting. Of course each case must be taken on its merits, as also must the question as to where overhead work should end, and where underground work should begin. In this country it is only in a few cases that it is desirable to have wholly one, or wholly the other; or, say, there are so many exceptional conditions to be met that hard-and-fast rules are not applicable. It therefore becomes a question of construction, and before one can construct one must have the way-leave power to do it. In my opinion the roads of the country should carry the service—the principle of the design of most installations should include this; private property should only be used incidentally. The uses of a public road expand with the ever-increasing public demands. And who shall say the public nay?

As an old stager in the art of getting way-leaves I may perhaps be allowed to sum up the situation thus: To quote the poet, "There is no darkness but ignorance." Now the ignorance and small-mindedness of many local authorities is the thing that has to be overcome by him who enters the way-leave field, and it is this ignorance which leads to so many dreadful-looking structures for the support of wires of any kind—things at all angles and upon every one's sky-line. Whilst Hogarth's line of beauty is difficult to maintain in such structures, what can be obtained is this—a structure that looks as if it fitted its purpose, to which the eye naturally reconciles itself.

"The black ugly creosoted pole" is only objectionable when improperly handled, it is only objectionable in imagination when properly treated. But it is so seldom one can get the chance of properly treating it, due to the want of co-operation on the part of local authorities in the selection of route. Where there is co-opera-

Mr. Heaviside.

tion between the local authorities and the engineer, in the majority of cases, routes can be found that permit of a seemly structure, and at the same time a stable one. There is little occasion to strike flagrantly across the sky-line of a pretty village, and if more public spirit and less ignorance existed there would be no trouble in the matter. To return to the creosoted pole. It is an honest thing and much better in the long run for everybody than the painted work, which for 90 per cent. of its life is in the condition of dirty finery.

However, taking things as they are, unless he who seeks way-leaves has great acumen, personal force, the faculty of taking trouble, and withal plenty of time to do it in, things must go on much as they are—that is, where expediency is not adopted the ultimate cost is always greater, which, say what you will, the public ultimately have to bear. But in these days, who has plenty of time? The best of us break down under the influence and pressure of obstruction, obstruction sometimes under a leadership not disinterested. Hence legislation is desirable, and in the direction indicated in Mr. Addenbrooke's Continental rules.

As regards the private owner, my experience has been that usually he is easier to deal with. I am speaking of where work is mainly on roads and only incidentally upon private property, which is the principle I advocate. The private owner is generally open to conviction, and his public spirit can be roused—only another illustration of how nice individuals can be when not forming a unit of a collective body. Supposing the way-leaves are secured, suitable wire supports follow. They need not be very thickly planted, because the wire usually employed being of some strength, breakages are not likely to occur from stress of weather. That is, as we all know, if the elasticity of the wire is allowed full play, and not overstrained so as to bring about a permanent set. Then the contraction due to cold and the elongation due to wind pressure can be fully met. It is the waving-cornfield principle that saves it. Thus, given a suitable structure, the danger to life from high pressure or big currents is very remote. Indeed, in my opinion the overhead trolley wire that is constantly being disturbed by the trolley-pole is a vastly greater danger; but being under supervision, how rarely one hears of any trouble! Of course, high pressure, or any pressure services, must be under supervision, and my experience is that the duties of the supervisor are very light, if, in the first instance, the work has been properly planned and executed.

The insulator-breaking trouble demands attention. In Great Britain, I venture to say, there are too many magistrates of the Great Unpaid order and too few independent stipendiaries. In Great Britain, especially in the colliery and manufacturing districts, one of the pastimes of the people—the ignorant—is to go about breaking insulators with stones from catapults, or hand-thrown. Sometimes, though proportionately rarely, offenders are caught by the police. Then the Great Unpaid, if the case is very clear, usually punish the offender by exacting a nominal fine—ridiculously inadequate as a balance to the offence or as a deterrent. When in Switzerland, I found that imprisonment was the punishment that fitted the crime,

the offence being taken as one against the wellbeing of the State. Hence way-leave legislation should include pains and penalties to meet such cases. Mr. Heaviside.

Perhaps Mr. Addenbrooke will allow me to explain my view of private electric lighting as a luxury. I agree that it is so. It is a luxury to have such a light, the light of convenience and beauty which diminishes not the purity of the air, hence preserves the health of the individual and the surfaces of his environment. Its first cost may be greater, but the advantages named, if taken at their true value, would prove it to be cheaper than all other illuminants. It was my fortune to visit the recent Gas Exhibition at Earl's Court. Wherever the light was in a confined place headache followed. Gas is a power and has its place, but not for lighting either the peasant's cottage or the king's palace.

Mr. F. GILL (*communicated*): The purport of the paper is to show that overhead mains are more economical for power transmission than are underground, but one does not find in the arguments facts which show that this is really so. Many persons are, of course, anxious to see overhead mains built, and it seems a pity that this question of cost should not be adequately dealt with. The author, while advocating overhead mains on the score of economy, only quotes figures of capital cost in support of his statements, and he does not in any way deal with the question of annual costs. Such a method is quite unsound, and no comparison between the two types of line is worth anything at all unless the total annual costs of each is investigated; such costs including, of course, interest on capital, depreciation, sinking fund (if necessary), and upkeep. It is quite clear from remarks throughout the paper that the author appreciates the necessity for efficient maintenance of the lines when erected, and it is therefore the more to be regretted that he has given no figures which enable the annual costs of overhead mains to be compared with the same costs of underground mains. If he has already investigated these annual costs, perhaps he will give them in his reply. One would like to know the main particulars of his estimates for the various lines, both overhead and underground, the rate of interest on the capital, the rate of depreciation allowed on the various portions of each class of line, whether sinking fund is allowed for, the provision for maintenance (not forgetting the cost of patrolling, arm and pole burning, stone-throwing and kite-tails), and the allowance for way-leave rentals. I do not say that overhead mains will come out more expensive than underground, but I do say that without such a comparison of annual costs, the investigation is very incomplete. Mr. Gill.

As regards creosoted poles, I do not know of any satisfactory means of painting, though many plans have been tried. Of course it is not necessary to use a creosoted pole at all, but if he does not, the author must make due allowance in his depreciation fund. As to the blackness of a creosoted pole, the colour closely matches the trunks of trees.

I have endeavoured to check the capital cost of the overhead mains on the basis of telephone experience, but unfortunately the author has given no details of the proposed construction. In the absence of such

Mr. Gill. details, I have assumed the following: 38 28-foot poles; 6 36-foot, and 5 extra poles of 45-foot, per mile, all creosoted, the last being for special crossings; on the 266 and 468 lb. wires, 4 stays to every fifth pole and one stay to each of the other poles, increasing the stays for the heavier lines; earth wire on each pole; high-tension insulators; spacing of wires 22½ inches; arms braced; and strain insulators in the stays. I have taken solid copper conductors; it is not apparent why the author should propose stranded conductors; for example, out of 47 American plants only one uses stranded copper, 36 use solid copper, and the remainder use aluminium. I have made a small allowance for wire nettings over other wires, although generally speaking the best cross-over method is by short span construction. I have not made any allowance for special iron work, if required, over railways, switchgear on the lines, telephone lines, telephone sets for the patrol men, nor for insulated stands for the men when using the portable telephones.

Estimating upon these figures the capital cost for overhead mains for 12,000 volts seems about right, but the cost of mains for lower pressures seems too low. It is not clear to me why there should be a difference between the capital cost of a specified set of conductors operated at pressures ranging from 6,000 to 12,000 volts; the difference in pressure does not appear to be sufficient to represent any substantial difference in the capital cost of the line; in the table of capital costs, however, there is an almost steady increase of £15 10s. per mile per 2,000 volts rise in pressure.

The sympathy of every telegraph and telephone engineer is with the author's remarks as to way-leaves. Some of us have lived with this trouble for years; unfortunately, however, the Legislature has not seen fit to give relief, and one can only hope that those building power lines will be more fortunate.

Mr. Highfield. Mr. J. S. HIGHFIELD (*communicated*): I think that any engineer who is operating in the country will be thoroughly in agreement with Mr. Addenbrooke that it is highly desirable that sanction should be given in reasonable cases for the erection of overhead wires. The paper is so complete that there is no possibility of adding very much to the information, but I should like to endorse Mr. Addenbrooke's view as to the great benefit obtained by the use of overhead wires, as they can be put up for temporary load and removed at low cost. This is particularly true in the case of a colliery district, as in times of brisk trade, when the price of coal is high, it is very common for quite a number of small workings to be commenced, and very often old shafts are opened up and the working of the mine re-started. In these cases, if overhead wires can be run, it is nearly always possible to get quite a good load, which may, however, not last for more than a year or two; this could not be obtained if underground cables had to be used.

I have used overhead wires for tramways for some years, and in a country district where the number of cars run per mile of track is small, the cost of underground cables is hardly ever warranted, and the advantage of being able to put up overhead feeders is very great. The St. Helen's Corporation tramway lines were equipped with over-

head feeders, and sanction was given by the Board of Trade for their use as long ago as July, 1899. In most cases a feeder consisted of one or two wires of 0000 S.W.G. No trouble of any sort has ever been experienced with these lines, and, so far as I am aware, they have never come down. Some time later it was required to supply works situated across-country from the power-station, there being no direct roads along which the cables could be laid. The district between the works and the power-station consisted almost entirely of ground covered with chemical waste, and if cable had been laid in the footpaths running through this district, it would have been liable to great deterioration. Consequently application was made to the Board of Trade in November, 1901, to erect overhead wires to supply these works. In spite of the fact that the tramways had been fed with overhead feeders running along the sides of the roads for two years previously, the Board of Trade took great exception to overhead wires running along these remote footpaths where practically no traffic existed; but after a considerable amount of trouble and a great deal of persuasion their consent was at last obtained in June, 1902, and the wires were laid, and have since worked quite successfully. These wires work with 500 volts pressure only. The poles consist of stout telegraph poles, double poles being used round curves. The insulators are of strong porcelain. A system of overhead wires is particularly useful in the case of village lighting. The villages of Lynton (in North Devon) and Mevagissey (in Cornwall) have been successfully lighted for a long time now, almost entirely with overhead wires. In both cases probably, if it had been necessary to use underground cables, the cost of lighting would have been prohibitive. Mr. Highfield.

If care is taken in the placing of the insulators, and in the positions and designs of the poles, the lines are hardly noticeable.

Mr. J. R. DICK (*communicated*): The following particulars with reference to one of the high-tension transmission lines in Lombardy show how small is the risk of breakdown and damage to property which has been commented upon in the paper. The installation has now been at work about seven years, and during that time only three breakages of one or more wires have occurred. The first was due to a heavy snowfall, and the other two to the failure of an insulator, in which case the conductor was melted by the arc which formed between it and the pin. Neither of these breakdowns resulted in any damage to life or property other than the transmission line itself. Although the climate in Lombardy is very trying during the winter, on account of the heavy rains and dense fogs, during which every object becomes thoroughly soaked with moisture, there was no detrimental leakage at the insulators. The breakdowns from lightning are comparatively infrequent. In general they are due to discharges between adjacent lines or between a line and the pole. In a few cases the lightning-arresters have been burnt out, but the stoppage of the supply never lasts more than three or four minutes. The average number of breakdowns from all causes is about five or six per annum, and the duration of the stoppage of supply was not more than five to ten minutes in any case. This result is achieved because of the Mr. Dick.

Mr. Dick. duplicate line which always permits the supply to be continued with one-half of the wires. The longest break in the supply was due to a lightning discharge interrupting the telegraph lines as well, thus preventing instructions being sent to the central station. The difficulties of obtaining way-leaves and expropriation under the Italian law are comparatively small, and provision is made for a fair bargain between the company and the proprietor on a definite basis. The cost of expropriation per metre of line and width of land varies from about 3d. to 4s., the average being about 10d.

Mr. Kensit. Mr. H. E. M. KENSIT (*communicated*): I should like to make a few remarks on the interesting points raised by Mr. Addenbrooke in relation to obtaining way-leaves for high-tension overhead transmission lines. I have recently spent a good deal of time in negotiating way-leaves for three such lines, two of about 9 miles and one of 3 miles—and it does take a considerable time—in fact, unless one is prepared to pay heavily, it is necessary to allow six or eight months to arrange cross-country way-leaves for even a few miles.

It is of very great interest to learn of the law as to arbitration on the subject of disputed way-leaves in Switzerland and Italy, and there is no doubt that such a law would be of immense service as a handle in getting opposing landlords to come to reasonable terms by voluntary agreement. In several cases I have been asked whether my company had any compulsory powers for pole-lines, and on replying in the negative I have been told that in that case they would not even consider the matter. A single such case may mean altering the route for miles and entering on a fresh set of negotiations with, say, twenty or thirty owners and tenants, thus involving the expenditure of a large amount of time and money.

With reference to the proposal for an arbitration law, it may be said that the telephone companies get on all right without it, and why cannot the power companies? Now, the fact that the Board of Trade will not allow a high-tension transmission line along the roadways and are opposed to their even being placed along footpaths, makes a power line a very different proposition from a telephone line. There is no question that poles in a cultivated field are a nuisance, and when you can obtain the way-leave the rent required may be anything up to £1 a pole or more. In my own experience I find that the average rent for a line across ordinary farming country will be at least 8s. per pole, or £20 per mile per annum, and this is liable to be increased after the line is built. The greatest disadvantage of cross-country way-leaves is that they can usually only be made subject to six or twelve months' notice, to allow of building, sale, etc., and this leaves endless opportunities for demands for increased rent, notice to quit, etc. In many cases the most direct route between two points of supply is that taken by the railway, and it occurred to me that if a right-of-way could be secured from the railway company it would vastly simplify matters, both by reducing the number of owners and tenants to be dealt with from many scores to only one, and by making the question of rental more definitely ascertainable and stable. I found by inquiry that in the part of the country I am dealing with the railway company's fence is

Mr. Kensit.

always placed inside their actual boundary by never less than three feet, and often nine feet or more. This would make it possible to erect a line outside their fence and yet on their land, so that one would be independent of other landowners, and yet would have access to the pole-line without trespassing on the railway line. I approached the Board of Trade to ascertain what their views would be respecting such an arrangement, and was given to understand unofficially that it would be favourable, and that it would be preferred to construction close to country roads or footpaths. I also approached the railway companies, and gathered that they would not be strongly opposed if the line was constructed to the satisfaction of their engineers, and if they were satisfied that there would be no induction troubles with their telegraph and telephone lines. I have not carried this matter far enough to speak more definitely just yet, but the method appears to offer such advantages that, although the cross-country way-leaves are practically completed for the two 9-mile routes mentioned, it is probable that it would be worth while to throw them over for the route along the railway wherever possible. In discussing the cost of way-leaves I have been considering wooden poles and a span of about 120 feet. It seems probable that the cost and many of the difficulties of obtaining way-leaves might be considerably reduced by the use of iron or steel poles or towers, giving a greater height and span, with but little additional capital cost.

In a paper read by Guido Semenza on "European Practice in the Construction and Operation of High-pressure Transmission Lines," before the American Institute of Electrical Engineers, on February 26, 1904, he gives the comparative cost per mile of construction with fifty-three wooden poles or with sixteen iron lattice poles set in concrete, and in each case allowing 8s. per pole for "right of land occupation." The cost per mile with wooden poles is given as £116, and with iron poles £122. The spans with steel poles are from 300 to 400, and even 600 feet. In the discussion on this paper particulars were given of the 100-mile steel tower transmission line in Mexico, which uses only twelve towers per mile. These are of the windmill type, weigh 1,500 lbs. apiece, and stand forty feet high on an eight feet square base. The cost of towers and insulators was about £140 per mile. With these greater spans it could be arranged in many cases to plant the poles in the corners of fields or straddling the hedges, so as to reduce the interference with cultivation to a minimum, and against the somewhat increased capital cost must be placed the lower cost of way-leaves and maintenance, and the reduced risk of breakdown.

Mr. Addenbrooke.

Mr. ADDENBROOKE, in reply, said: I will try and deal with the speakers in their order. First of all, I must thank Mr. Gavey very much for the way in which he spoke. With regard to the costs for various voltages, the tables were made up from actual estimates and tenders. The variation in cost for the different pressures is due to extra quality of insulators, to rather longer poles, perhaps to rather a greater section of pole in the case of the higher voltages, and in various other ways which it is difficult to specify. It is quite correct to say that the tables are made up for only three wires on the poles. The table was made up in that form after a good deal of consideration, because it is very

Mr Addenbrooke. difficult to make comparative statistics with more than one circuit. Before doing so, however, I collected, as I have done for some years, and obtained in various ways a great number of statistics relating to overhead wires; and I find that a very large number of circuits of great length are used in different parts of the world, having only three wires, and are found quite sufficient for running large manufacturing works in America and on the Continent. In some cases more are used, and in those cases, comparing overhead transmission with cables, there is the extra advantage that you can put up an extra set of wires on poles at very little more than the cost of the copper, arms and insulators and the erection. I have not taken advantage of this facility in the tables, although in my opinion it is a most material one. For instance, perhaps I may be allowed to mention a practical case which I am about to deal with. I am the part possessor of some coal mines in South Staffordshire, in the area of the Midland Electric Power Corporation, Limited. I applied lately to the Company to see if they could give us energy, they having their circuits within half a mile. They gave me a favourable quotation for the current, but said that the cost of bringing the cables to the mines would be about £400, and that they would require 5 per cent. interest on that amount guaranteed in addition to the cost of the current. I wanted that current for finishing the sinking of new shafts, and afterwards for working them, if successful, in proving the coal. But I was not able to give an absolute guarantee. It may be that that coal will turn out very good indeed, or it may be that it will not be very good, or there may be difficulties connected with water, or something that might prevent my going on. Now it is a serious thing for a company to have to lay a cable costing £400 for a single customer unless he can give them a fairly long contract, because should the thing turn out unsuccessful the cable is of practically no value. On the other hand, if those coal mines turn out well a very much larger quantity of current would be required, and the first cable that was laid would probably not be large enough, and we should have to pay another £400, or the Company would have to incur the risk of considerably more expenditure to start with. On the other hand, you will see from the tables that half a mile of overhead wires, conveying about 100 kilowatts, which is all that is wanted to start with, at 8,000 volts, would cost a very small sum indeed; and as I have mentioned in my paper, supposing things went wrong those poles and wires could be taken away, and would still represent perhaps three-quarters of their original value. That from a financial point of view is a very important consideration. As regards running circuits near Post Office and other wires, a great deal depends on the question of crossing. I will not weary you by going into the question. I hope some day or other the pamphlet of the American Institute of Electrical Engineers, which I referred to in my paper, may be published—it deals with this question. It came to me through a friend in America, and I do not know whether it is private or not, but it does contain an immense amount of information on this question, of a practical character, from people who have had this class of circuit in use for some years, and they seem generally agreed that by a sufficient number of crossings—(I take it that perhaps both the

Mr. Addenbrooke.

telegraph wires and the electrical energy mains may have to be crossed)—it is quite feasible to be pretty near neighbours without too much disturbance; in fact, there seems a general consensus of opinion that it is quite possible to run telephone wires on the same poles. As a matter of fact, it is the habitual practice almost everywhere to run the telephone circuit for signalling and communicating purposes along the same line of poles underneath the high-tension circuit, and by crossing and perhaps twisting, the induction is got rid of to a sufficient extent to allow speaking on a very considerable number of miles of circuit.

I do not know that I need say anything on Mr. Portheim's observations, except that they confirm the great value of overhead conductors. No doubt in the class of district he is dealing with way-leaves may not be at all difficult to obtain, the country is open, and there are not too many trees. But the country in different parts of England differs very much. In many districts there are large numbers of trees, and the fields are enclosed. I was not quite clear what he said about running along the roads. It is possible that the Board of Trade are allowing conductors to be put along the Welsh roads, but in my own negotiations with them they have practically told me that they could not, at present at any rate, tolerate wires along main roads, although, as Mr. Kensit has mentioned, they are maybe allowed along pathways.

Mr. Portheim.

Mr. PORTHEIM: They have given permission in some instances along the main roads. In the other cases it has been cheaper for us to take a private right of way.

Mr. Addenbrooke.

Mr. ADDENBROOKE: I hope to a considerable extent this general permission may be enlarged. The more we get accustomed to overhead wires the more we shall find out, as a later speaker said, that there are really no accidents. When the Institution party were in Italy the year before last, I had some conversation with Mr. C. E. L. Brown on the subject, and I asked him whether in his experience, which I suppose is second to none in Europe, he knew of any serious accident which had happened to members of the public on circuits which his firm had erected, and he told me he could not recall a single one. He did know of two or three cases in which workmen by attempting to work on live poles had got bad shocks, and had even been killed, but as regards accidents to the outside public he said he did not recollect a single one, and there seems a very large consensus of opinion to the same effect.

Coming now to Mr. Sparks's observations and to my reference to the Board of Trade, the words I used in my paper were very carefully chosen. Although I have not alluded to it in my paper—and if Mr. Sparks had not spoken I should not have alluded to it at all—I believe it will be found on inquiry, that the permission of the Board of Trade to use overhead wires was in the first instance obtained by me, and I obtained it after an appeal to the Committee of the Institution to help me, which they did not see their way to do, so I carried the matter through by myself. I do not think any one therefore is in a position to say what opposition there was and what opposition there was not except those who have seen the actual correspondence. As regards the electrical officers of the Board of Trade, they have I think always been ready to meet one halfway, and to go even further, but the

Mr. Addenbrooke. resistance to granting overhead wires, or granting them in any practical form, came from other directions. I do not wish to specify them, but in the course of the negotiations which I had with the Board of Trade I came pretty closely in contact with some of their departments, and was able to see the working of them. It must be remembered that this paper was written more than a year ago, but I thought under the circumstances it would be better to leave this matter as it stood. The year before last certain questions were asked in the House as to what the President of the Board of Trade was doing in the matter. Those questions were asked because I found there was such heavy resistance, and were very largely prompted by me through a member who was kind enough to take the matter up. At that time Mr. Balfour's reply was to the effect that permission for two undertakings had then been granted. As the Board of Trade had granted me permission for two undertakings at that time, I think it may fairly be stated that those were the only two undertakings in the country for which permission had been granted, and all I can say is that there was a very stubborn resistance on the part of certain people at the Board of Trade, first of all, more or less, to giving way on principle, and secondly, when they gave way on principle to allowing the thing to be put into practical form. I believe that people who have applied lately have found they get on pretty well, but if I am not mistaken those two early permissions had a great deal to do with it. Of course I remember the Grosvenor Gallery conductors very well; they were under my charge for some time. All the early work was covered. No doubt there was a good deal to be said for taking it down, but, on the other hand, similar work has remained up in Italy, all over the United States, and a great many other places. What I always say on this subject is that continental cities have not been burned down. There is an idea here that unless you run electrical wiring in certain ways specified by the English insurance companies there will be a universal conflagration. But if you go all over the Continent you will find wiring done in a way which would give English insurance companies fits, but nevertheless you will find that the insurance companies there have not become bankrupt, nor have they made any big protests against the class of work that is done there. I do not say it is the best class of work, or that it is work done in the best style; but it is a very great pity that if work done in that style stands as well as it does on the Continent that we should be deprived of the advantage of using it in England.

Mr. Kensit spoke on the question of way-leaves in various parts of the country. I have had a double experience in the matter. In one case the way-leaves were very easily obtained, and in the other there has been considerable difficulty. As I mentioned in the paper, the principal obstacles are cantankerous trustees, who will not take any responsibility, or—I do not wish to malign the profession—the solicitor who is on the make. Between those two classes of people you may get very seriously floored, and of course the local authorities have also their say in the matter, but a great deal of the say of the local authorities is due to the solicitor, who is their clerk. I think I had better not pursue further that aspect of the subject.

Mr. Addenbrooke.

I was exceedingly interested in what Mr. Bloemendal said, and particularly his reference to the schoolmaster and the parson as being effective in preventing insulators being broken. I have here the boiled-down replies to a number of questions which the American Institution addressed to the managers of different plants throughout the country. A great many of the plants experienced trouble from having the insulators on their lines maliciously broken, and the following means were taken to stop it:—(1) By posting warnings; (2) offering rewards for evidence that would enable them to prosecute and convict guilty persons; (3) having watchmen along the line; (4) arrest and fining. And then there is a note in brackets below, "Not much success with any of the above methods." I notice that the parson and the schoolmaster do not appear to have been tried. Perhaps we can give them a hint to this end.

One speaker has referred to the cable-makers. If I thought I was going to do damage to the cable business I might not have read the paper, but my own view very strongly has been—and I know it is held by a certain number of cable-makers, at any rate—that a general adoption of overhead mains would lead to such an extension of electrical work in this country that much larger quantities of cables would be used. Again, the cable-makers, a large portion of whose business consists of contracting for laying cables, can just as well contract for putting up overhead lines; there is just as much profit to be gained in that way as by laying cables under roads. As regards painting poles, it is not perhaps necessary to use the ordinary paint. Some paint may be found which will do. All I pointed out was that it is the very dead black of the creosoted pole that makes it stand out so much in the landscape. While I am on that point I should like to mention a matter which has come more prominently forward in the last few months, and that is that in America, and to some extent on the Continent, they are now beginning to run very much longer spans of overhead mains. The early bare overhead mains on the Continent, I believe, were mostly put up with soft copper, but by using stranded, hard-drawn copper it is possible to run spans up to one hundred and fifty or more yards quite easily; and some very long lines of overhead mains have been erected, using light steel towers as supports, which can be made at something like £10 a-piece. The spans average one hundred and fifty or one hundred and sixty yards. With such a class of construction it is easy to carry the wire ten or fifteen feet higher than is customary with poles. The mains are in this way kept more out of the way. You only have one-fourth or one-fifth of the way-leaves to get; and in an enclosed country like so much of England you could go from corner to corner of the fields, right across them, and avoid planting poles in the middle of agricultural land, which causes difficulty when using ploughs. I believe it is highly probable that a method analogous to this will be found the best solution to many of our difficulties in this country in running overhead mains. As regards the relative value of aluminium and copper, I have very little experience. What I have just said bears on the question if you are to run long spans, which I believe are very advantageous, of course you must have something

Mr. Addenbiooke.

with a high tensile strength, and I do not think aluminium is altogether suited for this. At any rate, I do not look on it as a very material factor in the success of overhead mains in this country in the immmediate future.

A great many people have told me that I was entirely Utopian in suggesting that it was possible in this country now, or in the future, to do anything to regulate in a better manner the running of overhead mains and obtaining way-leaves for them; but it appears to me that whether one succeeds in the near future or not, there is nothing like making a beginning. I must say I was very much struck in reading through the Italian law, which seems to put the whole thing so clearly and pertinently. It is a curious fact, which was pointed out to me by an engineer in speaking on the subject, that there does exist already in this country a set of circumstances under which compulsory way-leaves can be obtained. In Italy you will notice that the law of electrical conductors is based on a pre-existing right of carrying water-pipes. We have not quite the same thing in England, but all local authorities, I understand, have a right of carrying sewers practically where they like. It was found when sanitary questions were considered that sewers had to follow the contour lines of the country very much, to get the regular requisite fall, and in order to enable local authorities to obtain this without having recourse to Parliament on every occasion, which would create a lot of unnecessary trouble and bother, there is, I believe, a general enactment by which a local authority can take private way-leaves to carry a sewer through private property. I do not know whether the two things are quite analogous, but still there is some sort of analogy in this. There is no doubt in my mind that we are beginning to be so dependent on electrical communication of one sort and another, whether for power, for telephones, or for telegraphs, that some regulation by which such wires or mains can be got across the country in the best possible manner would be greatly to the interest of the people of this country generally.

The Chairman.

The CHAIRMAN: I am sure it will be your wish to record a very hearty vote of thanks to the author of this most interesting paper. We hope local authorities will soon realise that highways were made for overhead wires, and will give us the facilities we need for them! I propose a very hearty vote of thanks to Mr. Addenbrooke for his interesting paper.

The resolution was carried by acclamation.

Proceedings of the Four Hundred and Nineteenth Ordinary General Meeting of the Institution, held in the rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 23, 1905—Mr. W. H. PATCHELL, Vice-President, in the chair.

The CHAIRMAN: Gentlemen, I must apologise for the absence of our President to-night, who is unavoidably detained and cannot be with us.

The minutes of the Ordinary General Meeting held on February 9, 1905, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council:—

TRANSFERS.

From the class of Associate Members to that of Members—

A. C. Cormack.
John Turner Morris.
George James Ralph.
S. G. C. Russell.

From the class of Associates to that of Members—

Cecil C. F. Monckton.
Fred Wm. Smith.

From the class of Associates to that of Associate Members—

Henry H. Arthur.
Allen M. Coombs.
James J. W. Grigg.
James Lord.
Robert S. Newton.
F. B. O'Hanlon.
John E. Pownall.
Henry S. Watson.
Walter T. Wheeler.

From the class of Students to that of Associate Members—

Alfred Sadler.

Messrs. J. T. Morris and H. M. Sayers were appointed scrutineers of the ballot for the election of new members, and at the end of the meeting the following were declared to have been duly elected:—

ELECTIONS.

Member.

Charles L. Edgar.

Associate Members.

James Askew.
William Briggs.
Joseph Wilfred Hadfield.
David Huntley.
James Samson Morton.
George Reginald J. Parkinson.
John Sedgwick Peck.

Students.

Henry Raper Ainsley.
Ernest Barraclough.
Kenneth Dudley Bullpitt.
John Arthur Cook.
Harry Corney.
Thomas Carey Dunne.
Alexander Victor Ferguson.
Herbert Owen Michael Gammon.
Charles Edgar Garratt.
Ernest William Heathcote.
Leslie Barnett Hewitt.
Clifford Hobson.
Robert Henry F. Houstoun.
Henry Thomas Jager.
Robert Donaldson Jenner.
Donald Knowles.
Thomas Saumarez Lacy.
Alfred William Lambourne.
William Thomas Massey.
Max Mercer.
Ernest R. Myers.
Herbert Pearce.
Thomas Sylvanus Pipe.
William Arthur Pitts.
William Rodger.
John Ernest Roberts.
Howard Mark William Royce.
Frederick Clifton Russell.
Charles Ernest Savage.
R. Savarimathen.
Robert Shaw.
Thomas Mountford Taylor.
Thomas L. B. Westerdale.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. L. Andrews, A. Artom, *The Electrical Review*; to the *Building Fund* from Messrs. R. H. Burnham, R. Livingstone, J. C. Smail, L. C. B. Trimnell; and to the *Benevolent Fund* from Messrs. G. D. Gibson, R. W. Hughman, S. Insull, A. W. Manton, A. Wyllie, to whom the thanks of the meeting were duly accorded.

The discussion on Mr. Addenbrooke's paper was concluded (see page 532), and the following paper was then read.

The meeting adjourned at 9.30 p.m.

SETTING TYPE BY TELEGRAPH.

By DONALD MURRAY, M.A. (Sydney).

(*Paper read February 23rd,* 1905.)

Any person who has had occasion to examine the records of patents connected with telegraphy, must have been astonished at the number of printing telegraphs invented during the past sixty years. In the United States alone over four hundred printing telegraph patents have been issued since the invention of the electric telegraph. These patents embrace about 150 distinct printing telegraph instruments or systems; and yet, with the exception of the stock-tickers and the Hughes printing telegraph, it is only within recent years that two or three really successful machines have emerged. In hardly any other field of human endeavour has so much labour resulted in so little return. There are many reasons for this want of success; but they are all due to the extreme complexity of the conditions to be fulfilled, and the absence of any technical literature explaining what these conditions are. With one or two exceptions, telegraph engineers, realising the difficulties of the subject, have left it alone, and printing telegraph inventors have, in most cases, been outsiders. In fact the whole of the problems connected with the handling of type, including typewriters, type-setting, type-casting, and similar machines, have proved very refractory, and the best solutions have often come from outsiders, who have stepped in where experts feared to tread, and have in this way blundered into unexpected success. The complexities culminate in the printing telegraph, because in that case the problem is to set type at a distance. The type may be fixed on the circumference of a wheel, or may exist as separate type on the ends of type-bars, as in most typewriters, or as loose type in a type-setting machine; but in all cases the problem is to set type—that is to say, to bring a particular type to a particular printing point in the shortest possible time, and in the case of the printing telegraph to do that at a distance over a single telegraph wire.

It may be pointed out, in passing, that all telegraph systems, from the Morse key upwards, are printing telegraphs more or less developed, and that a completely developed telegraph system must be a printing telegraph. Telegraphy is one of the few branches of human activity in which the tendency to substitute machinery for human skill has not yet made much progress; but the advantages to be gained are considerable, and there is every indication that the era of fully-developed machine-telegraphy has now arrived. The subject is very large, and

it will be necessary to confine attention, in this paper, to the class of printing telegraphs used, or being tried, by telegraph administrations for the transmission of ordinary commercial and press messages. For this class of work the stock-ticker systems are of course unsuitable, their field being the urban distribution of news, with some possibilities as local feeders to the general telegraph system. The Hughes printing telegraph is widely used for general traffic on the Continent of Europe, where about three thousand of these instruments are in service; but the Hughes prints the messages on a tape, and its speed is limited by the manual skill of the operator. For a long time there has existed a need for increased capacity of transmission, printing in page form, reduction in the percentage of errors in transmission, and increased economy of labour. During the last few years there has been a remarkable outburst of activity amongst printing telegraph inventors desirous of meeting these requirements. The Baudot system, widely used in France, dates back to about the year 1880; but the Rowland, the Buckingham, the Murray, and the Siemens and Halske systems are all products of the past four or five years. It is with systems of this kind that this paper proposes to deal.

THE ESSENTIAL FEATURES OF TELEGRAPHY.

If we disregard the small class of telegrams that merely express emotions, the essence of telegraphy is control. When A sends a telegram to B, it promotes or restrains the actions of B, and B frequently reciprocates by controlling the actions of A. Telegraph systems, therefore, belong, not to the class of producing or distributing, but to the class of controlling mechanisms. From the point of view of the theory of machines, the only possible form of controlling mechanism is the lock and key. It is for this reason that telegraph instruments consist almost entirely of ratchet mechanisms, including the complicated permutation locks that form the main feature of all the modern high-speed printing telegraphs. At the transmitting station certain ratchet mechanisms are used to impress permutation patterns on a small stream of energy which flows through the intervening space from the sending to the receiving station. It is these peculiar patterns impressed on the energy stream that operate the permutation locks of the receiving mechanism, and thus subsequently determine the motions of the recipient of the message.

Hence the fundamental feature of printing telegraphy, and therefore of all practical telegraphy, is the kind of patterns or permutations transmitted. In telegraphy these patterns are symbolic. The word, as spoken, has to be broken up into letters, and the letters translated into special telegraphic symbols. On the other hand, the essence of telephony being direct communication, we are forced to use a method preserving the exact form of the word symbols that we habitually use. This results in the employment of excessively complicated patterns or variations of the energy stream, so that about thirty impulses per letter are required in the telephone, as against about four per letter with the Morse alphabet. That is to say, the telephone saves labour by giving

direct communication only at the expense of very heavy wire cost. The telegraph, by using the symbolic method of representing letter sounds, effects an immense economy in the number of signals per letter, thereby effecting a great economy of wire, but only at the expense of increased labour cost for translation. It is the object of machine telegraphy not only to increase the saving of telegraph wire still further, but also to reduce the labour cost of translation and writing by the use of suitable machines. The simplicity of these machines and the saving of wire cost depend on the fewness and simplicity of these signals. It is in this reduction in the number and in the simplification of signals that there will be found to lie not only the fundamental distinction between the telegraph and telephone, but also the fundamental criterion of all telegraph systems—What number and what kind of signals per letter do they require ? For a given wire, the fewer the signals the more the business that can be passed over it ; and for a given amount of business the fewer the signals the cheaper the wire that can be used. And with the increase of distance the cost of the wire becomes all-important. Pupin self-inductance coils are being applied to help the telephone, but they are equally applicable to the telegraph. On the other hand, if we can do without inductance coils so much the better ; and the way to achieve that result is to use as few signals per letter as possible. Hence nothing can alter the fundamental criterion—What number and what kind of signals per letter does the system use ? And so far as the class of work dealt with by telegraph administrations is concerned, the telegraph systems with the best signals for long-distance work will, in the end, supplant all others.

Telegraph Signalling Alphabets.

A little consideration of the facts will show that the codes of signals or telegraphic alphabets that meet all the requirements for signalling by means of an electric current through a telegraph wire are comparatively few in number. If we first regard the matter from a metaphysical point of view, it will be found that all signals have a Space aspect and a Time aspect. Signals in which the meaning depends on the space aspect may be conveniently described as space signals. In the broad sense these are telegraphic, and they appeal to the eye. Signals in which the meaning depends on the time aspect may be conveniently described as time signals. In the broad sense these are telephonic, and appeal to the ear. For instance, a signboard may extend over 10 feet and 100 years ; but the intelligence conveyed does not depend on the duration of the signboard. It is a space signal. On the other hand a Morse signal in a wire may extend over half a second and 500 miles. In this case it is the time aspect which is significant. It is a time signal. In a printing telegraph the space signals forming the written message have to be converted into time signals, in order to be sent over the telegraph wire. The wire signals must occupy various positions in time, because, for commercial reasons, the wire cannot occupy various positions in space. That is to say, there must be only one wire.

At the receiving end of the wire the time signals have to be again converted into space signals if they are to reach the brain of the recipient through his eyes, and that is a necessity, because the average citizen does not know Morse. It is the time signals in the wire that it is so important to economise.

It is not possible in this paper to go fully into all the possible kinds and best forms of telegraph signals, but the following summary will be sufficient to preserve the thread of the argument.

Unlike telephonic signals, all telegraphic signals are built up out of definite units (Morse dots or half-waves). It is not possible to signal telegraphically by varying the duration of these units. Nor is it desirable to signal by varying the amplitude of the units, as that introduces the weakness of the quadruplex. If weakness is to be avoided,

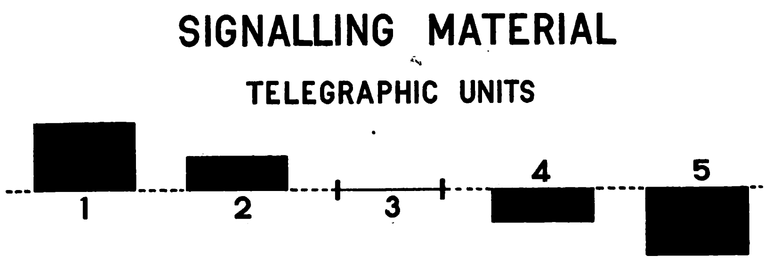


FIG. 1.

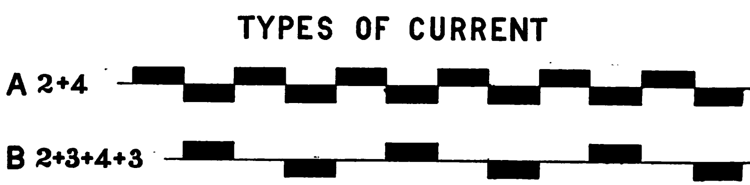


FIG. 2.

the meaning of the signals must not depend on varying amplitude or duration of the units, but only on varying permutations of the units. Fig. 1 shows the only units ever used in practical telegraphy.

In regard to units 1 and 5, it is obvious that bad weather is always a critical time with aërial telegraph wires, and, from a business point of view, a telegraph system employing the quadruplex method of two variations of current strength that can always be relied upon to fail during bad weather—thereby doubling the trouble—is disastrous. Two variations of current strength are also undesirable for underground cables, the inductive trouble between neighbouring wires being severe with the strong current needed. Quadruplex types of current are therefore clearly not suitable for a first-class printing

telegraph, and they are useless for any telegraph system that has to work over long distances.

As for the zero unit 3, one of the worst evils with which telegraph engineers have to contend is the inductive interference between neighbouring wires, leading to the mutilation, and sometimes to the obliteration of signals. Experience has shown that the best remedy is always to keep sufficient current flowing in the wire to preserve a positive or a negative value in spite of the parasitic currents. This at once rules out the zero unit on long lines with a number of wires, or in multiple-wire cables—that is to say, on busy lines where a printing telegraph would find its chief employment. Thus units 1, 3, and 5 are excluded, and A (Fig. 2) is the only reliable type of current for a high-speed printing telegraph on overland lines or multiple-wire cables. It is this type of current that is employed in the Baudot, Murray, Rowland, and Buckingham systems, and it is the ordinary "double-current" of the British telegraph service. This one familiar fact, that alternating current must be used, sweeps into oblivion at least three-fourths of the printing telegraphs invented during the past sixty years.

For single-wire ocean cables, where there is no inductive trouble from neighbouring wires, the zero unit can be employed, and as only short impulses are permissible, B (Fig. 2) is the only type of current that is used in ocean-cable work.

There remains the question whether the shape of the signalling units is important from the point of view of machine telegraphy. About ten years ago there was a brisk discussion in some of the electrical journals in regard to the advantages of the simple harmonic curve or sine wave for the transmission of power by the alternating current. A group of American scientific men, headed by Professor Rowland, who contended that the sine wave was the best, took it for granted that the best method of transmitting energy was also the best method of transmitting intelligence, and advocated the use of the sine wave in telegraphy. Certainly if Smith wants to make Jones spin round like a dancing dervish, the best way might be for Smith to transmit sine waves to Jones; but in practice, Smith always wants to make Jones perform an excessively complicated and irregular series of motions, and for this purpose it is essential to transmit similar motions by introducing upper harmonics in a fragmentary, non-periodic, and very irregular way. Even if it were possible to signal by means of sine waves, there is against the sine wave arrangement the practical disadvantage that special machinery would be required to generate the sine waves, and also to repeat them, as an ordinary repeater would at once send them on as square signals. So far as undulatory signals are concerned, there is, of course, nothing to prevent them being applied to any system of machine telegraphy if desired.

A and B (Fig. 2) being the sole permissible types of current, it only remains to build up alphabets or codes of signals by introducing irregularities or patterns by various substitutions of units. Space will not permit of more than a few of the alphabets of practical importance being referred to. The simplest of all these alphabets is that employed in the Hughes printing telegraph. It is as follows :—

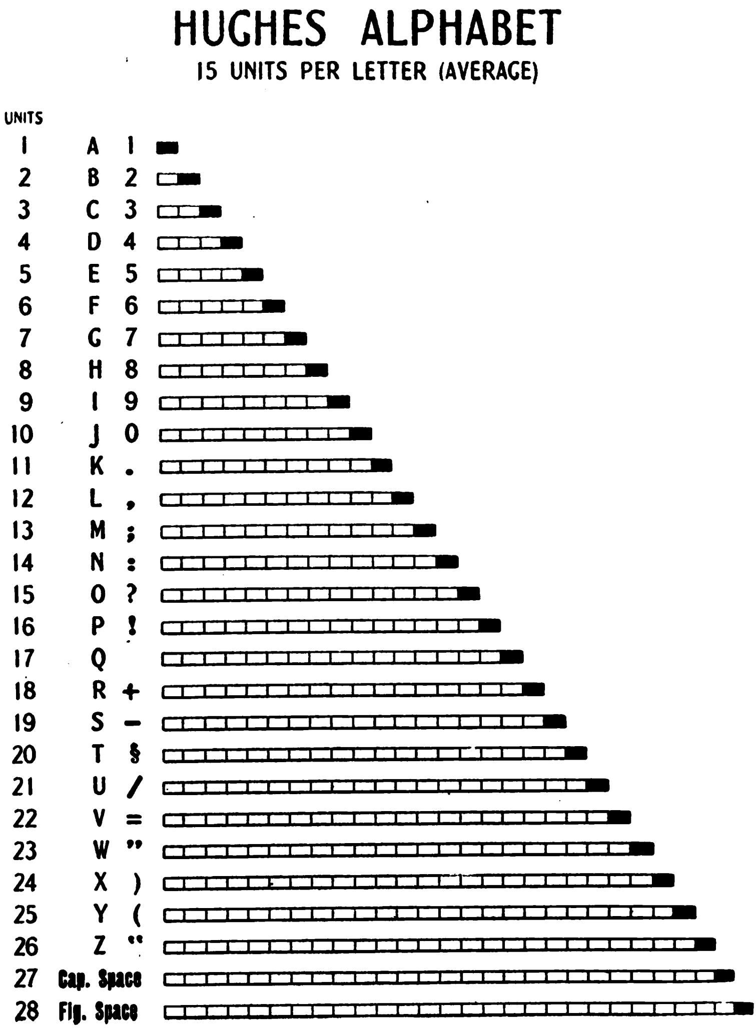


FIG. 3.

On the average there are about 15 units per letter in the Hughes alphabet. There is only one current unit, but there are no less than 14 zero units.

The number of units in the Hughes code can be reduced by using positive and negative units—"A," for instance, being a positive unit, and "B" a negative unit; but the most satisfactory way to reduce the number of units is to resort to more complex permutations. Perhaps the simplest alphabet so formed consists of the permutations of two current units in eleven different positions, with the proviso that not less

than one zero unit must intervene between two current units. This gives forty-five permutations. This alphabet is the basis of the Rowland system, and may, therefore, be appropriately called the Rowland alphabet. It is as follows:—

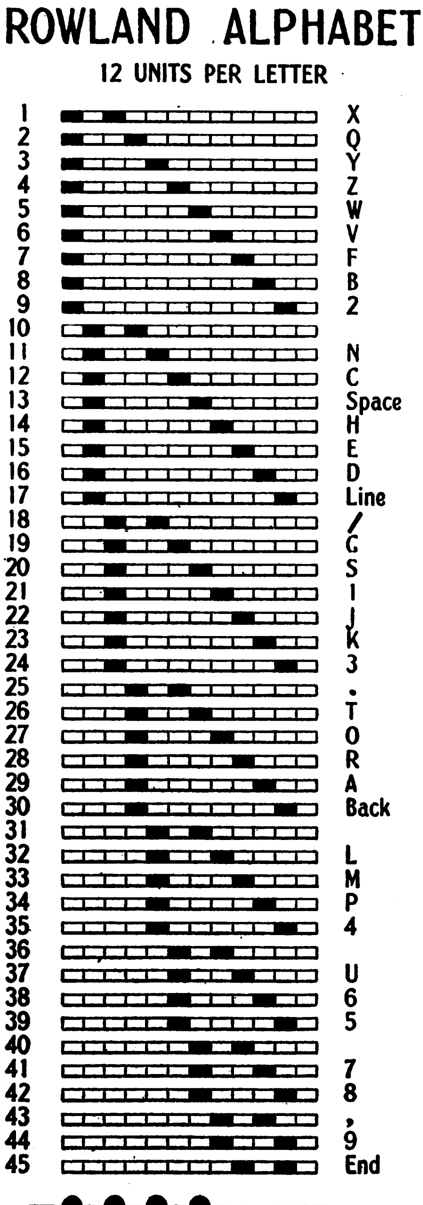


FIG. 4.

In the Rowland alphabet, as each current unit must be separated by at least one zero unit, a zero unit has to be added at the end of all the letters making it a 12-unit alphabet, and the exigencies of the Rowland

MORSE ALPHABET

8 UNITS PER LETTER (AVERAGE)

UNITS	FRE-QUENCY	
4	14,000	E
6	10,000	T
8	9000	A
6	9000	I
8	8000	N
14	8000	O
8	8000	S
10	7000	R
10	6000	H
10	5000	D
12	5000	L
10	4500	U
14	4000	C
10	3000	M
12	3000	F
12	2500	W
16	2500	Y
14	2400	P
12	2000	B
12	2000	G
12	1500	V
12	800	K
16	600	Q
16	500	J
14	500	X
14	300	Z
20	700	1
18	600	2
16	600	3
14	500	4
12	500	5
14	500	6
16	500	7
18	500	8
20	500	9
22	700	0
20	4500	,
14	3000	.
18	1000	-
18	300	?

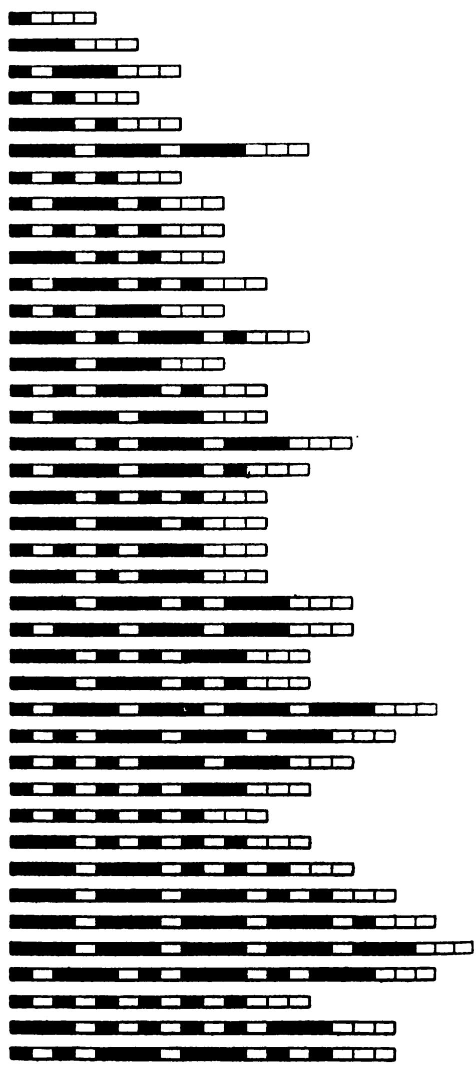


FIG. 5.

system require an extra unit, making 13 units per letter. In order to secure the advantage of "double-current" working, this alphabet, as used in the Rowland system, is to some extent masked by being built up out of an alternating current. (See foot of column in Fig. 4. This shows the 45th letter of the Rowland alphabet as actually transmitted.)

The beautiful automatic photo-printing telegraph system developed by the Siemens & Halske Company, in Berlin, is based on the same Rowland alphabet, but in this case it is not masked by the introduction of alternating current. Each letter is represented by two short impulses, one positive and one negative, in eleven different positions.

If we remove the restriction that each current unit must be separated by at least one zero unit, we can at once make better alphabets. The only condition to be observed is that not more than two different units must be employed, in order to leave the way open for the use of the alternating current. We can, for instance, build up the familiar Morse alphabet with positive and zero units, as on p. 562.

Or we may substitute negative units for zero units, and so conform to type A (Fig. 2). The only point requiring mention is that the unison device in the Morse alphabet consists of a negative or zero dash of 3 units, which is used for this purpose only. As the result of long experience with the Wheatstone, the British Post Office has found that the average Morse letter is equal to four complete reversals—that is to say, 8 units. For manual signalling of all kinds, the arrangement upon which the Morse alphabet is built is not only good, but it is practically the only arrangement possible. There are only two different time intervals, namely, 1 unit and 3 units. For manual signalling, intervals of 2 and 4 units are not sufficiently distinct from 1 and 3. With machine telegraphy, on the other hand, time can be divided with great accuracy, and the use of more than two time intervals presents no difficulty. Consequently with machine telegraphy a shorter alphabet than Morse is possible.

Further, although the Morse alphabet is ideal for manual signalling, it has a grave disadvantage for machine telegraphy. The letters are of unequal length. Only those who have made a careful study of the printing telegraph problem from a mechanical point of view, can realise the extreme importance of this question in constructing a printing telegraph. Uniformity is the key to success in machinery. It makes for simplicity. With all machinery, simplicity, other things being equal, is of prime importance, and owing to the delicacy and inherent imperfection of telegraphic machinery, the need for simplicity in this case is overwhelming. That simplicity can only be secured by a signalling alphabet in which all the letters are of equal length. It is the same necessity that has compelled the use of letters all of the same width in typewriters. The Morse alphabet, however, has been in possession of the field so long, and telegraph officials in English-speaking countries are so saturated with Morse traditions, that it would be impossible to introduce a new alphabet if the operators had to learn it. Fortunately with machine telegraphy that is not the case. All the operator has to do is to learn typewriting. As a single illustration of the mechanical advantages of an equal-letter alphabet, such as that

used in the Baudot and Murray systems, it may be mentioned that a keyboard perforating instrument to produce the Morse tape for the Wheatstone transmitter, requires a group of nineteen punches and their corresponding parts, as against only five punches and their corresponding parts in the Murray keyboard perforator. As an automatic printing telegraph requires about four keyboard perforators at each end of a circuit, if an equal letter keyboard perforator can be constructed for £20, and an unequal letter perforator costs £60, there is a difference of capital cost of about £300 per circuit in favour of the equal letter alphabet. This is a large item if many circuits have to be equipped, to say nothing of the fact that cost of maintenance is roughly proportional to capital cost.

The alphabet used in the Buckingham printing telegraph has the peculiarity that it is both equal and unequal. It uses the Morse dot and dash, but as each letter is represented by six impulses, three positive and three negative, it is not necessary to use the negative dash solely as a unison mark as in Morse. Letters are distinguished by counting every sixth impulse, which is a negative dash, as in the Morse alphabet, but the negative dash is also used for building up the letter permutations. The result is that the Buckingham is an equal-letter system at the receiving station ; but at the sending station it is involved in the complexities of an unequal-letter alphabet. The Buckingham alphabet is shown on p. 565.

The length of the letters in the Buckingham alphabet averages about 10½ units, as may be proved by counting the number of units in each letter, and then counting the letters in several sentences and averaging.

The Best Machine Telegraph Alphabet.

Unquestionably the best alphabet for machine telegraphy is that used in the Baudot and Murray systems. It is the shortest of all practicable telegraph alphabets, in fact the shortest possible, and it is an equal-letter alphabet, consisting of five units per letter. The permutations are made up out of impulses of 1, 2, 3, 4, and 5 units' duration, both positive and negative (or positive and zero). Being an equal-letter alphabet, the signals can be divided off into letters by measurement. There is, therefore, no need for a unison signal to separate the letters as in Morse. Hence there is no space between the letters. The result is that impulses frequently extend from one letter into the next, and the average number of marking impulses per letter in the Murray system is only about 1¼, and a trifle more in the Baudot. In the Rowland there are four and a half marking impulses per letter. The Rowland alphabet, therefore, sends about three and a half times as many signals per letter as the Baudot and Murray alphabet. The apparent Murray alphabet, as it appears in the perforated transmitting and receiving tapes, is shown in column B, Fig. 7, and the signals transmitted by this tape are shown in column C. Column A gives the frequency of the occurrence of the letters of the alphabet.

The apparent Baudot alphabet as it is transmitted is shown in column D.

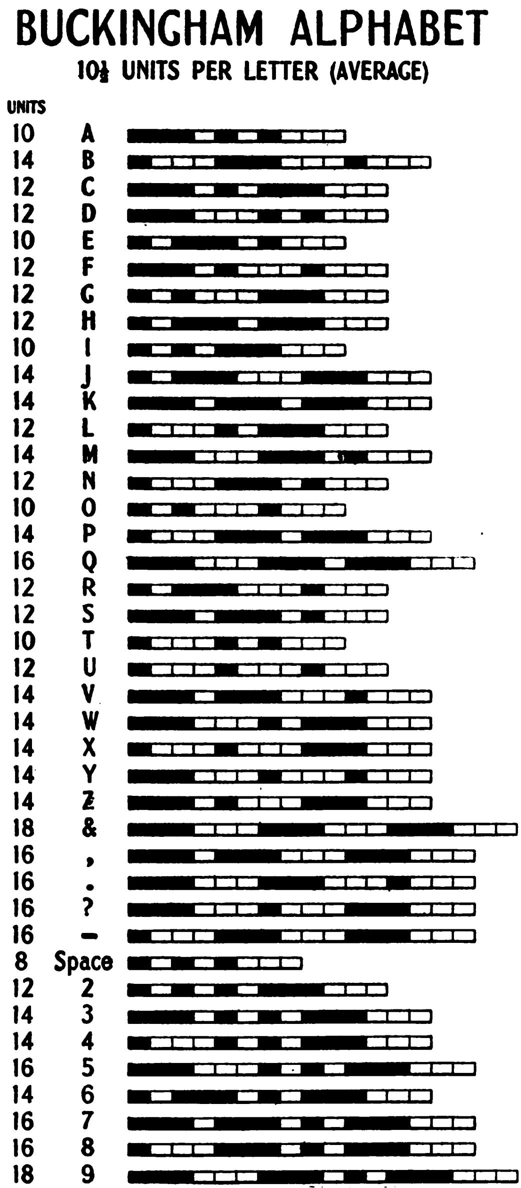

BUCKINGHAM ALPHABET
10½ UNITS PER LETTER (AVERAGE)
UNITS
10 A
14 B
12 C
12 D
10 E
12 F
12 G
12 H
10 I
14 J
14 K
12 L
14 M
12 N
10 O
14 P
16 Q
12 R
12 S
10 T
12 U
14 V
14 W
14 X
14 Y
14 Z
18 &
16 ,
16 .
16 ?
16 -
8 Space
12 2
14 3
14 4
16 5
14 6
16 7
16 8
18 9

FIG. 6.

BAUDOT & MURRAY ALPHABET

5 UNITS PER LETTER

A	B		C	D		E
14,000	E	3		E	2	1
10,000	T	5		T	!	2
9000	A	&		A	1	3
9000	I	8		I	[illegible]	4
8000	N	£		N	[illegible]	5
8000	O	9		O	5	6
8000	S	:		S	;	7
7000	R	4		R	–	8
6000	H	;		H	[illegible]	9
5000	D	–		D	0	10
5000	L	%		L	=	11
4500	U	7		U	4	12
4000	C	(		C	9	13
3000	M	?		M	)	14
3000	F	"		F	£	15
2500	W	2		W	?	16
2500	Y	6		Y	3	17
2400	P	0		P	%	18
2000	B	/		B	8	19
2000	G	'		G	7	20
1500	V	)		V	'	21
800	K	½		K	(	22
600	Q	1		Q	/	23
500	J	¼		J	6	24
500	X	¾		X	,	25
300	Z	!		Z	:	26
4500	,	,		É	&	27
3000	.	.		⁂	⁂	28
	Space			:	.	29
	Caps.			Fig. Space		30
	Figs.			Cap. Space		31
	Line			Not used		32

FIG. 7.

Actually the space between the unit signals in the Baudot is very small, and the groups of positive and the groups of negative signals run together into single signals, even in passing over a comparatively short line. The Murray alphabet is always worked with alternating current, so that the real Baudot and Murray alphabets as they actually go over a telegraph line are identical, and are shown in column E.

The only difference is in the allotment of the letters to the various permutations, the Murray arrangement being designed to punch as few holes as possible in the paper tape. Baudot, in a paper on his system in the *Bulletin de la Société Internationale des Électriciens*, vol. xi., credits the authorship of this alphabet to Wildman Whitehouse, in 1853, but the Whitehouse patent of 1853 gives only the alphabet shown in column B, Fig. 7, which was proposed by Morse and others, and is so obvious that no merit can be claimed for its use. To Emile Baudot belongs the credit of having been the first to use what I have described as the real Baudot alphabet. This is the shortest possible telegraph alphabet fulfilling the conditions already set forth, and it is the ideal alphabet for machine telegraphy. It was the natural outcome of the Baudot system. In the Murray system the instruments had to be specially designed with the express object of using this alphabet. It is the foundation of the great success achieved by the Baudot system during the past twenty years, and no system of machine telegraphy can hope for any wide and permanent success on land unless it uses this alphabet. Other alphabets, especially the Morse, will no doubt continue to be used with machine telegraphy for particular purposes, but the Baudot is the one alphabet that has any prospect of coming into general use for machine telegraphy. It bears the same relation to machine telegraphy that the Morse alphabet does to manual telegraphy. They are each without a rival in their respective spheres. Actually so far as the alphabet is concerned the Murray system has a slight advantage over the Baudot, because in the Murray system correcting impulses are generated from the signals themselves, whereas in the Baudot system two correcting units are required for every four letters, and on the average two units have to be allowed for retardation. Thus in practice there are six units per letter in the Baudot system and five in the Murray.

So far as ocean cabling is concerned, the number of possible alphabets is very limited. The Morse, as used for cable work, is shown in column A in Fig. 8 for the sake of comparison with the only equal-letter alphabet for ocean-cable work that is shorter than the Morse, namely, that shown in column B.

Cable Morse averages about 7·4 units per letter, and the equal-letter alphabet about 6·4, the advantage being about 10 per cent. in favour of the equal-letter alphabet. The use of this set of permutations (three different units in three different positions) as a telegraphic alphabet appears to have been first proposed by Cooke in 1836. So far it has not been put to any practical use, but there are interesting possibilities connected with it which will be referred to later on. It may be noted in passing that I have arranged the permutations so that fourteen of the most frequently used letters are the same as Morse.

Cooke's arrangement only provided for the twenty-six letters of the alphabet, and it does not seem to have been published.

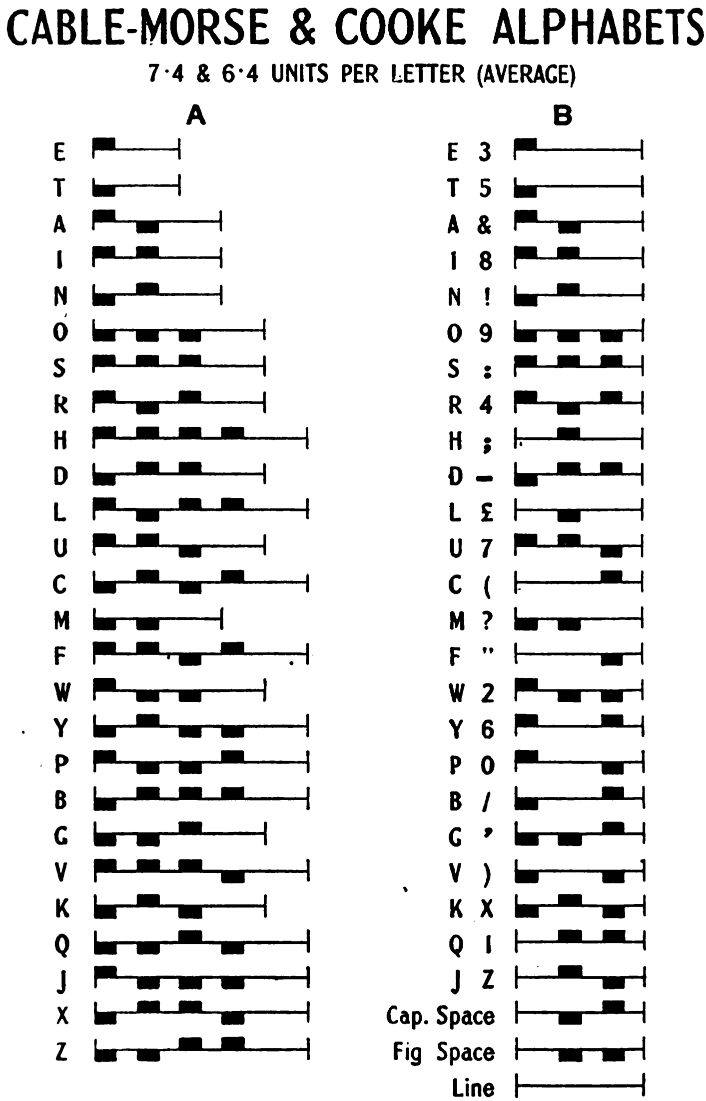


FIG. 8.

Summing up, the order of merit of the leading printing telegraph systems, from the point of view of economy of line-time by using the shortest alphabet or code of signals, may be set out as follows :—

	Five Letters.	Five Figures.
Murray ...	25 units	35 units
Baudot ...	30 „	30 „
Morse ...	40 „	74 „
Buckingham ...	50 „	74 „
Siemens & Halske ...	60 „	60 „
Rowland ...	65 „	65 „
Hughes...	75 „	75 „

With figures there is a slight difference, owing to the use of figure prefixes and suffixes in some cases and not in others. The foregoing estimate for figures is based on the figure group 13579. It will be noticed that with figures the Morse alphabet comes out badly. By way of a practical comparison the word PARIS is given in each of the alphabets, on p. 570.

SYNCHRONISM, ISOCHRONISM, AND UNISON.

Investigation having shown that the Baudot alphabet is the best, the next question is how to utilise it—and, in fact, any telegraph alphabet—as the basis of a printing telegraph system. As already explained, when there is only one wire or channel of communication, the conversion of space signals into time signals and back again may be regarded as the chief operation in printing telegraphy. If there were five wires, as at A in Fig. 10, the signals would remain space signals throughout. If, on the other hand, there is only one wire, as at B, Fig. 10, then there must be some means of putting the line successively in connection with the space signals 1, 2, 3, 4, 5, so as to collect them as time signals; and at the other end of the line there must be an arrangement to enable the wire to distribute its time signals successively to the space-signal positions 6, 7, 8, 9, 10. At each end of the line there must be an oscillation or rotation of some mechanism adapted for collecting and distributing the signals. This is one of the chief features of printing telegraphy, and therefore of all telegraphy through one channel. In the Baudot and Rowland systems we have complete rotation, a revolving contact arm sweeping the space signals successively into the telegraph line as time signals from a fixed contact-wheel and distributing them in the same way at the other end. In the automatic telegraphs the contact-wheel becomes infinitely large, that is to say, a rack, such as the Wheatstone transmitting tape. In this case, owing to the size of the wheel, it is impossible to have a revolving contact arm. Instead, the contact-arm is fixed and the wheel or tape-rack revolves. In B, Fig. 10, for instance, it makes no real difference whether the contact-arm revolves and the contacts 1, 2, 3, 4, 5 remain stationary, or *vice versâ*. The effect is the same in both cases. In nearly all printing telegraphs a revolving wheel distributor is used. In the case of the Hughes the distributor and the type-wheel are one and the same. In the stock-tickers it is a little ratchet-wheel on the same shaft as the type-wheel. In the Baudot, Rowland, and Buckingham the distributor is an entirely separate

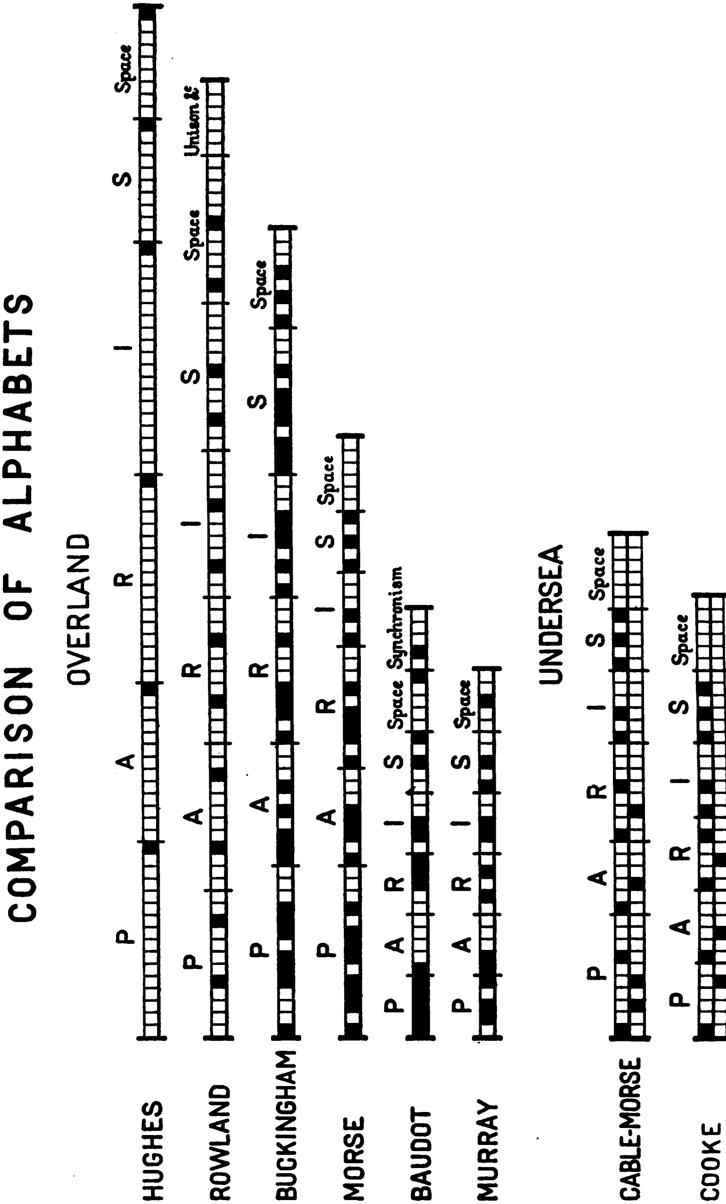

COMPARISON OF ALPHABETS
OVERLAND
HUGHES
ROWLAND
BUCKINGHAM
MORSE
BAUDOT
MURRAY
UNDERSEA
CABLE-MORSE
COOKE
P A R I S
Space
Unison &c
Synchronism

Fig. 9.

mechanism from the type-wheel, and is connected with it in a very complicated manner. In the Murray system the distributor is a punch-arm and a moving paper-tape, that is to say, a portion of a rack. In this respect it closely resembles the Wheatstone recorder, but instead of inking the tape it punches holes in it. In the method of translating and printing after distribution of the signals, the Murray system differs from all previous printing telegraphs, and it belongs rather to the class of automatic type-setting machines, such as the monotype; and for the purpose of automatic type-setting in the ordinary acceptation of the word it was indeed originally designed.

It is owing to the necessity for converting space signals into time signals and back again that all telegraph systems using one wire or

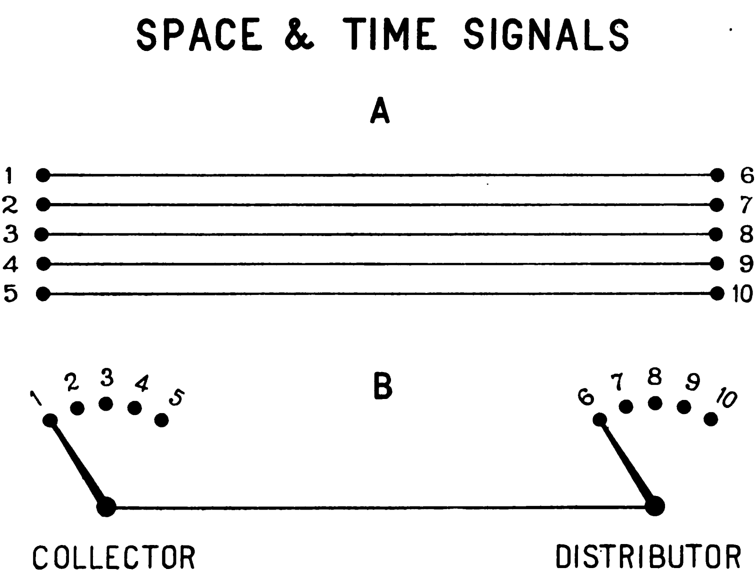


FIG. 10.

channel of communication involve "synchronism," "isochronism," and "unison." As already pointed out, if we had five wires instead of one, intelligence would be transmitted by space signals only. There would then be no need for a unison device, because each letter would stand by itself distinct, and there would be no synchronism, and only isochronism in respect of successive letters. With one wire the collector and distributor must both maintain the same speed—they must be isochronous—or the signals will get mixed up and lost. For instance, the sounder must work at exactly the same speed as the Morse key, though there may be considerable lag. There must also be a unison mark to distinguish the letters, the negative dash being used for this purpose in the Morse alphabet. The terms "unison," "synchronism," and "isochronism" are often very loosely used, but in telegraphy—

"Unison" is the method adopted for correctly dividing off or distinguishing the groups of letter signals.

"Isochronism" means identical speed of two bodies at a distance from one another.

"Synchronism" means identical speed and phase of two bodies at a distance from one another.

It is a physical impossibility to secure synchronism between a driving and a driven body, and in telegraphy all so-called synchronous systems are really only approximations to synchronism, and the more precise the synchronism required, the worse the system. Isochronism on the other hand is not only easy to secure, but may be made perfect.

In Fig. 11, if we take a shaft, D, with three wheels, A, B, C, keyed to it, and if B is the driving-wheel, then the speed of all three wheels

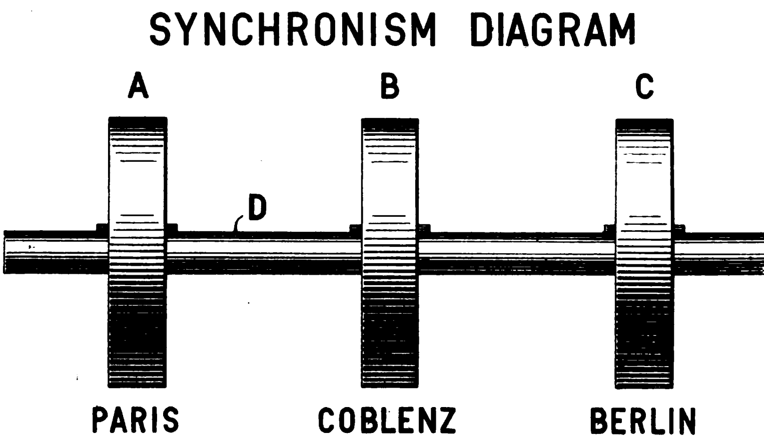


FIG. 11.

must be identical or the shaft will break. That is to say, the isochronism is perfect, but the synchronism is not. The friction of the air on A will twist the shaft D and cause A to lag behind B. If D is a large steel shaft a few inches long, the lag will be infinitesimal, but if D is a telegraph wire 500 miles long, the lag becomes a serious factor. Although it is physically impossible to secure synchronism between a driving and a driven wheel, B and A, it is possible to secure synchronism between two driven wheels, A and C. The Baudot is usually worked as a synchronous system, and Fig. 11 is the method adopted in a Baudot repeating station. For instance, in the quadruple Baudot working between Paris and Berlin there is a repeating station at Coblenz, which controls the speed and phase of the instruments in Paris and Berlin. Coblenz is the driving-wheel B, and A and C are Paris and Berlin. This clever scheme secures perfect synchronism over 700 miles (1,100 kilometres) separating the two capitals, but unfortunately it is a thing that can only be done once, and in any case

it does not really get rid of lag. The retardation of the signals has still to be allowed for. Hence there is a physical limit of say 500 miles (800 kilometres) to even approximate synchronism in telegraphy. Abandoning synchronism and relying simply on isochronism, the phase may differ to any extent, and the limitation of distance imposed by synchronism is at once removed. The Baudot has to abandon synchronism in order to perform its greatest feats. To get six transmissions over one wire between Paris and Marseilles, 550 miles (880 kilometres), two wires are used, one to send six messages simultaneously to Marseilles, and the other for six return messages to Paris. In this way lag or retardation is of no consequence as long as the signals remain clear, isochronism or identical speed being all that is required. Similarly, in working through 560 miles (900 kilometres) of single-wire ocean cable between Marseilles and Algiers, two transmissions of thirty words per minute each are secured in one direction through one cable, and another cable is used for the return messages. Between Marseilles and Algiers the difference of phase on the Baudot instruments amounts to about 90 degrees. The Murray system makes no attempt to secure synchronism. It is an isochronous system, and the duplex balance is used to secure working in both directions on one wire, the Murray system, like the Wheatstone, working very easily in this manner. Retardation under these circumstances ceases to be important, because the distributor need only keep step with the arriving signals, which might, if necessary, take five minutes on the journey. This arrangement has also the advantage that ordinary repeating stations can be used.

I would, therefore, prefer to describe as synchronous a telegraph system that endeavours to obtain sufficient identity of phase between the collector and distributor, to be able to work in both directions over one wire without resorting to the duplex balance. In this case the collector and distributor are identical in form, and can interchange their functions at will. A system relying on identical speed only, and the duplex balance for working both ways on one wire, is what, I think, should be described as an isochronous system.

What has been said about translation, collection, and distribution of signals may be summarised in the following chain of operations, forming the essential features of all printing telegraph systems :—

1. Message recorded in Roman or other type, or script handed in for transmission.
2. Translation into telegraphic space signals.
3. Collection of the telegraphic space signals in the form of time signals.
4. Transmission of the time signals over a single telegraph wire.
5. Distribution of the time signals in the form of space signals.
6. Translation of the telegraph space signals into Roman or other type characters.
7. Recording of the Roman or other type characters in the form of a message for delivery.

Although this chain of operations is complete in itself, something more is required for a high-speed printing telegraph. With a good telegraph alphabet, such as the Baudot, the capacity of a telegraph line for transmitting messages is so great that one operator cannot keep it fully occupied. If the line is to be completely utilised, it must, therefore, be given in turn to several operators. This involves some species of rotating mechanism. Hence the employment of several operators to send signals in rotation is only an extension of a process already in use. In Fig. 10, at B, it merely involves extending the number of space-

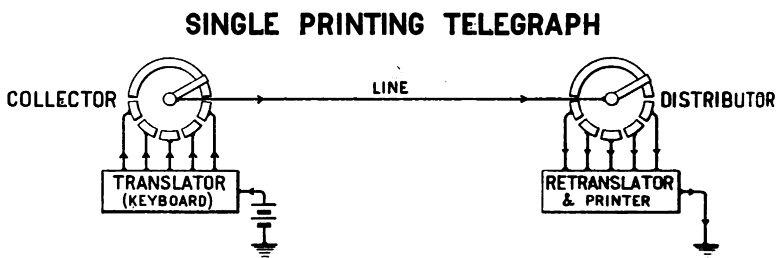


FIG. 12.

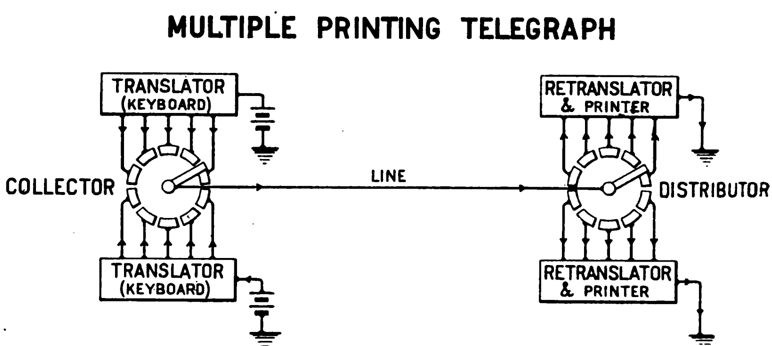


FIG. 13.

signal contacts at each end of the line. The employment of several operators is therefore easily and naturally provided for.

The operators may take turn about in transmitting (1) letters, or (2) messages. The first is the multiple method, of which the Baudot is the best example; and the second is the automatic method, of which the best-known form is the Wheatstone. The Murray system is simply a fully developed Wheatstone system using the Baudot alphabet. Each of these methods—the multiple and the automatic—has advantages and disadvantages which it is not possible to discuss here, beyond saying that the two methods appear to be in a sense complementary to one another, each being capable of doing work for which the other is not so well suited.

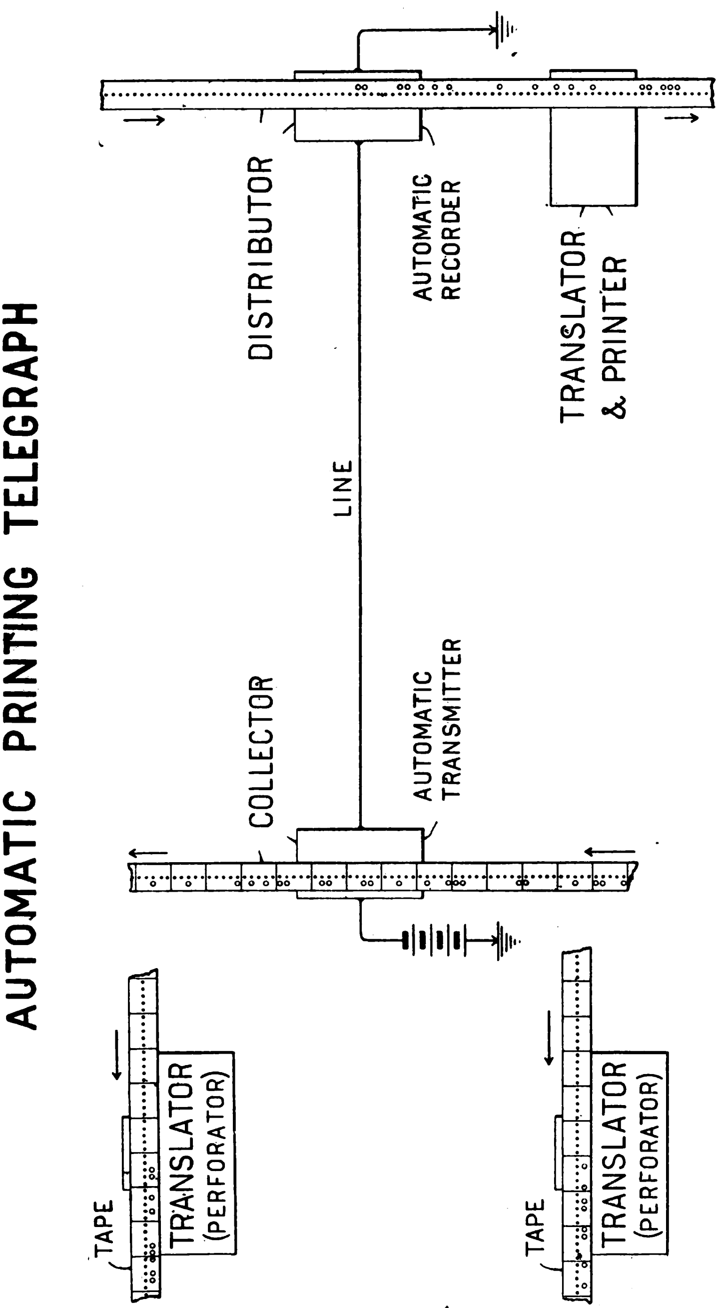

AUTOMATIC PRINTING TELEGRAPH
DISTRIBUTOR
AUTOMATIC RECORDER
TRANSLATOR & PRINTER
LINE
COLLECTOR
AUTOMATIC TRANSMITTER
TAPE
TRANSLATOR (PERFORATOR)
TAPE
TRANSLATOR (PERFORATOR)

FIG. 14.

Figs. 12, 13, and 14 illustrate these and other points mentioned. Fig. 12 is a diagram of a single printing telegraph for one operator. Fig. 13 shows the multiple method by which several operators transmit single letters turn about. Fig. 14 shows the automatic method for transmitting messages turn about. It will be seen that the multiple and automatic methods differ only in degree and not in kind.

THE MURRAY AUTOMATIC SYSTEM.

The Murray automatic system may be taken as an example of the practical application of the principles that have been described.

The Murray Tape, and the Correction of Errors.

As already explained, the collector and distributor in the Murray system each consist of an oscillating arm or lever, and a moving band of telegraph tape about half an inch wide, the messages being recorded

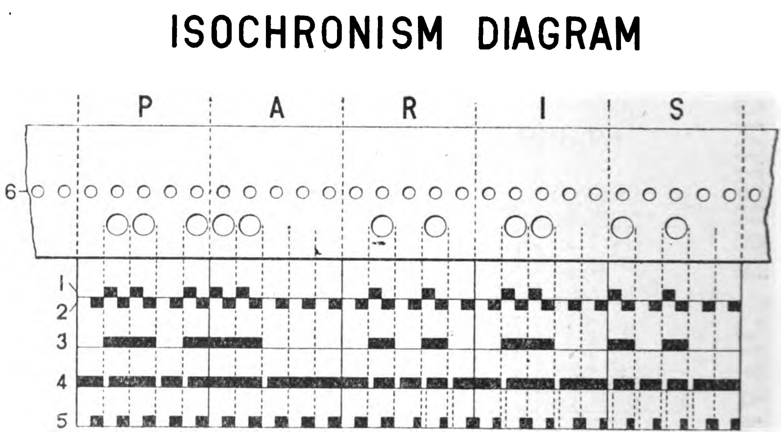


Fig. 15.

as perforations in the tape. The oscillation of the lever is a subsidiary feature arising out of the employment of tapping instead of sliding contacts. Relatively to the direction of motion of the tape, the lever or arm is stationary. Formerly the transmitting and receiving tapes were unlike, but they have now been made identical. Fig. 15 shows a piece of this tape perforated with holes representing the word "PARIS," while beneath it, on line 3, are shown the main-line time signals which this tape transmits, or which produce this tape at the receiving station. For the sake of clearness the main-line signals are shown as made up of positive and zero units. The system can be worked in this way, but it is preferable, as already explained, to use positive and negative units (double-current), zero positions being filled with negative units. Each letter permutation occupies half an inch lengthwise of the

tape, each letter space being divided up into five equal units. Any one or more of these units may be perforated with a message hole. This gives 32 permutations. Following the example of the Hughes and Baudot, one of these groups is used as a figure shift and another as a letter-shift, and there are about 52 type characters. It is possible to use 84 characters, but the additional complexity is undesirable, and 52 characters have been found ample for all requirements. The central row of feed-holes in the tape 6, Fig. 15, is punched beforehand at an immense speed by pulling the tape through between a punch-wheel and a die-wheel. The same method is now used for perforating sheets of postage stamps, and it can be carried out on a manufacturing scale as soon as a demand arises.

While describing the tape, an important point in regard to automatic printing telegraphs may be explained. So far as commercial telegrams are concerned, a serious objection to direct-transmitting page-printing telegraphs is that errors show up badly in the printed message, and operators have to work slowly and carefully to avoid making mistakes. Telegraph operators are remarkably accurate, but in spite of the best care and skill, errors are frequent. Any one who is accustomed to using a typewriter knows how difficult it is to write a page of type-written matter without striking at least one or two wrong letters. In four cases out of five there is consciousness of the error as soon as it is made, and steps can be taken to correct it. In tape-printing telegraphs, like the Hughes and Baudot, the errors and the succeeding corrections can be cut out of the tape before it is pasted on to the telegraph blank, and there is then nothing to disfigure the telegram, and make the recipient uneasy about its accuracy. With a direct-transmitting page-printing telegraph this cannot be done. A wrong key depressed is an error printed at the other end of the line, and the correction following only magnifies the blot. All automatic systems of telegraphic transmission in which there is preparation of the message beforehand, possess the potentiality of correcting an error before transmission. In the Wheatstone automatic system, for instance, this process is known as a "rub-out." When an operator at a Wheatstone puncher makes a mistake he can pull the tape back and punch it full of holes, thereby obliterating the mistake. In this case the mistake is represented by a series of dots on the received tape, and the transcribing clerk passes over it. In automatic telegraph systems, in which a printing machine takes the place of the receiving operator, it is a comparatively simple matter to arrange that the printer shall imitate the action of the operator by remaining inactive when dots only are coming over the line. In this way no trace of the error appears in the printed message. This, for instance, is done in the Buckingham system. In the Buckingham system, however, as with Morse, the groups of letter signals on the transmitting tape are of varying length. It is consequently not possible to pull back the transmitting tape letter by letter in the keyboard perforator. It has to be adjusted by hand, or pulled back unit by unit, a process necessarily involving some loss of time, and many corrections in this way would materially reduce the output of the operator. In the Buckingham system also it is necessary to rub

out everything as far as the end of the previous line. Hence, to correct a single letter a whole line may have to be rubbed out. With automatic systems like the Murray, using an alphabet in which the letter signals are all of the same length, the transmitting tape, while being prepared in the keyboard perforator, can be pulled back instantly letter by letter by depressing a lever. If an operator strikes a wrong key, all he has to do to rectify his error is to strike the back-spacing lever, the letter-shift key, and the desired letter—the work of a moment. A word of six letters can be blotted out in this way in a few seconds, and no trace of the correction, not even a blank space, will appear in the printed message at the other end of the line. This relieves the operator from the dread of striking a wrong key, the strain on his attention is less, and he can work faster without working harder. One or two automatic type-setting patents have included schemes for correcting errors in the perforated tape; but, as far as I am aware, the Buckingham was the first printing telegraph to adopt a plan for invisible correction, and the Murray system was the first, and, so far, is the only system having the power to correct instantly single letters or words.

The First Translation.

The messages for transmission are punched in the tape by keyboard perforators of a very simple character. These translate the Roman letter space signals into the space signals of the five-unit telegraph alphabet. Fig. 16 is a general view of this translating instrument, with the cover on and the lid opened like a piano, ready for operation. Fig. 17 shows the instrument with the cover removed. At the back there is a box holding a condenser to suppress the spark at the punching contact. On top of this box rests the ordinary Wheatstone tape-feed wheel. In front is the actual perforator, which includes a punch-block with five punches and an electromagnet, the armature of which punches on its front stroke and feeds the tape forward one letter space on its back stroke. This magnet does the whole of the work, and there are only two electrical contacts, one a punching contact and the other a letter-counting contact to indicate the length of the lines for the page-printing. There is also a typewriter keyboard with thirty-three keys, the touch and depression of which have been made very light and short to increase the speed of operation. A simple selecting or translating mechanism completes the machine. On the right may be seen the lever for pulling the tape back letter by letter when a correction is necessary. The correction is made by striking the letter-shift key (Caps.). This punches five holes in a letter space, and so blots out all other permutations which may have been previously punched. At the other end of the line this signal leaves the printer unaffected, its function being to shift the action of the printer from figures to letters. The signal may be repeated half a dozen times so as to blot out a whole word, and there will be no trace of the error in the printed message. As the tape is divided off by printed cross-lines into letter spaces, it

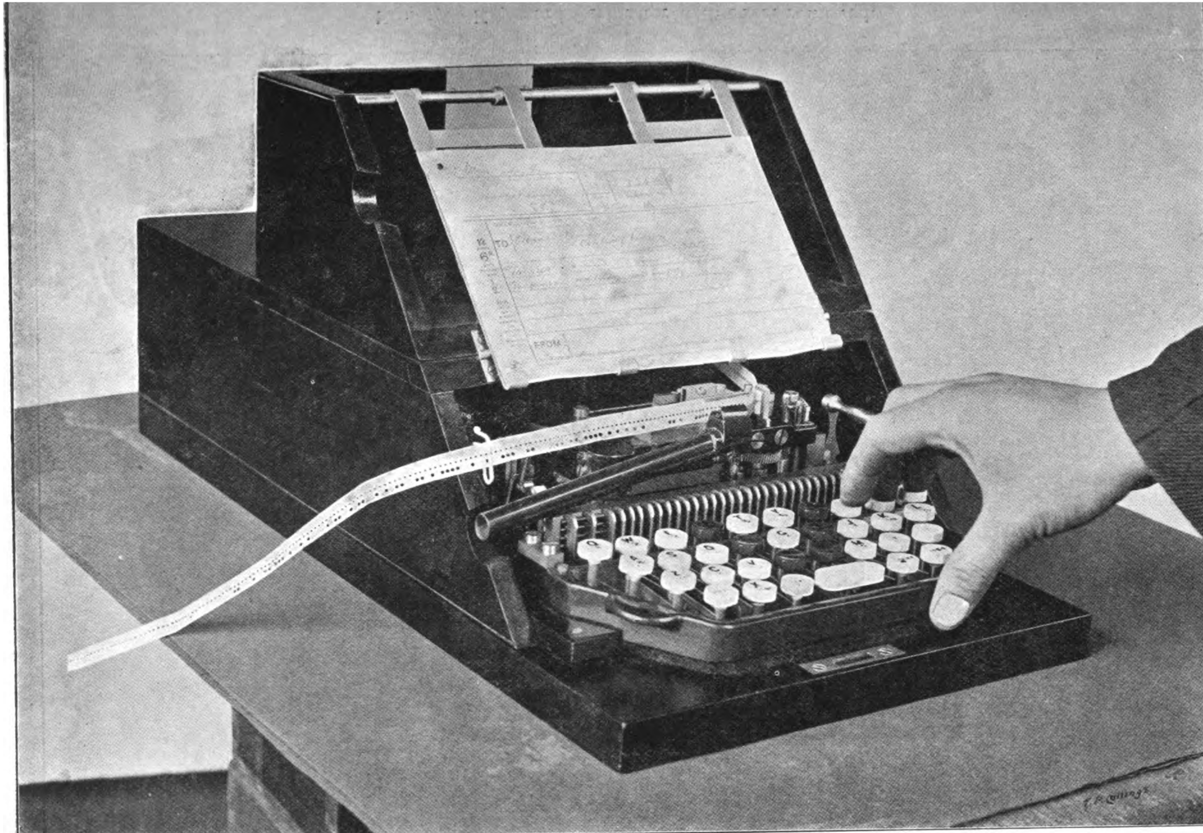


FIG. 16.—Murray Keyboard Perforator ready for use.

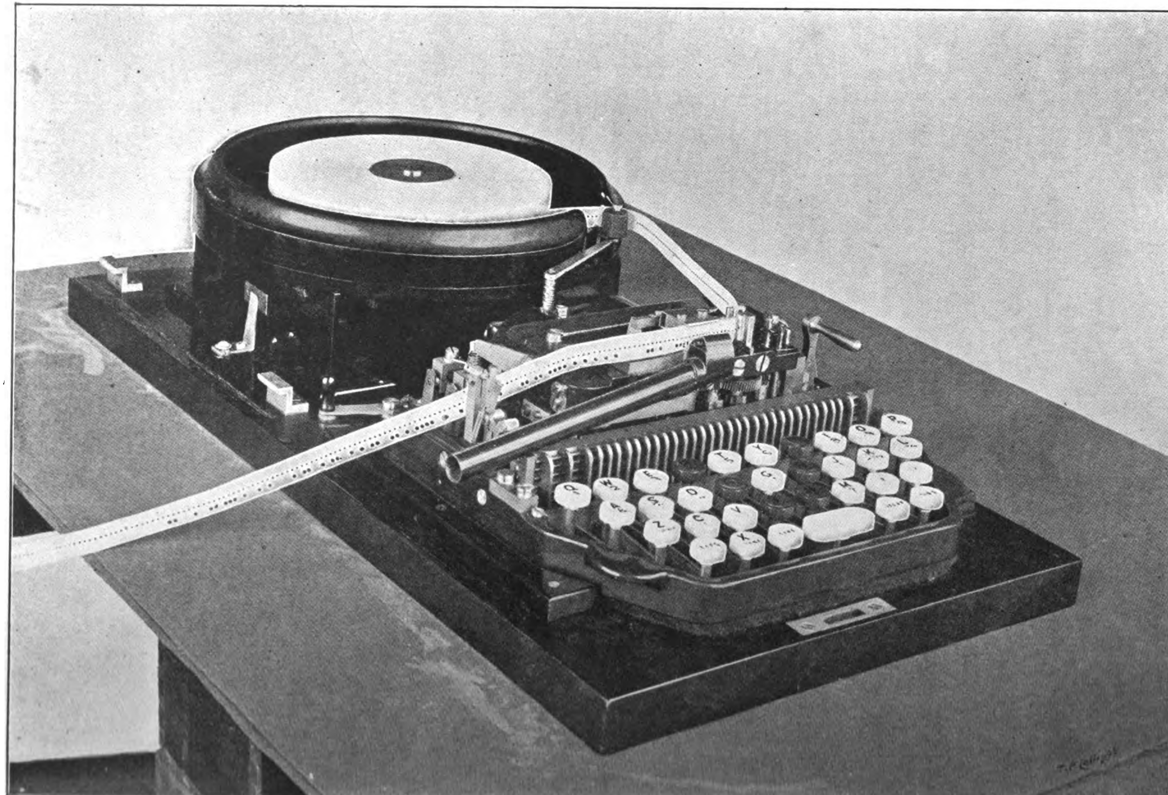


FIG. 17.—Keyboard Perforator with cover removed.

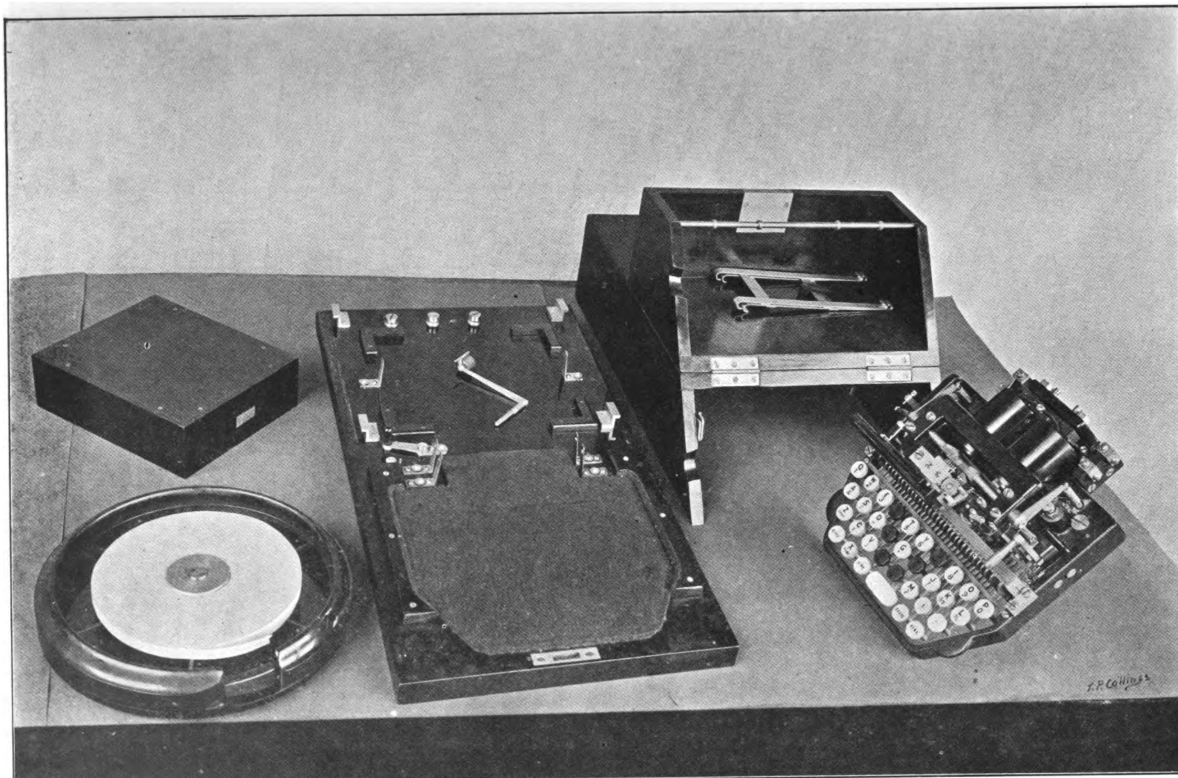


FIG. 18.—Keyboard Perforator lifted apart.

is practically as clear as print, and an operator can learn to read it in a week. It is not necessary to read the tape, but as it is easily learnt the perforator has been so designed that the perforated tape is visible up to the last letter punched just above the keyboard and below the message being perforated. In this way, what Wildman Whitehouse, in his remarkable patents of 1853 and 1854, describes as a "home record" is supplied for those telegraph administrations that require it.

The keyboard perforators can be constructed to work on any telegraphic voltage, or on a 110 or 220 volt electric-light circuit, and the current consumption is small—about one-third of an ampere during the momentary depression of each key.

Speed of Operation on a Keyboard.

So many keyboard machines for producing perforated tape have been made during the past twenty or thirty years, that it is unnecessary to go into further details, beyond saying that the whole machine and any part of it have been made instantly removable and interchangeable. There are no screws, or terminals, or wires to be unfastened. If an instrument or any part of it is out of order, it is simply lifted away and another put in its place. Fig. 18 shows the parts separately. The key buttons are in rows of different colours and of slightly different heights to facilitate operation without taking the eyes off the copy—an essential condition for high speed on a typewriter keyboard. This mode of working is not so difficult as might be supposed, particularly when the operator is trained from the first to write in this way. A young telegraph operator in Berlin learned to write on one of the Murray keyboards in a fortnight, at the rate of 120 letters (20 words) per minute without looking at the keyboard, and this operator, after several months' practice, reached a speed of 436 letters (72 words) per minute in the same way. This is an exceptional case, and the operator was accustomed to piano playing, but it illustrates the possibilities of this method. The Hughes is habitually operated without taking the eyes off the copy, and in America the linotype operators have, during the past year or two, made a notable increase in their speed by adopting the same plan, though the linotype keyboard is very large and contains ninety keys.

It may be mentioned in passing that there is much misconception about the speed of manual operation of keys and keyboards. In reality it is much below what is generally supposed. It is a subject of great importance to telegraph administrations, and it has special bearings on the printing telegraph problem; but there is not space in this paper to do more than point out that the average speed on a typewriter keyboard is not more than about 120 letters (20 words) per minute. Experiments with typewriter-keyboard instruments during the past year or two in England, Germany, and the United States have been very disappointing to telegraph engineers, owing to the exaggerated ideas that had previously prevailed about the advantages of such an arrangement. The remedy, as already mentioned, is

to train the operators to write without looking at the keyboard. It is possible that in this way the average speed may reach about 180 letters (30 words) per minute, or about double the average speed of a good Morse key operator. As the Murray system uses the five-unit alphabet, the five permutation keys of the Baudot system may be substituted for the present typewriter keyboard in the perforator. This would have the drawback that the operator would have to learn the permutations, the operator and not the machine then doing the translation; but it would have the advantage that the operator could keep his eyes on the telegram as easily as with the Morse key. In fact two sets of these permutation keys can be used, one set for the five fingers of each hand, to be worked alternately. But with properly trained operators the typewriter keyboard is undoubtedly the best.

Collection and Transmission.

The perforated tape, containing a batch of three or four messages, having been prepared, the next step is automatic transmission, that is to say collection of the space signals and transmission as time signals. There are two well-known methods of automatic transmission of telegraph signals from a paper tape. The first is the direct method, in which metallic brushes make electrical contact directly through the holes in the tape. The second is the indirect method employed in the Wheatstone automatic transmitter. This utilises the principle of the Jacquard loom, the signals being transmitted by the intervention of a number of small levers and rods controlled by the perforated tape. Experience has shown that the indirect method of transmission is more reliable than the direct method, and it has the additional practical advantage that by adjustment of the levers and contacts the relative duration of the positive and negative units can be regulated with great nicety. Another advantage of the indirect method of transmission is that the mechanism for perforating the transmitting tape is simpler and quicker than that required for making tape for direct transmission by contact brushes.

The object of the Murray single-line transmitter is to produce, from a series of adjoining but separate holes (1, Fig. 19) in a tape, not a series of short dots like 6, but a continuous telegraphic signal 3, such as would be produced by a contact brush passing over the slit or elongated perforation 2, equal in length to all the holes 1. The mechanism has also to break or reverse the current 4 when one or more unperforated units of tape 5 intervene, and to maintain this zero or reversed current in the line until one or more perforated units of tape occur. It is a process of integrating or producing a continuous signal from a series of discontinuous holes. The object is to reduce as far as possible the number of signals transmitted over the line by making the instrument transmit the real Baudot alphabet. If it were not for this arrangement the number of signals that would have to be transmitted to produce the Murray tape would be nearly double what it is now. It will be

seen presently that at the receiving station there are instruments which exactly reverse this process. They differentiate or split up the wholesale signals into their retail units, thereby producing at the receiving station a replica of the transmitting tape.

As there is only one row of message-holes in the tape, the Murray transmitter, in its improved form, is a considerable departure from the Wheatstone model. In the Wheatstone the signals depend on the relative positions of message-holes arranged in two rows. In the new Murray transmitter the signals depend on the relative positions of perforated and non-perforated units of tape. The mechanism is shown in Fig. 20 (collector). The tape and the transmitter together form the collector. There is the usual Wheatstone star-wheel 15 to feed the tape forward by means of the central row of holes, rack and pinion fashion;

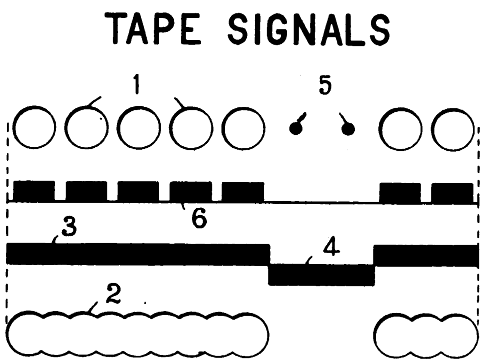


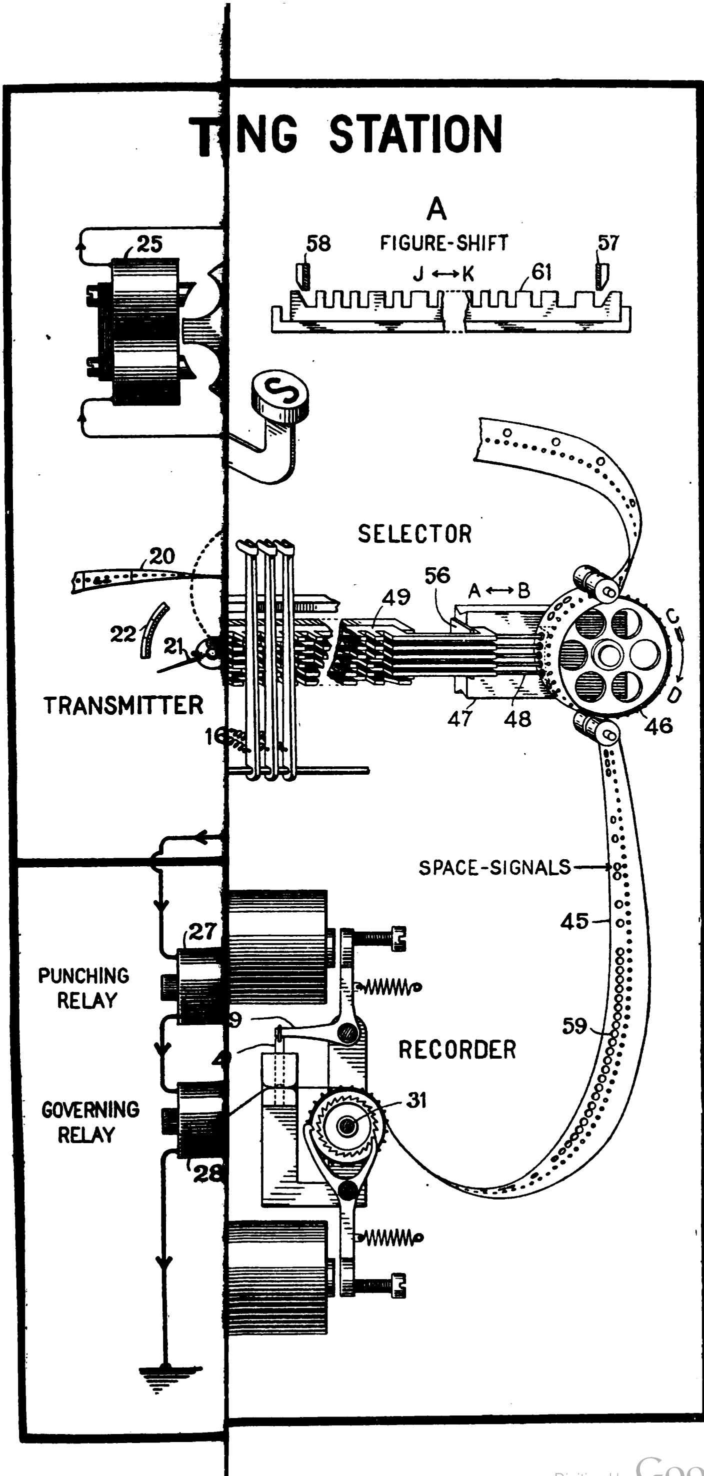
FIG. 19.

but in place of the two small upright rods used in the Wheatstone for entering the holes in the tape, there is a single rod 1. This rod is pivoted at one end of a horizontal thrust-lever 2. This thrust-lever is in its turn pivoted near its middle on a vertical lever 3, oscillating on the centre, 4. This vertical lever is kept oscillating steadily by a small eccentric wheel 5 and connecting rod 6, of the same kind as that employed in the Wheatstone transmitter. The thrust-lever 2 has a tooth 7 on its under side, which rests against a pin 8 in the vertical lever 3. The result of the oscillations of the vertical lever 3 is to cause the horizontal thrust-lever 2 to oscillate also. The free end 9 of this thrust-lever is placed close to the two free ends of two converging thrust-bars 10 and 11, which are pivoted one on each side of the pivot 12, of an ordinary Wheatstone contact lever 13. These converging thrust-bars are lightly pressed against a friction roller 14 by two small steel wire springs. The usual Wheatstone jockey roller 17 ensures the contact lever 13 making firm contact and remaining either on contact 18 or on contact 19.

When the machine is in operation, and tape perforated with a message is being fed through in the ordinary Wheatstone manner, the

small steel rod 1 reciprocates regularly against the paper tape 20, as may be seen from the dotted arc 16 described from the centre 4. It makes one reciprocation for each unit of the tape, the eccentric wheel 5 making ten revolutions for one revolution of the star-wheel 15, the spindles of the two wheels being connected by ten to one gearing as shown by the large dotted circle. If there is a message hole in the tape, the tip of the rod enters the hole and the complete motion of the rod is unobstructed. This permits the thrust-lever 2 to move as if it were one piece with the vertical lever 3, and the oscillation causes its free end to strike the free end of the lower thrust-bar 11, connected with the contact lever below the pivot 12. This throws the contact lever over on to the marking contact 18. If at the next oscillation the small upright rod again enters a hole in the tape, the same action is repeated, but the contact lever being already against the marking contact, and held there by the jockey roller 17, no change is made in its position, and marking current continues to be sent out to line. No further repetition of message-holes in the paper tape makes any change in the position of the contact lever; but if a non-perforated unit of tape intervenes, then the small rod 1 is obstructed by the tape, and the oscillation of the vertical lever 3 causes the rod 1 to depress its end of the thrust-lever 2, thereby raising the other end 9 of the thrust-lever, which then, in the course of the oscillation, strikes the upper thrust-bar 10. This throws the contact lever over on to the spacing contact 19, thereby breaking the marking current and throwing spacing current into the line. If in the next oscillation another non-perforated unit of tape intervenes, the same action is repeated, but as the contact lever is already resting on the spacing contact, no change in its position takes place, and spacing current continues to be sent out until a perforation occurs in the tape. This, by the action already explained, throws the contact lever over again on to the marking contact. It is always the first of a series of perforated units or the first of a series of non-perforated units of tape that operates to throw the contact lever over from one contact to the other. By this means the space signals punched in the tape are converted into time signals of the real Baudot alphabet.

There are a number of details connected with the transmitter, but the only one worth mentioning is the method of getting rid of bias—that is to say, the method of ensuring that the positive and negative half-waves or units shall be both of the same length. In the Wheatstone transmitter, bias is shown by the galvanometer needle inclining to the right or left according as the positive or the negative half-waves are longer, when reversals are sent. The contacts are then adjusted till the bias ceases to show on the galvanometer. The best that can be said for this arrangement is that it is rough and ready. In the Murray transmitter a more precise and scientific method is used. The eccentric wheel 5, Fig. 20, makes one complete revolution for each half-wave, positive or negative. A small steel pointer 21 is inserted in the eccentric wheel, and on the frame of the machine there is a short scale 22. If the transmitter is driven very slowly by hand the galvanometer needle will be observed to come to zero at the moment when the



T NG STATION
A
58
FIGURE-SHIFT
57
J ↔ K
61
25
S
SELECTOR
20
56
49
A ↔ B
C
22
21
TRANSMITTER
47
48
46
D
16
SPACE-SIGNALS
45
27
PUNCHING RELAY
59
RECORDER
31
GOVERNING RELAY
28

positive or the negative contact is broken. If the pointer points to the same division on the scale for a positive break as for a negative break, then there is no bias, and the positive half-wave is the same length as the negative half-wave. If, on the other hand, the pointer is alternately in advance and in retard for each half-wave or unit, then bias exists, and the contacts have to be adjusted till it disappears. As each half-wave or unit is equal in length to the circumference of the circle described by the tip of the pointer 21 it will be seen that bias is not only shown on a magnified scale, but is exactly measured. Absence of bias —that is to say, correct timing of the signals, is important in all systems of machine telegraphy, including even such simple apparatus as the Wheatstone automatic, and the indicator just described leaves nothing to be desired as far as the Murray system is concerned.

Fig. 21 is a general view of the transmitter. At the back may be seen the rectangular frame of a La Cour phonic wheel motor that drives the transmitter. This motor is more clearly shown in the general diagram of the system, Fig. 20. A vibrating reed 23 sends electrical impulses alternately to the two motor magnets 24 and 25, and these keep the notched armature 26 in steady rotation. This armature 26 drives the star-wheel 15 and eccentric wheel 5. The arrangement is in reality an electrical pendulum with the action reversed, the pendulum driving the escapement wheel, the two magnets being the two pallets of the escapement which pull the escapement wheel 26 round step by step, though the inertia of the wheel is sufficient to ensure uniform motion. The vibrating reed being isochronous in the acoustical sense, the speed of the motor is the same for all amplitudes of vibration. That is to say, variation of current strength has no effect on the speed of the motor. The speed is remarkably uniform, and the motor is ideal for the purpose and gives no trouble.

This single-line transmitter has several advantages over the Wheatstone instrument, the chief one being that there is a direct thrust against the contact lever from the driving mechanism, independent of the strength of the paper tape. Hence this thrust can be made strong in order to give very firm contacts.

THE DISTRIBUTING MECHANISM.

The distributor at the receiving station consists of two relays, a vibrating reed, and a recorder. In practice the main-line signals pass through an ordinary polarised relay to earth, this relay operating two local relays; but for the sake of clearness the main-line signals are shown passing direct through the two local relays 27 and 28, Fig. 20, to earth. It is by means of these two relays that the rest of the distributing mechanism is controlled. The recorder, which punches the message-holes in the receiving tape, is also shown in Fig. 20. This recorder consists of a punching magnet and a spacing magnet. The punching magnet operates a punching lever 29 and a punch 30. This punch corresponds with the small rod 1 in the transmitter. The punch reproduces the motions of the rod, and thereby reproduces the perforations in the tape. On short circuit the punch and the rod may

be seen to be working precisely in step with each other. The spacing magnet feeds the tape forward unit by unit by means of the escapement and star-wheel on the motor-driven spindle 31. In order to secure the alternating action of these two magnets, the contacts which close their respective circuits are arranged one on each side, 32 and 33, of a vibrating reed 34, driven by a magnet 35. As the reed vibrates, short impulses are sent alternately to the punching and the spacing magnet. When there are no signals passing in the main line, a special device (not shown) cuts out the spacing magnet by opening the switch 60, and so stops the tape; but as long as a message is passing the vibratory action of the spacing magnet is continuous, and the tape is fed forward at a fixed speed. The punching magnet works alternately with the spacing magnet, but only when there is marking current in the main line, the punching relay 27 controlling the circuit of the punching magnet. The oscillations of the tongue of relay 27 and of the vibrating reed must therefore be not only isochronous, but also synchronous. Between sending station and receiving station isochronism only is needed, but between the receiving instruments on the same table synchronism is essential. This is the case not only with printing telegraphs, but with all machines. The parts must move synchronously or the machine will not work. Fortunately, practical synchronism, when it is a question of a few inches and not of miles, is very easy of attainment. In the Murray system it is achieved in the following manner, which also of course secures the needful isochronism between the two stations.

The Method of Securing Isochronism.

The reed 34, kept vibrating by the magnet 35, beats against buffer springs 36 and 37. These limit the amplitude of vibration, and, as the energy imparted to the reed must find some outlet, the frequency is increased. Without buffer springs the amplitude of vibration varies in proportion to the strength of the current through the reed magnet, but the frequency is wonderfully uniform. The reed is then isochronous in the acoustical sense. With a fixed amplitude determined by the buffer springs the frequency varies with the strength of the current. The reed is no longer isochronous. Its speed becomes very sensitive to variations of current, and can therefore be controlled by controlling the current to the reed magnet. If the circuit of the reed magnet 35 is traced from the battery 38, it will be found that it has two break-points—one at the contact of the tongue 39 of the governing relay 28, and the other at the reed contact 40. The punching and governing relays are identical in all respects, and they are driven by the same power, so that their synchronism is perfect. (They are driven wheels only a few inches apart.) The tongue 39, therefore, moves in exactly the same time as tongue 41. Hence as long as tongue 39 oscillates synchronously with the reed, tongue 41 will do the same. In fact, the two relays might be replaced by one relay with two tongues. The back and front stops, 42 and 43, of the governing relay are electrically united, so that at the beginning and end of a main-line

signal when the tongue of the governing relay crosses from one contact to the other, there is a momentary break in the reed magnet circuit during the time of the transit of the tongue. This transit time can be varied to a considerable extent by adjusting the position of the contact stops. Opening the contacts increases the governing effect and closing them diminishes it. The reed magnet 35 breaks its own circuit at 40 in the usual vibratory way. If the reed 34 is oscillating in synchronism with the relay tongue 39, then full vibratory impulses will flow through magnet 35, as the two contacts at 39 and 40 will be opened and closed simultaneously. But if the reed tends to go faster than the arriving signals, then the two contacts 39 and 40 no longer open and close together, but more or less alternately, and the current impulses in the magnet 35 are more or less clipped. This reduction in the supply of energy to the reed at once reduces its speed till the contacts are again simultaneous. In practice the receiving vibrator is set to run about 2 or 3 per cent. faster than the arriving signals, and the governing action of the two interfering break-points in the same circuit results in the establishment of a steady dynamic balance between the accelerating tendency of the reed and the retarding tendency of the arriving signals. The effect is exactly the same as that produced by two taps in a water-pipe. If both are opened and closed together the water flows through in gushes; but if they are opened and closed alternately no water gets through, and more or less water gets through accordingly as the taps are opened and closed more or less alternately. By this means the relay tongue 41 not only vibrates at the same speed as the reed 34, but preserves the same phase. That is to say, tongue 41 touches contact 44 at the same time as the reed touches contact 33, even when the reed is making seventy-five vibrations per second (150 words per minute). But the tongue 41 does not move for every vibration of the reed. For instance, if there is a 4-unit marking signal coming over the main line, then the tongue 41 closes on its punching contact 44 for an interval of 4 units' duration, and the reed then makes four vibrations, touching the contact 33 four times, and in this way punching four successive holes in the tape by the alternate actions of the punching and spacing magnets. The transit intervals of the tongue of the governing relay 28 might be said to be like the teeth of an old saw. Some are there and some are not, but such as remain are all at unitary distances from one another (see line 4, Fig. 15). So also with the signals, and there are always enough signals to govern. Actually one correction per letter is sufficient, but on the average there are not less than two and a half, because each main-line signal gives two corrections, one at the beginning and the other at the end of the signal. As a matter of fact the governing action may be stopped during the arrival of as many as six or eight letters without upsetting the isochronism, and if a line disturbance or other cause throws the reed out of step it comes in again within five letter spaces—that is to say in half a second at a hundred words a minute. The reed is only in step as long as signals are coming over the line, and it goes out of step at the end of a batch of messages, but

five space signals at the beginning of a new batch of messages bring it in again ready to record the messages. By this arrangement the necessity for sending correcting impulses over the main line to secure isochronism is avoided, the correcting impulses being generated locally from the arriving letter signals themselves. In this respect the Murray system has followed the excellent example set by the Hughes.

The isochronism diagram, Fig. 15, illustrates the co-operation of the current impulses. It shows a section of receiving tape punched with the word "PARIS." Beneath are the current impulses, indicated by black bars. Line 1 shows the impulses given to the punching magnet by the successive contacts of the reed 34 with the contact 33. Line 2 shows the uninterrupted succession of impulses to the spacing magnet from the reed contact 32. Line 3 shows the main-line signals. This illustrates the great saving of line signals effected by the special arrangements of the collecting and distributing mechanisms. Without these special arrangements it would be necessary to transmit over the main-line the signals shown in lines 1 and 2, that is to say, about five times as many signals as are now required. Line 4 shows the gaps in the circuit of the reed magnet 35 made by the crossing from contact to contact of the tongue 39 of the governing relay. Line 5 shows the impulses generated by the reed contact 40. As long as these impulses do not get into step with the gaps in line 4 they are not interfered with; but directly they do they get clipped, as shown for the sake of illustration in the second half of line 4. The speed at the two stations can, of course, be varied by altering the position and size of the weights on the vibrating reeds at the two stations.

A speed of no less than 900 letters (150 words) per minute in one direction has been reached with this recording mechanism, at which speed the punching magnet has to punch seventy-five holes per second, and the punching and spacing of the receiving tape have been perfectly performed at 960 letters (160 words) per minute, or eighty holes per second. Even this is not the limit, and it is possible that the speed might be forced up to even 1,200 letters (200 words) per minute. As half the time is required for spacing, the punching of eighty holes per second means that the punching magnet operates in $\frac{1}{160}$th of a second. As it takes a pressure of about 2 lbs. to force the punch through the paper, the punching magnet can not only operate with considerable speed, but also with considerable power. For ordinary commercial telegraphy there is no necessity for such high speed, and it is probable that, if the Murray system comes into general use in preference to other systems, the speeds required will be from about 240 letters (40 words) up to about 720 letters (120 words) per minute.

The transmitted signals having been correctly distributed along the receiving tape 45, Fig. 20, by the recording mechanism just described, the tape may then be used in its turn for again transmitting the signals over other circuits, or it may be run through the printer to translate the signals into printed letters. As the signals when retransmitted are sent on in as perfect shape as from the original transmitting station, it is possible to transmit messages in this way at a high speed to any distance overland—from London to Calcutta, if need be. Owing to

the power to retransmit from the received tape, it is also possible to transmit messages from any number of stations A, B, C, to station D, and to sort out the messages there and retransmit automatically in accordance with the addresses to any stations E, F, G, any one or more of which may again retransmit messages requiring to be sent on still further.

The Second Translation and Printing.

If the tape is not required for retransmission, then the tape signals have to be translated into Roman letters and printed. The essential features of the Murray printer are shown in the general diagram, Fig. 20. The tape is fed along letter by letter (five units at a time) by a star-wheel 46, carried on a shuttle 47, which is kept reciprocating in the directions shown by the double arrow A–B, by means of a cam, each reciprocation rotating the star-wheel one letter space as shown by the arrow C–D. The shuttle carries a die-plate coinciding with the circumference of the star-wheel, and having five holes corresponding with the message-holes in the tape. Five rods 48, fixed in the ends of five slotted combs 49, are free to enter the five holes in the die-plate if they are not obstructed by the paper tape which passes between the rods and the die-plate. On its inward stroke the shuttle with its die-plate presses the tape, with a letter group of perforations, against the ends of the five rods 48. Rods that are opposite holes in the tape pass through into the die-plate, and the positions of the corresponding combs are not altered. But in the case of non-perforated units of tape the corresponding combs are thrust back about $\frac{1}{16}$th of an inch. By this means one particular group of slots in the combs, and only one out of fifty-six, is brought into alignment so that a latch or cross-bar 50 can drop into it. Although only four latches are shown, there are fifty-six of these small levers, one for each of the fifty-six permutations or groups of slots. The latches or cross-bars are supported by a universal bar 51 just clear of the teeth of the combs. At the right moment this bar drops in the direction of the double arrow F–E and leaves all the cross-bars resting on the combs; but only one out of the fifty-six is selected by the particular aligned group of slots corresponding to any particular group of holes in the tape.

From the point of view of the theory of machines, the combs form a complicated system of fifty-six inverse permutation locks. The fifty-six cross-bars are the bolts or latches, and the universal bar 51 is a universal key. The locks are continually being closed in different ways by the tape, and the universal key 51 is continually opening the locks. The tape, however, is only an intermediary. The locks are really closed selectively by the keys of the perforators at the transmitting station, the keys of the perforators being real keys in the ordinary sense of the word, and not nominal keys like those of a typewriter. The letter signals sent over the line are an exact copy of the wards of the keys of the keyboard perforators. This intricate system of keys and locks is inevitable in all cases where complex control is needed. Amongst other instances it is used in systems of railroad interlocking gear. In

outward appearance there is no resemblance between the Baudot and the Murray telegraph systems, but the principle of the permutation locks is the same in both cases. Fig. 22 A shows the system of permutation locks in the case of the Baudot, and Fig. 22 B that employed in the Murray system. As the Baudot prints from a type-wheel, the locks are arranged round a wheel, and as the Murray prints by means of the straight row of keys of a typewriter, the locks are arranged as a rack. Fig. 23 shows an average permutation letter signal of the Baudot and Murray systems, the Morse, the Rowland, and the Hughes drawn on the same scale, and, for the sake of illustration, in the form of ordinary keys.

Let us assume that latch or cross-bar 50 in Fig. 20 has been selected. This cross-bar, under the action of its spring 52, throws a hook 53, attached to a typewriter key 54, under a striker-bar 55, which is continually oscillating in the directions shown by the double arrow G–H, and the typewriter key is in this way sharply depressed so as to print. The object of this arrangement is to secure speed and smoothness of

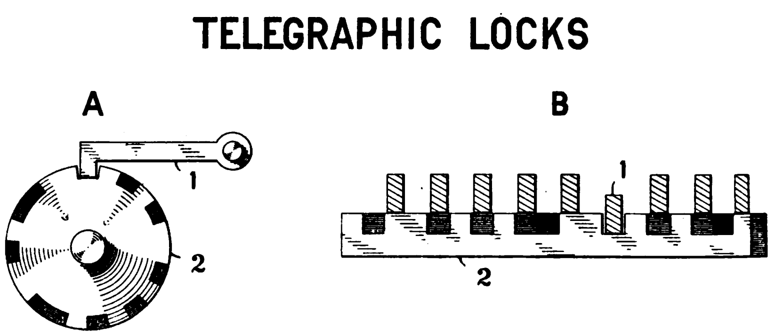


FIG. 22.

action. The moment the hook 53 is engaged by the striker-bar 55, the system of locks can be opened by the universal bar 51 and closed again with a fresh letter permutation simultaneously with the printing of the last letter. This arrangement practically doubles the speed of the printer. The shuttle on its outward stroke restores the combs to their original positions by means of the plate 56, which engages with inward projecting teeth on the overhanging ends of the combs. The cycle of operations, including the rotation at the right moment of the star-wheel 46 through one letter space, the reciprocation of the shuttle 47, the reciprocation of the universal key-bar 51, the oscillation of the striker-bar 55, and the return of the combs to zero position ready for the next letter, is performed by the usual battery of cams to be found in all automatic machines in which the cycle of motions is short and repeated millions of times. When the cycle of automatic motions includes hundreds of different successive motions and is only repeated thousands of times, we find, in place of cams, a chain of perforated cards, or similar devices, as in the Jacquard loom. When the cycle of

motions to be performed is infinite and is performed only once, we get the perforated tape of the automatic telegraph. In this case the tape only repeats when it has to influence the motions of a large number of individuals, that is to say, in the case of press dispatches. In the case of a telegram to a private individual the tape is used once only. In other words, the motions with which telegraphy has to deal are enormously more complicated than those in the most complicated known system of weaving. That is probably one explanation of the slow progress that has hitherto been made by machine telegraphy.

Although in the Murray printer there are only five combs with rods 48, there is a sixth comb 61, which in one position causes figures to

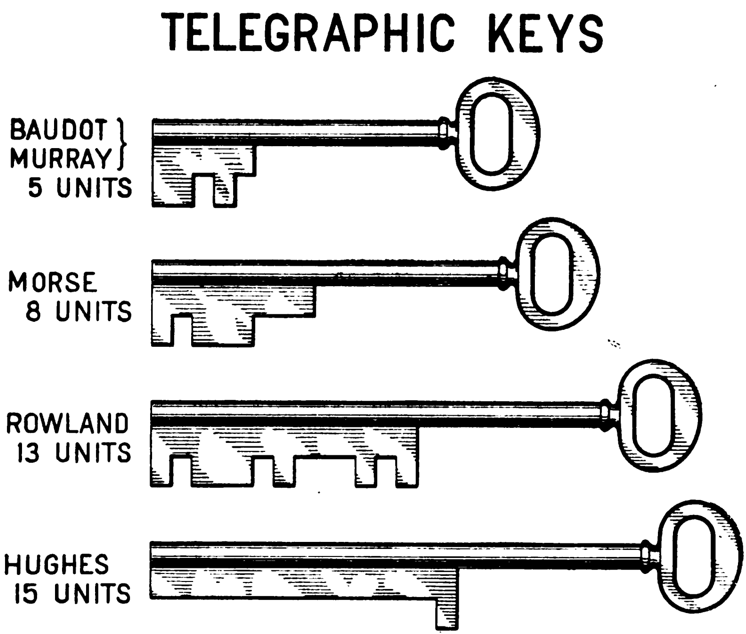


FIG. 23.

be selected and in the other position letters. It is operated by two special cross-bars, one at each end. This arrangement is shown more clearly at A, Fig. 20. At each end of this comb will be noticed a V-shaped slot, and the two cross-bars shown in section at 57 and 58 carry wedge-shaped pieces for striking the edge of these V-shaped slots. When a figure-shift signal is transmitted, it selects the figure cross-bar 58. This drops into its group of slots, strikes against the sloping edge of the figure-comb slot, and moves the comb $\frac{1}{16}$th of an inch to the left in the direction J, thereby opening the figure-locks and closing the letter-locks. A letter-shift signal succeeding a group of figures operates 57, which restores the figure-comb to its original position, in which the machine prints letters. If the letter-shift signal,

ooooo (Caps), is repeated, no further action takes place, and the printer remains idle till a letter signal comes along. Thus, in the tape at 59, Fig. 20, there is a correction consisting of five letter-shift signals, which have been used to "rub out" a wrong word of five letters. For the reason just given the printer remains idle during these five letter spaces, and there is no trace of the error in the printed message.

An interesting point is the method of securing unison. In the Murray system the question of unison is removed from the line to the tape. As with the Wheatstone recorder tape, it is a question of correctly dividing up the signals on the tape. With the five-unit alphabet each letter occupies five units, or half an inch on the tape. Hence to divide up the signals properly we have to start at the right unit and then measure 5 units at a time. The star-wheel 46,

METHOD OF SECURING UNISON

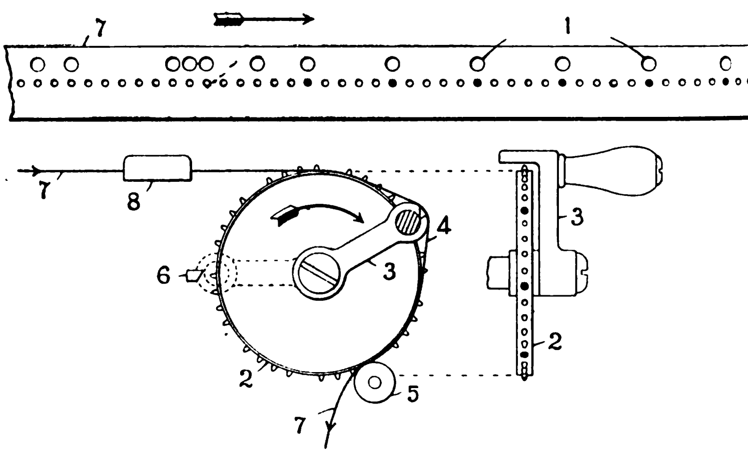


FIG. 24

Fig. 20, does the measuring, and all that is necessary is to place the tape properly on the star-wheel. To enable this to be done with facility, each batch of messages begins with four or five space signals, as shown in Fig. 24 at 1. The space signal is a perforation in the third-unit position. In the star-wheel 2 it will be noticed that every fifth tooth is omitted and the vacant tooth positions blackened. The tape 7 is inserted in the machine so that the space holes 1 come opposite the missing teeth of the star-wheel which show up black through every fifth central hole in the tape, as shown for the sake of illustration by the blackened centre holes in the tape opposite the space signals 1. The tape is then in unison. In the event of it not being in unison, it can be put in step by rotating the small unison arm 3. This bags up the tape as shown at 4, and advances the tape on the star-wheel to the

extent of one unit. A maximum of four and an average of two revolutions of the unison arm 3 brings the tape into step. The roller 5 is spring-pressed, so that it gives way and permits arm 3 to pass. The unison arm when not in use is held locked in the dotted position by the spring-tooth 6. 8 is a guide for the tape. The missing teeth in the star-wheel and the space signals show at a glance at the beginning of a message whether the tape is in step. If a line interruption causes the unison to fail in the middle of a message, two or three revolutions of the unison arm by the attendant set all right again. This does not often happen, however, as a line interruption must be complete for at least seven letters before the unison can be affected. Momentary interruptions do not disturb it. Apart from occasional line interruptions the receiving mechanism perforates the tape with surprising accuracy. On short circuit it will run for hours without making a single false perforation.

In regard to the printing of the messages on telegraph forms, there are four motions of the sheets of paper to be provided for, two horizontal and two vertical, as follows :—

Horizontal	Letter feed. Line feed.
Vertical	Column feed. Page feed.

The letter feed is already performed automatically by the typewriter. To provide for an automatic line feed, there is a small letter-counting device on the keyboard perforators. This indicates the end of a typewriter line of about seventy letters. A key is then depressed which punches a line signal in the tape. This is transmitted to the receiving station, where it operates a special key of the typewriter, which throws in a clutch, causing automatic mechanism to pull the typewriter carriage back to the beginning of the line. The same action provides the column feed up to a new line by a simple arrangement used in most modern typewriters. The page feed is more complicated and has not yet been made completely automatic in any printing telegraph, although there are several automatic page feeders giving good service with printing presses. In fact, many of the modern illustrated magazines are printed automatically from a pile of cut sheets by this means. At present, in the Murray system, the page feed is managed as follows : A stop signal is transmitted on the tape at the end of each message, and this not only runs the typewriter carriage back to the beginning of a new line, but also stops the printer. The attendant then pulls out the finished message by hand, and by a simple device it automatically tears off at the right point, leaving the next telegraph blank pulled in to the right starting point. The attendant then presses a button, which starts the printer on the next message. The process is quite rapid. Later on it may prove worth while to make the page feed completely automatic. The printer will then feed itself from a pile of cut telegraph blanks, and toss out the finished messages like tickets from a cash register. The chain of

mechanisms will be completed by an automatic folding and enveloping machine, which will fold the telegrams and thrust them into transparent envelopes and seal them so that only the addresses will show through.

The printer, two views of which are shown in Figs. 25 and 26, is now driven by a small motor consuming about 40 watts for a speed of 140 words per minute. This motor also supplies the power for automatically returning the typewriter carriage to the beginning of a new line. Fig. 27 is a general view of a set or "station" of the apparatus. Only one keyboard perforator is shown; but in practice three or four are needed.

Capacity of the Murray System.

The speed of the printer is now not less than 900 letters (150 words) per minute, and the automatic portion of the mechanism runs perfectly up to about 200 words a minute. Unfortunately, however, no typewriter yet constructed will stand the strain of more than about 720 letters (120 words) a minute in continuous service. The received tape may be divided into sections and run through two printers when the apparatus has to be worked at top speed. Under existing conditions, however, high speeds for handling commercial messages are for many reasons undesirable, and a maximum of 120 words a minute in each direction seems to meet all requirements.

In practice with the Murray system—as with all other telegraph systems from the Morse key upwards—the average output is not more than about 50 per cent. of the maximum. Time and patience will no doubt stop some of the leaks; but others, such as errors caused by line interruptions, seem to be incurable. The British Post Office has now had the Murray apparatus in use for seven hours a day for about eighteen months between London and Edinburgh, handling ordinary commercial telegrams, and the German Post Office has been giving it a prolonged trial between Berlin and Emden, and is now equipping two circuits with the improved apparatus. The Russian Post Office has also decided to give it a trial, and has ordered Murray apparatus to equip two circuits. In this case one of the conditions is that the system must transmit Latin or Russian characters at will. I understand that the experience of the British Post Office is that five operators at each end of a Murray circuit, equipped with the apparatus in its original form, can exchange about 200 telegrams an hour, and under favourable circumstances occasionally as many as 240 per hour. With the new motor-driven printer, with automatic line feed, invisible correction of errors, and other improvements, it is estimated that it will be possible for six operators at each end of a Murray circuit to exchange about 300 messages an hour. That is to say, twelve operators on one Murray circuit will be able to do the work of sixteen men on two Morse quadruplex circuits, or at lower speeds eight operators will be able to do the work of twelve. Actual tests show that the improved printer has a maximum speed of five messages a minute, so that the working

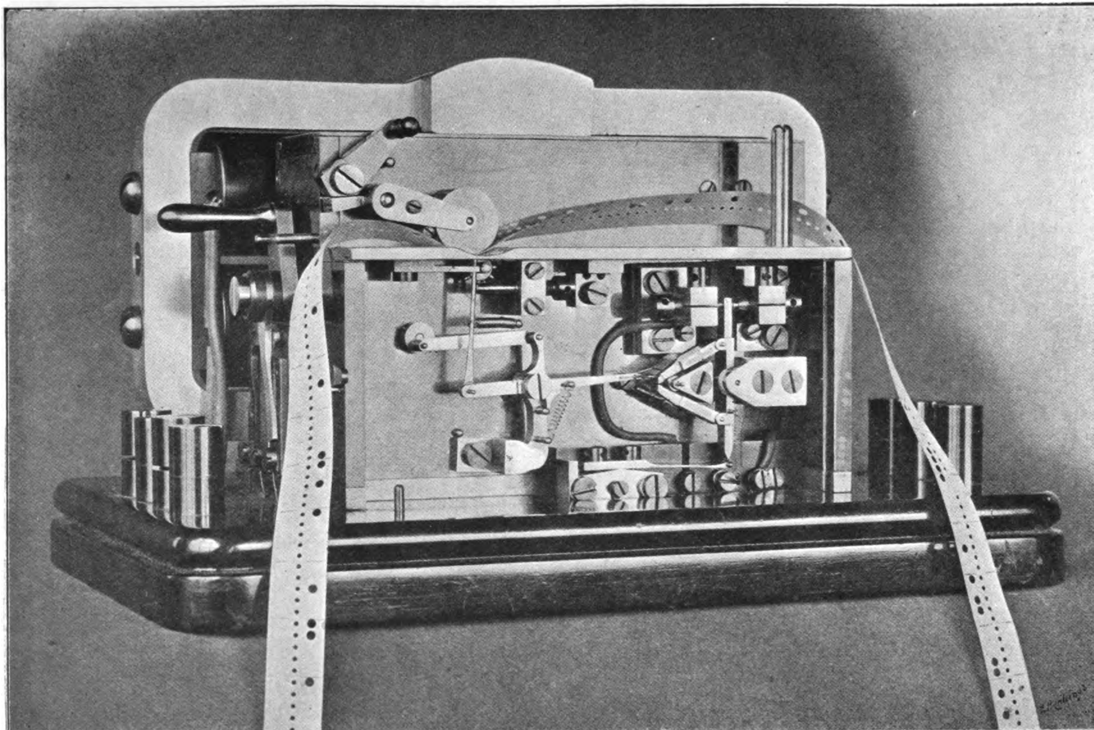


FIG. 21.—Single-line Transmitter.

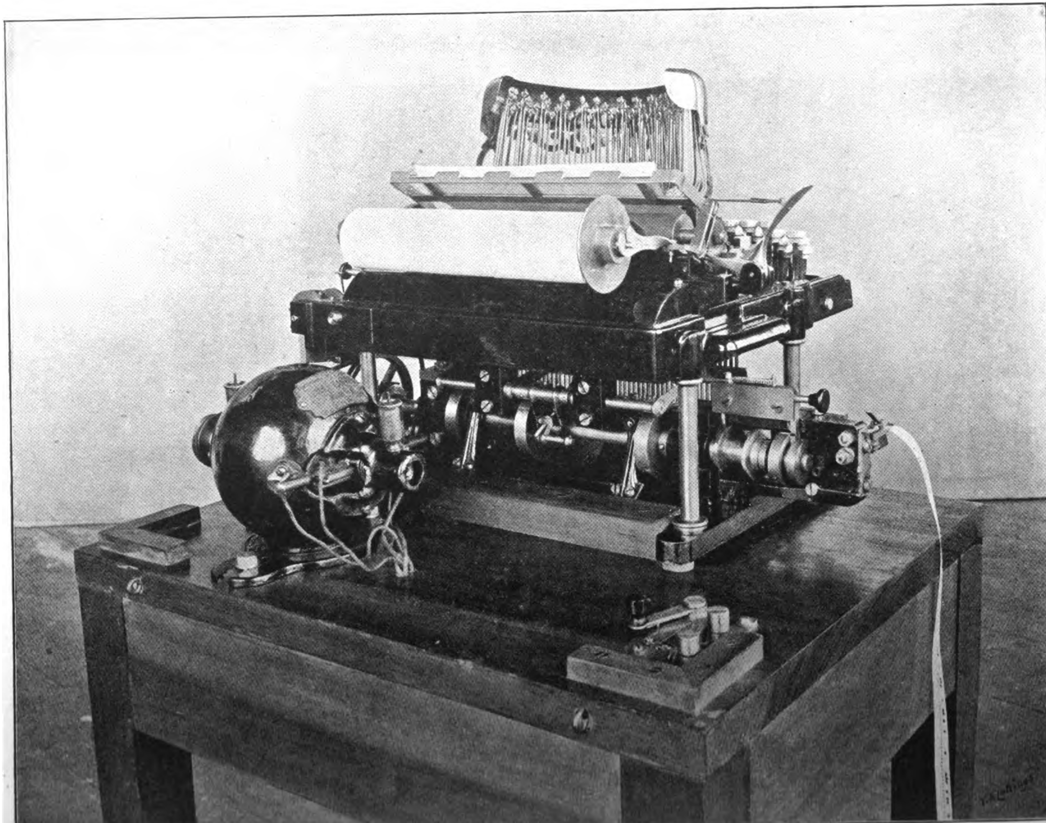
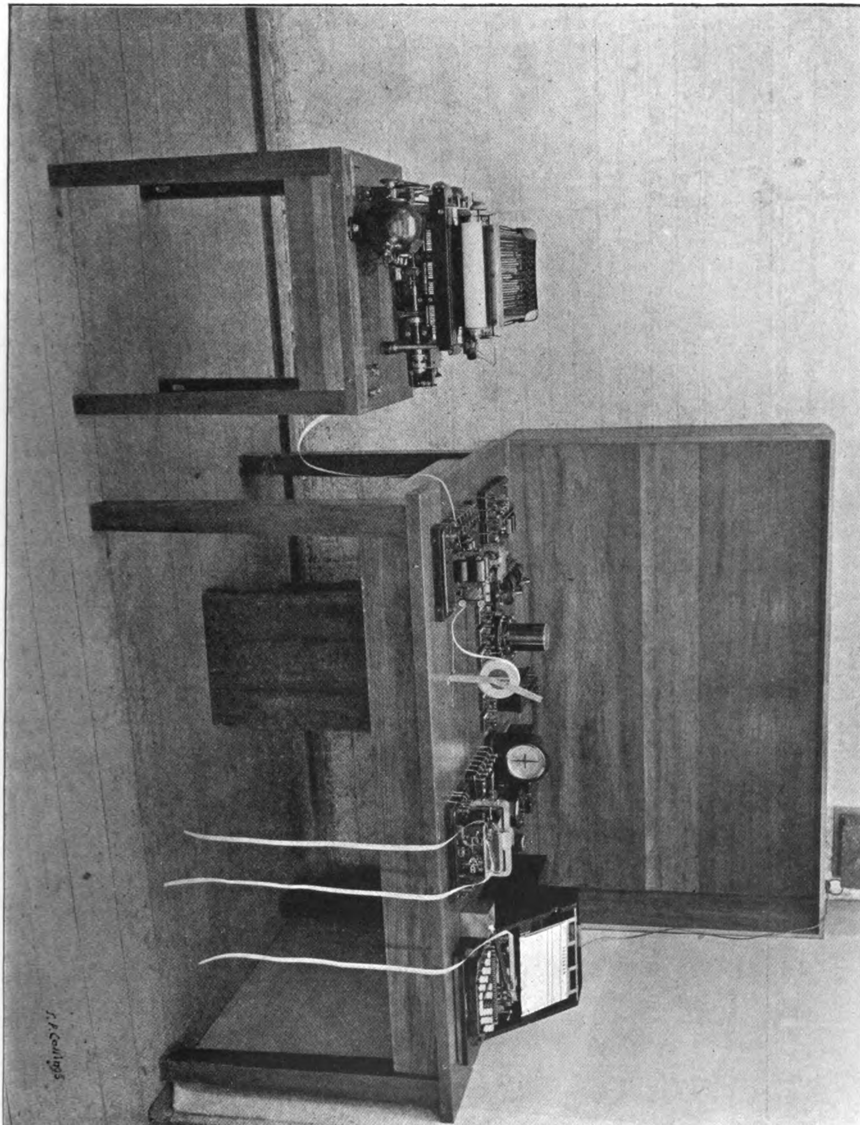


FIG. 25.—Murray Printer complete with Typewriter.



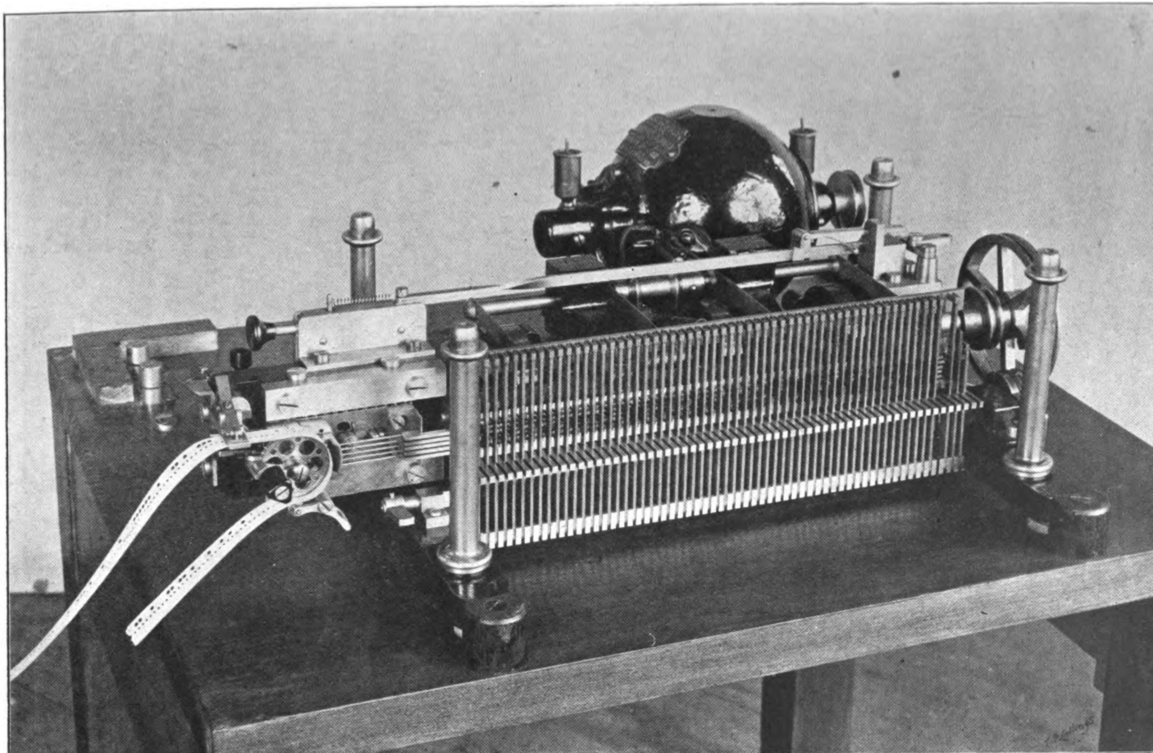


FIG. 26.—Murray Printer with Typewriter removed.

average may be safely put at 150 messages an hour. A weak point with many printing telegraph systems is durability. I understand that in this respect the Murray apparatus has proved satisfactory.

AUTOMATIC TYPESETTING BY TELEGRAPH.

Mention has already been made of the fact that the Murray printer was originally designed, not for telegraphy, but for automatic typesetting in the ordinary sense of the word, the idea, inspired by newspaper experience, having been to operate a linotype automatically by means of a perforated tape produced by a typewriter keyboard. Several machines of this kind are in successful operation, notably the Lanston Monotype. But of these inventions the Murray is the only one that has been designed on lines permitting of successful telegraphic development. In fact, in the case of the Murray apparatus the telegraphic development has been so successful that it has quite overshadowed the original typesetting idea. Three years ago the Linotype Company in New York offered to carry out the experimental work involved in applying the Murray key-selecting mechanism to the linotype, and want of time alone, owing to telegraphic developments, prevented me accepting that offer. It is obvious that it is just as easy to operate a linotype keyboard automatically by telegraph as to operate the keys of the typewriter used in the Murray printing telegraph system. No doubt this will be done in time, but the labour saving will not be very great. The cost of setting a column of news matter by the linotype is roughly about 3s. 6d. The automatic mechanism would double the speed, but, as an attendant would still be needed, there would only be a saving of labour of about 1s. 9d. per column, or a saving of only 15s. or 20s. per night even for large newspapers. This would hardly be sufficient to induce newspapers to start automatic typesetting by telegraph, but the possible saving of time is a more important feature. The saving of a few minutes is vital to newspapers at certain hours, and this may ultimately lead to automatic typesetting by telegraph ; but there are many obstacles in the way. One is the necessity for press messages being revised, punctuated, corrected, and often cut down before being set up in type. The Murray is the only automatic apparatus making provision for this difficulty by allowing the editorial corrections to be carried out by the compositor while the type is being automatically set. All that can be said about automatic typesetting by telegraph is that it is a possibility of the future, and that if it is done at all it will have to be done on the lines of the Murray apparatus, because the Murray system alone is practical both from the newspaper and from the telegraphic point of view.

TYPEWRITING ACROSS THE ATLANTIC.

Another interesting possibility, with better prospects of early achievement, is printing telegraphy in connection with ocean cables. This has been rendered possible by the Murray apparatus in conjunction

with Mr. S. G. Brown's beautiful cable relay and perforator, and the Cooke alphabet shown in Fig. 8. A sample of this alphabet, perforated in a piece of automatic cable tape, is shown in Fig. 28. As this is an equal-letter alphabet it can be perforated in cable tape by a slightly modified form of the Murray keyboard perforator already described, and this tape can be transmitted in the usual way through an automatic cable transmitter. About two years ago the Eastern Telegraph Company used some tape that I had prepared with this alphabet to transmit from Porthcurnow, and no difficulty was found in reproducing the tape at Gibraltar by means of the Brown relay and perforator. The length of the cables from Porthcurnow to Gibraltar is 1,190 nautical miles (about 2,160 kilometres), and the KR of the cable used is 2·8 millions. A slightly modified form of the Murray printer has been designed to print from this tape. Six combs are provided with rods projecting alternately above and below to engage with the message holes on both sides of the tape, and there is a seventh figure-shift comb. Not only is this arrangement perfectly practicable, and the instruments all known to work well, but it has the advantage that, as the alphabet is about 10 per cent. shorter than

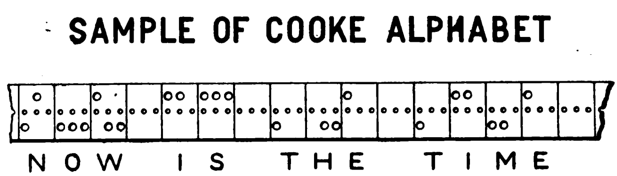


FIG. 28.

the cable Morse alphabet at present used, the carrying capacity of the cable is increased, roughly, about 10 per cent., a considerable gain when dealing with such an expensive investment as an ocean cable. It will also be noticed that fourteen of the most frequently used letters in this equal-letter alphabet are the same as Morse. Those accustomed to Morse cable tape will notice that the sample of tape in Fig. 28 can be read without serious difficulty, only one letter, H, being different from the Morse. With such a world-wide business as that of the Eastern Telegraph Company there are difficulties in the way of introducing the Cooke alphabet, but in the case of the Atlantic cables these difficulties do not exist. There is no technical obstacle in the way, and it is undoubtedly practicable with the aid of the Murray keyboard perforator, the Brown cable relay and perforator, and the Murray printer to work a typewriter in New York by playing on a keyboard in London. It is also possible by the same means to operate a linotype in a New York newspaper office by playing on a keyboard in London. For commercial reasons the possibility of automatically setting type across the Atlantic can never be of practical importance, but typewriting across the Atlantic is well within the bounds of utility, and want of time is the only reason why it has not

been attempted. In fact, there are so many possible developments of the Murray apparatus that it has been impossible for one man unassisted to attend to them all.

A Brief Comparison and a Few Words About the Cost.

Putting aside these possibilities and returning to the subject of telegraphy on land, it seems to me that the Murray automatic system is pre-eminently adapted for long-distance work, iron wires, underground cables, and press dispatches. Its power of retransmission from the received tape, its high speed, its simplicity, and its provision for instant and invisible correction of errors, appear to make its position fairly secure for the classes of work just mentioned. But for short-distance traffic the multiple principle certainly will have a number of advantages when developed so as to save labour. This is the weak point with the multiple systems at present. They do not save labour. Of these systems the Baudot is, to my mind, undoubtedly the best. Like the Murray system, it is constructed on true scientific lines that permit of further development and adaptation to the needs of various countries. It works well through cables and over considerable distances (thanks to the Baudot alphabet), but its chief limitation, in addition to printing its messages on tape, is the use of synchronism, and for isochronous working over long distances it uses two wires, while the Murray uses only one. For short distances the theoretical capacity of the Baudot is enormous, probably not less than 5,400 letters (900 words) per minute—direct type-printed messages—but in practice not more than six transmissions at 30 words a minute each are attempted, giving a maximum of 1,080 letters (180 words) per minute. It is used on all the busy circuits in France, and Paris is connected with London, Hamburg, Berlin, Vienna, Rome, Milan, and Algiers by means of Baudot apparatus. Several sets are also in use in Brazil and in French Indo-China.

A large amount of inventive talent has been devoted to the Rowland multiple system, and I have no doubt that the clever engineers connected with this interesting apparatus will continue their improvements. Being a direct-transmitting page-printer, errors cannot be corrected without showing on the printed message, and the Rowland alphabet bars it from use on underground cables and for long-distance traffic. Its potentialities, however, for handling short-distance traffic are considerable. Its maximum speed is about 1,800 letters (300 words) per minute, duplex, over moderate distances; but so far, with the Rowland, as, indeed, with all other telegraph systems, including even the Morse key, there is a large gap between promise and performance. The Rowland system was tried and rejected by the British Post Office about three years ago. Since then several sets have been on trial on the Continent.

The Buckingham automatic system cannot retransmit like the Murray, so that it cannot cover such great distances nor retransmit news so conveniently to a number of centres. It does not correct errors so efficiently, it is not so rapid, and it is much more complicated; but it works very

well over long distances of 1,000 miles (1,700 kilometres) duplex, giving a maximum speed of about 100 words a minute in each direction, and the mechanism employed is remarkably ingenious. It has been in commercial use by the Western Union Company in America for several years past, and it was tried and rejected by the British Post Office during the winter of 1903.

A very attractive scheme for high-speed printing telegraphy is the use of light and photographic paper for the printing process; but the systems employing this method have not yet got beyond the interesting stage. The Siemens & Halske photo-printing telegraph is the best up to the present, and it reaches a speed of 2,000 letters a minute in one direction, but the messages come out recorded on a damp photographic tape, an arrangement that does not seem very promising from a practical point of view.

The cost of the multiple and automatic high-speed systems of printing telegraphy may appear at first sight to be very high if we compare it with the cost of the Morse key and sounder, or with a Morse quadruplex set, and it is this comparison that telegraph administrations in English-speaking countries have habitually made, wrongly, as I believe. A Baudot sextuple outfit to equip both ends of one circuit costs, it is stated, about 20,000 francs, or, in round numbers, about £800. A complete outfit of the Murray system costs about the same at present, though it is expected that the cost of manufacture will be reduced when sets are made in numbers. In regard to the prices of the Rowland and Buckingham systems I have no information, but they can hardly be less costly than the Murray and Baudot. In any case, taking £800 as the cost of an automatic or multiple system, this is less than the cost of two linotypes, of which at least a score are to be found in most large newspaper offices. I understand that the French Post Office does not possess more than about 250 sets of the Baudot system. Many of these are only for double and triple transmission, so that even if we take the cost at as much as £300 per set (that is to say, equipment for one end of a circuit only) the total cost, apart from the cost of development, has only been somewhere about £70,000, a very small sum for such a large business as a telegraph administration to spend on special machinery that, at a cost of from £600 to £800 per circuit, will save a capital expenditure on wire, at £12 per mile, of from £2,000 to £12,000, compared with the Morse quadruplex; and that will, in the case of the Murray system, at any rate, save about 25 per cent. in labour compared with the Morse key and sounder.

Conclusion.

It has not been possible in this paper to do more than refer briefly to the special conditions to be fulfilled by successful printing telegraphs, to the relative advantages of the multiple and the automatic systems, the advantages and disadvantages of direct keyboard transmission and of indirect automatic transmission, the relative merits of the typewriter and other keyboards and keys, speeds of keys and keyboards, and the advantages of machine compared with hand

telegraphy; but there will no doubt be opportunities of dealing with these points in the technical journals at some future time. Nor has it been possible to touch upon the practical difficulties which have been encountered, chiefly in connection with page-printing. To enumerate only a few, there are special page-printing telegraph difficulties in handling short commercial telegrams, practical working difficulties arising out of line disturbances, checking, and corrections, difficulties based on the necessity for following the ordinary telegraph routine, which varies in each country, and difficulties connected with the use of suitable telegraph forms and preserving records of messages. Many of these difficulties are so small and impalpable that even men who have grown up in the telegraph profession have failed to recognise their existence until actually confronted with them. Their invisibility, however, has not made them any the less formidable, and the fact that they have been overcome is due to the cordial co-operation, advice, and assistance freely given by the telegraph officials in America, Great Britain, and Germany. The Postal Telegraph Company in New York and the Telegraph Administrations in London and Berlin have devoted to the Murray system not only time and money, but the best technical skill of their engineering staffs. And the results, I hope and believe, will prove satisfactory to all concerned.

Discussion at Meeting of March 2nd.

Mr. J. Gavey: I think we have all read Mr. Murray's paper and listened to his explanation of the working of his apparatus with a great deal of pleasure. His paper is very valuable, not merely from what it tells us, but from what it suggests. In view of the statement that he makes of the plethora of printing and writing telegraphs that have been invented and described, it will no doubt occur to members present to ask how it is that after so many years of telegraphic work the principal telegraphic administrations still use the old-fashioned Morse for the greater portion of the telegraphic business of the world. There are various reasons for that. Mr. Murray has given us some; amongst others he states that the Morse alphabet is *per se* the ideal alphabet for manual transmission. But there are other reasons which have prevented so far the general use of machine telegraphy. The telegraphic public have been educated to a high degree of speed, and they demand the same rapid rate of delivery whether a message has to be transmitted over two or three miles or over hundreds and thousands of miles. Not only does a man in Glasgow expect his message to be delivered in London as quickly as a man in Westminster expects his to be transmitted to the City, but a New York man expects the same speed to London. The result is that there is a tendency to discourage any operation that intervenes between the receipt of a message at the counter and its passage over the wire, and again at the far end between its reception and its being handed over to a delivery messenger. The effect is that most telegraphic administrations have considered the ideal system to be a manual system, involving the use of the simple Morse key, in which the messages are brought up to the instrument at the forwarding station and despatched over the wires, while at the Mr. Gavey.

Mr. Gavey. receiving station they are written off direct, packed in envelopes, and sent out without any intervening operations. It has always been recognised that although this may be an excellent system for the rapid transit of messages, it is rather costly in the sense of the capital involved in the erection of numerous lines. That difficulty has been got over in a measure by multiplying the channels available on each wire, by duplexing, by quadruplexing, and by other methods. I observe that Mr. Murray does not like the idea of the quadruplex method. I need not say we all know that the B side, the increment side of a quadruplex system, is always the weak portion of that system, and that when troubles arise from bad insulation or other faults with which the telegraph man has to contend, the B side is the side that always fails. I may add, however, that we have had a very marked measure of success in the use of quadruplex working on our new underground lines which are now being laid, and are rapidly approaching completion, between London and Glasgow. The more important of these wires had to be laid up in loops; in fact, the design was similar to that of a telephone cable, because we knew there would be a large amount of mutual induction, static and magnetic, between neighbouring wires if they were worked as single wires to earth. We provided loops, therefore, for all main circuits, and we have adopted the following method of working so as to get as effective a service out of the metallic loop as we could out of single wires. We generally put a high-speed Wheatstone or a quadruplex circuit on a loop, and we superimpose on them a duplex circuit, so that we are able in that manner to get six channels out of two wires. That is one method of effecting economy, and there are others which I need not refer to now. When we come, however, to one of the important functions of the Post Office, the transmission of press matter, there machine telegraphy has not only been generally used, but I have no hesitation in saying that the work of the country could not have been carried out without it. At least two millions of words have to be transmitted over the Post Office wires every night, but without the machine telegraph known as the Wheatstone system it would have been utterly impossible to deal efficiently with the amount of news handed in every evening. The Wheatstone system is an admirable one so far as the transmission is concerned. The reception has always to my mind been the weak portion. Work can be turned out at three or four hundred words a minute, but at the far end it is necessary to employ a large staff to write out laboriously from the record slip. When I heard first of Mr. Murray's beautiful system it struck me that therein lay the hope of the economical distribution of our news matter, as well as perhaps an improved method of dealing with ordinary public messages.

With reference to the other systems, we are always experimenting with various types of instruments at the Post Office. In the last eighteen months we have carried out experiments with the Rowland telegraph which Mr. Murray refers to, which was exhibited in Paris and was briefly described by me in a paper read shortly after the Exhibition. We have tried the Buckingham; we have tried the Mercadier, in which they profess to send twenty-four messages simul.

taneously on one wire; we have tried the Pollak-Virag, in which a punched slip is reproduced, not in typewriting, but in actual cursive characters, and we have also tried the Creed and the Murray. Mr. Murray's is a beautiful system, and all I can say is, that I hope it will be as successful as it promises to be. Perhaps the feature in Mr. Murray's apparatus which has struck me as being the most novel, and certainly which I have appreciated very highly, is his extremely ingenious method of obtaining asynchronous running between the transmitting and the receiving apparatus. His method of placing spring buffers at the extremity of the vibrating reed, and thereby admitting of regulating the speed by varying the current due to clipping the signals, is a beautifully designed piece of apparatus, and in itself I must say it struck me as one of the prettiest and most novel features of the whole design. Mr. Gavey.

Mr. H. LAWS WEBB: Mr. Murray has, I think, covered the subject very broadly and very deeply. I have marked three or four points that occurred to me in the paper as being of interest to discuss. Mr. Murray says on page 557 that the fundamental distinction between the telegraph and the telephone is in the simplification of signals. In the long run the proof of the pudding is the eating, and the fundamental commercial distinction between the telephone and the telegraph is that the telephone gives you direct communication between the speaker and the listener without any intermediary apparatus or transmission or operation, such as Mr. Gavey has described, of taking the message to the operator, and of sending it out again, and so forth. That is the whole point in the difference between the telephone and the telegraph, and that is the reason why the telephone has completely outdistanced the telegraph, in point of volume of business done, within a very few years. You will find that in long-distance telephone service, in spite of the heavy wire cost which long-distance telephony involves, and in spite of the high cost of using it, journalists, who are very acute business men, use the telephone for transmitting their messages, because they find they can get more work done in a given time and at a lower price by the telephone than by the telegraph. With regard to the question of telegraph alphabets, Mr. Murray states with absolute certainty that he has the best alphabet, so there is no need to discuss that question. The question of errors is a very important point. Mr. Murray described, on page 577 I think it is, how the perforating operator can correct errors, how a typewriter operator in every page makes some mistakes, but he has consciousness of those mistakes in the making of them, and can rub them out. With the ordinary typewriter of commerce I find a good many mistakes occur that the operator has no consciousness of at all, and I have no doubt the same must occur in perforating telegraph messages. Therefore some errors get through to the receiving instrument before they are noticed. It would be interesting to know how the received errors are taken care of. I have studied the paper pretty carefully, and I do not find that point described. Errors must naturally occur, in any telegraph system, that do not go out to the line on the tape, from "kicks" on the line, and from impulses getting shortened or lengthened, which must appear as errors on the receiving instrument. I think that Mr. Webb.

Mr. Webb. is a very practical point; it would be interesting to know how the actual received errors are dealt with. Dealing with the key-board perforator, on page 579, although Mr. Murray is an authority on the subject, I think he hardly does justice to the actual possibilities of key-board perforators for telegraph work. The Buckingham key-board perforator, which is an electrical perforator, producing letters of unequal length, has absolutely displaced in the New York offices of the Western Union Company all the mallet punches formerly used for Wheatstone work, because it is found that the capacity of the operator is so largely increased. My recollection is that they find an expert operator can perforate messages at the rate of about 80 words a minute on the Buckingham perforator. I fished out a record of one man's work there. It shows in 152 days a total of 47,708 messages. That is one man's work on a key-board perforator with Wheatstone working—an average of just under 314 messages a day. On some days this gentleman's work went very much over the average. On some days it reached 600 odd messages, and on one day 700 odd, and there were numerous days with over 500 messages. The American message averages a good deal longer than the English message, for the reason that the custom there is to give the address and the signature free, and people therefore write ample addresses and very often full signatures, so that the average number of words is considerably more than in the average English message. I do not wish to make any captious criticism, but I think it would have been better, since Mr. Murray is so convinced that he has the best alphabet and the best system, if he had left aside other systems and confined his description to his own, because there are one or two remarks here that other people might reasonably object to. It is said the Buckingham system was tried and rejected by the British Post Office during the winter of 1903. It certainly was rejected, but opinions may differ as to how far it was tried. Mr. Murray has said that his system has been tried by the Post Office for eighteen months, but I think he would not be far wrong if he doubled that time. The Buckingham system was actually tried, in steady work with the Post Office employees, for something like eight weeks, and the trial was stopped before the operating staff had fairly a chance of getting hold of a new system. Of course the trial of a new system like that might reasonably last a good deal longer to find out its weak or its strong points. It is natural to imagine that an operating staff would have a certain amount of difficulty in picking up the working of a new system, so that I do not think that constitutes a very thorough trial, although I can quite understand that the Post Office, which was already experimenting with the Murray system, did not wish to carry on very long experiments with the two. It is no good trying to flirt with two people at the same time. I quite agree with Mr. Murray's remarks on page 596 that the capital cost of the apparatus for working a high-speed printing system is of very small consequence when you consider it in comparison with the capital cost of the lines and the amount of labour which it saves. I quite appreciate the theory that Mr. Gavey has mentioned in explanation of the Post Office system of hand working, that what people want is very rapid transmission; they take no

account of distance, and demand a minimum amount of delay, regardless of distance. But I have not the slightest doubt that with proper organisation in the handling of the messages, commercial messages could be transmitted just as quickly by an automatic machine system as by hand. Mr. Webb.

Mr. W. JUDD: I have had the pleasure of knowing Mr. Murray for some years past, during the time he has been carrying out his experiments, and have had the pleasure of very many interesting conversations with him, in which he has shown what is obvious and manifest in his apparatus, the originality and ingenuity which he has brought to bear on his task. One of the things that interests telegraph and cable people is this question of alphabets. It is almost impossible with any large system of telegraphy to tamper with your alphabet. If you do, it will be very much the same as if our President were to decree that our discussions here were to take place in Volapuk. I think I remember that in a certain colony not very many years ago it was determined to introduce the international code in place of the original Morse code. Whether they succeeded or not I do not know, but I believe a rather chaotic period intervened. I think that before altering the alphabet on any large system we should have to consider the matter very seriously. In connection with his alphabet, Mr. Murray suggested to us the use of Mr. S. G. Brown's cable relay and receiving perforator, by means of which we should punch out a slip with his alphabet at the other end of a long cable, pass it through a transmitter, and so repeat the operation until we get to the other side of the world. This would be a comparatively simple thing if we had a separate cable for every country; that is to say, if we had a straight through line from here to the Antipodes, but on lines where there is way traffic coming in and going out at all sorts of intermediate stations, we are confronted with the difficulty, which all telegraph men will understand, of the running message, or check numbers for each circuit. The message would start with a London number, and it would naturally have under that system to carry that number right through, because of the copying effect of the apparatus. It would be impossible, therefore, transferring from one circuit to another with all sorts of intermediate numbers coming in on it, to keep any control over the traffic, and, of course, a lost message is the unpardonable sin in any telegraph service. These difficulties may seem small, but Mr. Murray has indicated in his paper that they become important when they have to be definitely faced. We are very much interested in the question of key-board perforator speeds. We have a model of our own which will be in use before very long, and the question of speeds to be attained on it is naturally somewhat interesting. We have tried it on ordinary typewriters, with our most expert typists, who could rattle away in a very imposing manner from a newspaper or from any plain writing, but when the hideous code words that the public inflict on us now have to be dealt with, the expert typist goes to work with one finger like a novice on a piano; so that when the last speaker told us of the operators in New York attaining a speed of 70 words a minute I felt very distinctly relieved. I only hope that we on this side of the Atlantic will be able to do as Mr. Judd.

Mr. Judd. well. There is one difficulty with all these improvements that are brought to the notice of any large submarine telegraphic enterprise. Improvements are desirable, of course, but the great distances, and climatic and other influences present certain difficulties. Still, we are always trying to carry out improvements, and are only too willing to hear of them. As far as the cable interest is concerned, I am bound to say that a great many of the improvements have come from outside our own circle, as is the case with Mr. Murray and Mr. Brown, our gold medallist of last year, who are outside the telegraphic ranks, and who are therefore not running in a groove as some of us have done for the last thirty or forty years. They come in with fresh ideas, scorning the limitations which we have put before us, succeed sooner or later in showing us that there is something outside of our own groove which is well worth our attention. That is very largely so in the case of the enormous amount of research and work in regard to alternating currents, magnetism, and self-induction. We rather thought that these things had nothing to do with us at one time. We have long since learned to see that is not so, and we have found that we can make use of these improvements and discoveries as well as anybody else. I think that is the principal advantage of an institution like this. It is not a meeting of traders who have axes to grind; it is a meeting of people who endeavour, each to the best of his ability, to advance the cause of the science in which we are more immediately interested, and in that way I think the Institution does more to spread abroad the knowledge we are all wanting than any other institution in the country.

Mr. Higgins. Mr. F. HIGGINS: I have been very much interested in this system for telegraphing with type at a distance. I think it will be extremely useful for long lines; but in the case of the business with which I am connected I can see that it would not be of any service whatever, because the operator preparing the message at 20 words a minute —which I think is an exceedingly low rate—would be far behind our operators, who send it out and deliver it direct at 30 words a minute. In the case of the City of London, we sometimes deliver messages between Ludgate Circus and Temple Bar to the newspaper offices on about six or seven lines at the rate of 5,000 words a minute; but Mr. Murray's apparatus would not assist us at all in getting the news out promptly. The initial delay in preparing the message and its translation at the end would delay the publication of news in the various editions of the newspapers and make this service rather a slow one compared with the present, which is altogether slower apparently in transmission than the Murray, but we multiply the number of wires to increase the rate of delivery. We have a wire for each separate service, and an operator transmits the messages as they are handed in direct without any previous preparation. They are received in the newspaper offices in Roman characters, which require no further translation. They are handed direct to the compositors, and in some instances the messages are delivered to the public within a few seconds of their having been delivered on our tapes. But for long-distance telegraphy, where the cost of wires is a very important consideration, I have no doubt that this interesting system might be of very consider-

able service and ultimately displace the Wheatstone, which will some day perhaps become obsolete. Mr. Higgins.

Mr. H. M. SAYERS: I do not think this discussion should close without an appreciation of the admirable way in which Mr. Murray's paper has been written. It is a model and an example of logical and orderly setting out of a beautiful invention. Both as regards the mechanical logic—if I may invent a phrase—and historical sequence the paper is an exceedingly interesting one. I do not think a better paper as regards style and arrangement has been read before any scientific institution for many years. I was interested in what Mr. Murray said to-night about the punching speed, because I was once a Wheatstone puncher myself. In those days the Wheatstone was the fastest instrument that was in use in this country. It had not then attained the speeds which have been reached in recent years, because iron wires were still in universal use, and we thought we did very well if, with fine weather throughout the whole of the line, we worked from London to Glasgow at 100 words a minute without a relay. The Wheatstone perforator is capable of 25 to 30 words a minute when a good operator has a continuous flow of work; 40 to 45 words a minute is easily done on press work where one gets sheets of 100 or 120 words each. The Wheatstone perforator has only three keys. There is no need to look at the keys at all; one could punch at full speed in the dark if the message were dictated to him, except for the fact that a good puncher keeps an eye on his slip. He can read both copy and slip faster than he can punch so that the process of looking out for errors, always considered an exceedingly important one, in no way interferes with the rapidity of punching. If with a Wheatstone puncher, in which one has to compose his letters in the Morse alphabet, 30 words a minute is a commercial speed, it seems to me that with the Murray or other key-board instrument operators with sufficient practice ought to be able to reach a speed of from 80 to 100 words a minute. Whether they can punch at that speed, and also keep an eye upon the slip is another question; that will only be answered when the apparatus gets into regular use. A Wheatstone operator does not reach 40 words a minute with six months' practice; it takes some years to get up to that speed. A good Hughes operator can send 50 words a minute, but with more key positions, and combinations as well, to learn, takes rather longer than a Wheatstone puncher to attain maximum speed. In the Hughes case there is, however, an instrumental speed limit. There is another point that strikes me as historically interesting. Mr. Murray mentions the possibility of a "space" alphabet. I wonder if he remembers that the old Cooke and Wheatstone five-needle instrument had a space signal alphabet. Each letter was indicated by the simultaneous movement of certain of the five needles, so that the alphabet was formed by space signals and not time signals. That is an interesting fact which may be added to Mr. Murray's collection of telegraph alphabets. Mr. Sayers.

Mr. F. J. MUDFORD: Some twelve or fourteen years ago I was interested in a telegraphic type-setting machine, the practical side of which I worked out. I have therefore been very much interested in Mr. Mudford

Mr. Mudford. Mr. Murray's paper and in the apparatus which he has shown us to-night I would like to make one or two suggestions which may be useful to him or to others. He mentioned the noise that the instrument made. By hanging the vibrating reed on rubber cords he will scarcely hear the reed working. He will also find that with the La Cour or phonic wheel he would get a saving by having two series of magnets in each case opposite each other, used so that the pull came exactly opposite the spindle. We found that the tug on one side was very strong indeed, setting up a considerable vibration and also interfering with the ease with which the wheel revolved. We found there was a considerable saving by having a magnet on each side. In Mr. Murray's case there would have to be two series of magnets, because one has to be a half tooth behind the other, so that he would have to have four magnets. Mr. Murray has got over one little trouble that we experienced, by doing away with the second revolving wheel that we used for our apparatus. We actually set type with our instrument between King's Cross and Holloway. Our revolving wheels ran at about 250 revolutions a minute, which brought us into the difficulty of a very small amount of time to deal with the contact. We got over that, strange to say, by having two contacts instead of one for each signal, and by having a system of intersections somewhat similar to what Mr. Murray has done in his printing machine. We had, say, twenty-eight working segments, and we had to operate two of these for each signal, so that with 24 + 4 contacts we could get four times twenty-four signals. We had therefore a very much longer time for the electrical operation of the relays. Another difficulty we had, which it might be of interest to some present to mention, was the operator keeping down the key too long. In that case he would send a repeat signal at the rate we were going. This led to the designing of a little apparatus which would break the connection when the signal had passed. On the depression of a key a battery contact was kept closed by an armature being held down by the soft iron extensions of a permanent magnet, but contact to the segment was only completed when the key was raised and the armature free to rise on the passage of a current to line which depolarised the extensions.

Mr. Adams. Mr. A. J. S. ADAMS: Perhaps one of the more important advantages of the Murray system has been overlooked in this discussion. If a Morse slip be handed to an operator for transcription, much will depend upon that operator, and, to put it mildly, his state of health; but with the Murray, directly a telegram arrives it is ready in Roman type, and all that remains is to tear it off and deliver it—a fact which, seeing that time and labour are money, should mean a great deal.

Mr. Murray. Mr. DONALD MURRAY, in reply: I think one of the chief points mentioned in the discussion was the question of speed on the keyboards. That is the very essence of printing telegraphy from a practical point of view. If that speed is not superior to the speed with the Morse key or some other methods of transmission, then printing telegraphs are useless. As a matter of fact it is superior. The speed possible on a keyboard is quite high; but when an operator has to sign and time short messages of twenty words, has to pick them up and

lay them aside, and do other little operations like that, the time lost is surprising. Mr. Webb mentioned the Buckingham system. I have some records of the work of the Buckingham keyboard perforator, and they are wonderfully good. One man, I was told in New York, punched 770 messages in 7½ hours, that is 104 messages an hour. But we must not take the maximum speed of the maximum operator ; we must be guided by the average speed of the average operator. That is the only thing that is of any practical importance. Mr. Webb mentioned the case of a man who, for quite a long period of time, 152 days, had done no less than 314 messages a day. That is very good ; I must admit it is better than I had expected. Nine hours a day is the usual time that American operators work, one hour of which may be regarded as slack time. Therefore, if you divide the 314 messages by 8, it gives you 40 messages an hour, and 40 messages an hour is not much better than good Morse key operators do in America. Thirty messages an hour is what they do here, and the messages are considerably shorter. If we multiply 40 messages an hour by 30 words for the average American message, and divide by 60, we get 20 words a minute. That is the average work of a good operator taken over a considerable period. The Morse key operator under similar circumstances will not do more than 10 or 12 words a minute when he has to sign the messages, and perform all the other little details. There are also times when business is slack. Sometimes there are no messages for the operator to go on with, and he has to wait. That is all included. That is the kind of speed I mean when I say 20 words a minute—including all the little incidental delays, signing and timing messages, tearing off the tape, rolling it up, and passing it on to the transmitter. All that cuts down the average speed. Taking everything into consideration, however, there is a considerable margin between the keyboard perforator and the Morse key. Morse key operators in America have attained a speed of 50 words a minute in short speed contests, and there was one very interesting tournament in which a group of male and a group of female picked operators competed. The female operators averaged 39·4 words a minute on the Morse key, and the male operators 43·5 words a minute, a very interesting result from many points of view, social as well as telegraphic. But these were picked operators ; and that was their maximum speed, and much beyond what they would achieve in ordinary telegraphic practice. Mr. Gavey made some remarks about the quadruplex, which reminded me that there is one point I have not made quite clear in the paper. The point I wish to make about printing telegraphs is that they can be worked with any method of transmitting signals. A quadruplex is really a method of transmitting signals over a line ; a printing telegraph is a system, it is not a method. A printing telegraph system can be worked by the duplex or the quadruplex method, provided you have a sufficiently good quadruplex ; but the weakness of the quadruplex comes in, and I do not think it would be worth while. The Murray system may be worked on the first side of the quadruplex, and the second side may be used for getting corrections and repetitions and sending messages by hand if desired. There is no competition between

Mr. Murray.

Mr. Murray.

a quadruplex and a printing telegraph. That is rather a mistake on my part; I should have made it clear in the paper, and Mr. Gavey has done well in calling attention to the point. I noticed that several speakers referred to synchronism, although I have drawn special attention in the paper to the fact that there is no synchronism between the two stations in the Murray printing telegraph. There is isochronism. It is quite an important distinction. If you only have speed to consider and not phase it makes a great difference. Reference was also made to the question of the correction of errors, and it was pointed out that many errors pass unnoticed, but in designing the correcting device it was not so much the correction of errors that was aimed at; the aim was to give the operator confidence, and therefore speed on the keyboard. When the operator knows that if he strikes a false key he can instantly correct the error, he can work more confidently and therefore more quickly. There is a saving of labour, and the operator can do more work without working harder. Of course, the correction of errors is in itself very important, and the human being does make unconscious errors, and such errors he cannot correct. To put the argument in another way, I might mention that medical men have frequently called attention to the remarkable analogy between the human brain and a gigantic system of telephone exchanges. It has been estimated that there are no less than 3,000 million nerve cells, or to follow out the metaphor, subscribers, in the human brain, and the network of nerves or wires is past all conception. What the human operator has to do is to make his connections in his brain correctly. It is a very curious fact that any one who chooses to keep a note of the errors he makes in writing, will find that most of his mistakes are mistakes of switching. As he writes, he thinks of the words or the letters ahead, and it will generally be found that the mistakes are transpositions of letters, especially when working on a typewriter. The mind is conscious of most of this stumbling the moment after it occurs, and it is to enable the operator to overcome this trouble that the keyboard is provided with a correcting mechanism. Mr. Judd mentioned some points about cable work. This is very important for the future; but I agree with him that it will be some time yet before we get printing telegraphs working on cables. With a complicated and world-wide system like that of the Eastern Telegraph Company, it would, of course, be exceedingly difficult to change from the Morse alphabet, but it is possible to modify the Murray printing machines in several ways so as to enable them to use the Morse alphabet. I have designed one arrangement for that purpose, and I think that that will be the starting-point for the application of the Murray printer in combination with the Brown cable relay and perforator to cable work. Possibly in time the Cooke alphabet may come into use, because it effects a saving of about 10 per cent. on the cables compared with the Morse, and that might be worth taking into consideration. On the Atlantic cables, there is no such objection to the use of the Cooke alphabet and a printing telegraph as Mr. Judd pointed out applies in the Eastern Telegraph Company's service. With the Atlantic cables there is direct communication between two centres of

commerce, and it is quite possible that in the near future something may be done in the direction of working the Murray apparatus in connection with the Brown cable relay and perforator on Atlantic cables. Mr. Higgins referred to the local distribution of news. The instruments for this purpose have been wonderfully developed; they are practically perfect, and there is no suggestion of competition between them and the Murray system. They have their field to themselves. There a high speed is obtainable on the keyboards because there is no timing of messages and no stopping; the operators go straight ahead all the time. On the other hand, with a system in which you have to handle ordinary short telegraph messages of 20 words, the conditions are altogether different, and the problems involved are very difficult indeed, and the solutions by no means such as one would expect at the first glance. Mr. Murray.

The PRESIDENT: I am sorry I had not the pleasure of hearing the paper itself, owing to the fact that I could not be present at the last meeting, but I have listened with great interest to the explanation of the apparatus which has been given us by Mr. Murray, and also to the discussion. There is only one point upon which I should like to have some further information. Mr. Judd said it would be impossible to alter the alphabet. Of course for the ordinary instrument that is quite true. There is the difficulty at present, as I daresay all of you know, that in the United States a different alphabet is used from that which is in use in this country, and this gives rise to a considerable amount of trouble. But I am afraid I have not understood the apparatus, because looking at it as an outsider, as I understand it, the sending clerk has to punch his message on the key-board on which the letters are marked, and the receiving clerk receives the message ready printed. Where does the alphabet come in? The President.

Mr. JUDD: Only for the reason that it would be necessary very often to read the tape as it came out. Suppose the printer breaks down, it would be necessary to read the tape, and it will not do to have an unknown language on it. You must be able to read it. The alphabet on the tape is a different alphabet from that which is in use now, and it would be rather awkward to have two alphabets in the same office at the same time. Mr. Judd.

The PRESIDENT: I think other speakers have already explained that any system of printing telegraphs is really only practicable where messages have to go over long distances, and are in the nature of press messages. Then printing instruments are very valuable, and judging from the time I took to learn the Morse alphabet when I was young and had to do with these matters, I do not think I should take very long to learn the other alphabet if I had to do so. These instruments will always be of a special character, so that you can always have special clerks to work them. I am perfectly sure that a system in which all the signals are exactly the same length is the correct one, and makes the simplicity of the apparatus possible. In that connection I think this apparatus has quite a unique feature; at any rate, I have never seen before that the demonstrator absolutely took the instrument to pieces before the audience and put it together again—and then it The President.

The President.

worked. That is a most important point, and that is really the correct principle for all construction. All apparatus should be made so that it can be put on a table, taken to pieces, put together again, and then made to work. As Mr. Murray said, it is the outsiders who are always inventing wonderful telegraph instruments; but all these are generally of such a complicated kind, that their adoption is difficult. The simpler the instrument can be made the better, and therefore this new alphabet which enables you to make so simple an instrument has a great deal to recommend in it.

It is now my pleasure to ask you to give a hearty vote of thanks to Mr. Murray, the author, for the very interesting paper which he has brought before us.

The resolution was carried with acclamation.

Proceedings of the Four Hundred and Twentieth Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 2, 1905—Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on February 23, 1905, were taken as read, and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associates to that of Members—

Kenneth F. Campbell.

From the class of Associates to that of Associate Members—

Stanley Harris.
Guy S. Long.
William H. Ridpath.
Henry H. Wright.

Messrs. G. C. Allingham and G. Stannage were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Associate Members.

William Randall Elliott.
Manley Farrer.
Wilfred Hoult, B.Sc.
Basil Mortimer Iles.
David Macbeth.
Peter Scott Macduff.
Sidney Gilchrist Smith.
John Edwin Teasdel.
A. V. Wardrop.
Fred Williams.
Edgar Williamson.
Charles Herbert Wright.

Associate.

Charles Robert Gibson.

Students.

Frank Barlow.
Albert Glazier.
Joseph Richard Hall.
Edward Friend Hetherington.
Louis Du Buisson Hugo
Sydney Arthur Joyce.
Philip Lobel.
George Arthur Milburn.
Ronald Atkinson Nuttall.
William Henry Marsh Parr.
Gilbert H. Pickering.
Samuel Ernest Povey.
Charles Russell Powell.
Anak Shore.
Norman Baldwin Smith.
Sydney Augustine Verschoyle.
Harold W. White.
Wilfred Yorke.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Archibald Constable & Co., Gauthier-Villars; to the *Building Fund* from Messrs. J. R. Andrews, R. C. Barker, A. D. Constable, K. W. E. Edgcumbe, A. G. Hansard, E. P. Harvey, C. E. Hodgkin, Lord Kelvin, A. E. Levin, J. Maclean, E. Mascart, J. L. F. Vogel, D. C. Wardlaw; and to the *Benevolent Fund* from Messrs. Dr. R. T. Glazebrook, S. G. C. Russell, H. D. Symons, F. W. Topping, to whom the thanks of the meeting were duly accorded.

The discussion on Mr. Donald Murray's paper was concluded (see page 597).

The meeting adjourned at 9.25 p.m.

Proceedings of the Four Hundred and Twenty-First Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 9, 1905—Mr. ALEXANDER SIEMENS, President, in the chair.

The Minutes of the Ordinary General Meeting held on March 2, 1905, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

Kenelm Edgcumbe. | John H. Fooks-Bale.
Samuel D. Schofield.

From the class of Associates to that of Associate Members—

Ernest E. Allen. | John L. Hermessen.
Campbell Macmillan.

From the class of Students to that of Associate Members.

Walter Alexander Turnbull.

Messrs. W. C. Clinton and J. S. Fairfax were appointed scrutineers of the ballot for the election of new members ; and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Associate Members.

William Thomas Fraser.
Robert Caspar Goldston.
Charles Leyson Goolding.
Alfred L. Hamkens.
Charles George Hampson.
James Henry Hardy.
George William Osborn Howe, M.Sc.
Arthur Daniel Jaffé.
Edward George Love.
Abraham Press.
James Rolls Walker.

Students.

F. C. Davies.	Percival Frank Harris.
Ernest Graves.	George James Jackson.
Maurice Gregory.	Bernard Percy Walker.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Whittaker & Co. ; to the *Building Fund* from Mr. M. M. Gillespie ; and to the *Benevolent Fund* from Messrs. A. F. R. Curteis, G. J. Gibbs, M. M. Gillespie, F. J. Thompson, to whom the thanks of the meeting were duly accorded.

Mr. E. H. Rayner's and Mr. R. Goldschmidt's papers were read and discussed.

The meeting adjourned at 9.35 p.m.

REPORT ON TEMPERATURE EXPERIMENTS CARRIED OUT AT THE NATIONAL PHYSICAL LABORATORY.*

By E. H. RAYNER, M.A., Associate Member.

Communicated by Dr. R. T. GLAZEBROOK, F.R.S., Member.

(*Paper read March* 9, 1905.)

The Paper deals with two investigations which have been undertaken on behalf of the Engineering Standards Committee, to whom the author is greatly indebted for permission to present the results to the Institution in this Paper.

The author realises the limits of laboratory tests, especially as regards the first portion of the paper, and that actual prolonged satisfactory running of electrical plant must be the final criterion in electrical as in all engineering. But the results here detailed will, it is hoped, be of use to the electrical industry, and if members present will, in the discussion, give the meeting some account of their difficulties relating to insulation, the author will feel himself well repaid; and is certain that the electrical industry at large cannot but benefit by such discussion, especially as in these days the highest practicable voltages must be used, and insulation is becoming the all-important factor.

PART I.—ON THE EFFECT OF HEAT ON THE ELECTRICAL AND MECHANICAL PROPERTIES OF DIELECTRICS.

The first investigation was one on the effect of heat on the electrical and mechanical properties of such insulating materials as are usually employed in electrical machinery, more especially in the construction of continuous-current armatures and transformers.

The second part deals with the internal temperature of field coils of continuous-current machinery.

The research on the effect of heat on insulating substances involved the determination of the voltage which would pierce the substance, and some tests devised to measure the loss of mechanical strength and increase of brittleness.

Several manufacturers were requested to submit specimens of fabrics and other materials to the sub-committee on Physical Standards of the

* Based on Reports issued by the Engineering Standards Committee and presented to the Institution by the kind permission of that body.

Engineering Standards Committee, and the tests have been carried out at the National Physical Laboratory.

Electrically-heated ovens were constructed and specimens of each substance were kept at 75°–100°, 100°–125°, 125°–150° C. As the work promised to require more time than the staff of the laboratory could afford, the author was requested by Dr. Glazebrook to take up the research.

The disruptive tests were carried out when possible on specimens which were placed between circular electrodes of about 1 in. diameter, 5 sq. cm. area, the upper one being loaded with a total pressure of two kilograms to ensure good contact. In the case of tapes, etc., two rods ¼ in. diameter were used with hemispherical ends, the pressure being ¼ kilogramme. In the tests with the former, the area of the dielectric exposed to pressure being fairly large, the spark naturally pierced at the weakest point of that area, whereas in using the smaller electrodes higher and more irregular voltages were required.

One of the aims of the investigation was to obtain standard conditions, and to that end the effect of the variation of the frequency on the disruptive pressure has been examined. It was found that the alternating pressure required to break down a specimen fell with the frequency, as is shown by the following results :—

Specimen.	Frequency.	Disruptive Pressure in Volts.
Press-spahn heated for 6 weeks at 75° C.–100° C.	56 36	3,750 3,300
Oiled Cloth (not heated)...	50 37	4,580 4,370

In order to discover whether this effect was confined to solid dielectrics alone, the pressure required to spark across an air-gap between two brass rods of ¼ in. diameter with spherical ends was determined, at the available limits of frequency, giving results which have been confirmed by experimenting with different lengths of air-gap. The following figures show that this effect is one which might be expected to occur in the case of all insulators, using this source of alternating potential (namely, a rotary converter).

	Frequency.	Volts (average).
Air-gap dielectric ...	36 56	4,250 4,710

A very important variable is the temperature at which the disruptive pressure is applied. This is clearly shown by the following figures :—

Specimen.	Time in Oven and Temperature.	Conditions.	Disruptive Pressure in Volts.
Press-spahn	$1\frac{1}{2}$ months at 100° C.–125° C.	Sparked in oven at about 100° C.	3,150
do.	do.	Sparked cold after one minute.	3,400
Oiled cloth	do.	Sparked in oven at about 100° C.	4,330
do.	do.	Sparked cold after one minute.	5,200

A frequency of 50 was decided upon as a convenient standard to be used throughout all the tests, and in view of the convenience of carrying out the tests at ordinary temperatures the specimens were exposed to the air of the room for a period of five minutes before being subjected to the tests.

The usual procedure in the tests was to raise the pressure on the specimen to the sparking-point in about a quarter of a minute. If exceptionally regular results were obtained, a further application of a lower pressure was made on an adjacent portion, note being taken of the time required to pierce the specimen.

Oiled linen heated to 125° C.–150° C. for three months :—

After standing five minutes in air the following figures for the disruptive pressure were obtained :—

3,600, 3,600, 3,600, 3,700 volts. Average = 3,620 volts.

Broken down	in	0′ 40″	by	3,000	volts.
„	„	0′ 10″	„	3,000	„
„	„	0′ 15″	„	3,000	„
„	„	2′ 15″	„	2,500	„
„	„	4′ 15″	„	2,500	„
„	„	0′ 20″	„	2,500	„
„	„	0′ 20″	„	2,500	„

Specimen of same oiled linen heated to 100° C.–125° C. for three months :—

Disruptive pressure obtained as above :—

4,200, 4,200, 4,200, 4,400 volts. Average = 4,250 volts.

Broken down	in	0′ 30″	by	3,800	volts.
„	„	1′ 0″	„	3,800	„
„	„	0′ 15″	„	3,500	„
„	„	1′ 0″	„	3,300	„
„	„	1′ 30″	„	3,000	„
„	„	4′ 0″	„	3,000	„
„	„	4′ 15″	„	3,000*	„
„	„	1′ 30″	„	3,000*	„

* In order to ascertain whether the exposure to the air had in any way altered the specimen, the pressure was continuously raised and was found to be 4,300 volts, showing that no appreciable alteration had taken place. The specimen was then tested again in the above manner and pierced as shown by the last two figures given above.

These figures have been stated in full so as to show the limitation of the general results of the experiments.

Mechanical Tests.

Under this heading punching and bending tests have been carried out.

A test of tensile strength was tried, but was found unsatisfactory on account of the brittleness of the substances.

For the punching test a small machine was constructed with a steel punch, ½ in. in circumference, which, from the results obtained, appears to afford an excellent and simple criterion of the mechanical properties of the specimens, and may be considered to give a fair measure of the increase of brittleness with temperature.

The initial pressure on the specimens was adjusted to about 15 lbs. below the punching pressure, the pressure being increased to break-down point, at the rate of about ½ lb. per sec. This rate of increase is of no little importance, as shown by the following results:—

Oiled board, unheated, normal condition—

Punched by 29, 29, 30, 28 lbs. Mean result = 29 lbs.

Punched in	0′ 3″	by	27 lbs.
,,	0′ 7″	,,	26 ,,
,,	0′ 23″	,,	25 ,,
,,	0′ 14″	,,	24 ,,
,,	0′ 20″	,,	23 ,,
,,	0′ 30″	,,	22 ,,
,,	1′ 55″	,,	20 ,,

The following method was employed in carrying out the bending tests: The specimen was bent round cylinders of gradually decreasing diameters, varying from 12 ins. to $\frac{1}{16}$ in., until it broke, or was badly cracked. If a specimen was capable of being bent round a cylinder $\frac{1}{16}$ in. diameter without breaking, it was bent backwards and forwards round this, up to ten times, unless, of course, it broke before that number was reached.

This test appears to be a very good one as regards brittleness, the increase of which is very marked in the majority of materials examined.

In many cases the disruptive pressure of the unheated specimen had a lower value than after heating, due in all probability to the presence of moisture, as the following figures indicate:—

Waterproof board heated (100° C.–125° C.) for seven weeks, broken down by 3,630 volts.

Waterproof board heated (75° C.–125° C.) for seven weeks, broken down by 3,730 volts.

Waterproof board unheated, broken down by 2,420 volts.

The break-down pressure of the unheated specimen being so low, it was subjected to a temperature of 75° C. for 14 hours, and it then broke down at 3,760 volts, which is practically the same pressure at which the heated specimens broke down.

Explanatory Notes to Tables.

The first column gives the names of the materials tested. The second column defines the condition of the materials :—

(*a*) Unheated.
(*b*) After being heated to 75° C.–100° C.
(*c*) „ „ „ 100° C.–125° C.
(*d*) „ „ „ 125° C.–150° C.

The third column gives the average disruptive pressure observed.

The fourth column, expressed as a percentage, gives the ratio between the greatest difference of the individual pressures from the average of column three, to that average.

The fifth column gives the thickness of the specimen in millimetres.

The sixth column gives the volts per millimetre thickness.

The seventh column gives the punching pressure in lbs.

The eighth column gives the results of the bending test. The expression ($\frac{1}{16}''$; 6) indicates that the specimen was fractured on being bent round a cylinder $\frac{1}{16}$ in. diameter three times in each direction. The expression ($\frac{1}{16}''$; > 10) indicates that the specimen withstood being bent round a cylinder $\frac{1}{16}$ in. diameter more than five times in each direction.

The figures $3\frac{1}{2}''$, $8''$, indicate the diameter of the cylinder, the curvature of which was sufficient to cause the specimen to fracture at the first time of bending.

All unheated insulators tested, with the exception of the very thickest, withstood the test of bending round the cylinder $\frac{1}{16}$ in. diameter more than five times in each direction, and as it would not differentiate these (*a*) unheated specimens, no figures have been recorded. This fact must, however, not be lost sight of when considering the results of the heated specimens.

The various specimens were heated for a period of six weeks to three months before being tested.

Substance.	Condition.	Disruptive Voltage.	Ratio of max. diff. from mean to mean.	Thickness. Millimetre.	Volts per millimetre.	Punching Pressure. Pounds.	Bending Test. Cylinder Diameter.
Press-spahn	a	2,180	9 %	·23	9,500	55	—
	b	2,330	6 „		10,000	57	$\frac{1}{16}''$; > 10
	c	2,330	6 „		10,200	27	$\frac{3}{4}''$
Press-spahn	a	2,920	13 „	·56	5,200	106	—
	b	3,550	3 „		6,300	105	$\frac{1}{16}''$ 6
	c	3,670	7 „		6,550	79	1″
	d	3,330	3 „		5,950	40	$2\frac{1}{2}''$
Press-spahn	a	6,650	2 „	1·61	4,150	> 150	—
	b	> 9,000			> 5,600	> 150	about 5″
	c	> 9,000			> 5,600	> 150	„ 8″
Press-spahn and Standard Varnish	a	3,610	8 „	·34	10,500	57·5	—
	b	7,120	12 „		21,000	74	$\frac{5}{8}''$
	c	> 9,000			> 26,000	67	1″
	d	> 9,000			> 26,000	60	$1\frac{3}{4}''$

Substance.	Condition.	Disruptive Voltage.	Ratio of max. diff. from mean to mean.	Thickness. Millimetre.	Volts per millimetre.	Punching Pressure. Pounds.	Bending Test. Cylinder Diameter.
Manila Paper ...	a	1,540	8 ,,	·28	5,500	62	—
	b	1,540	2 ,,		5,500	25	1/2″
	c	1,590	5 ,,		5,700	20	1″
Manila Paper ...	a	1,620	4 ,,	·38	4,300	69	—
	b	1,920	4 ,,		5,100	41	1/16″; 4
	c	1,840	5 ,,		4,800	24	1 3/4″
Manila Paper and Standard Varnish	a	1,800	2 ,,	·34	5,300	54	—
	b	3,400	8 ,,		10,000	47	1″
	c	4,340	8 ,,		12,700	42	2″
	d	4,180	9 ,,		12,300	37	2 1/2″
Waterproof Board	a	2,420	2 ,,	·29	8,300	62·5	—
	b	3,720	7 ,,		12,800	67	1/16″; > 10
	c	3,630	11 ,,		12,500	34	1″
Waterproof Board	a	3,300	4 ,,	·44	7,500	94	—
	b	4,480	17 ,,		10,200	102	1/16″; > 10
	c	5,200	10 ,,		11,800	57	1″
Oiled Cloth	a	4,580	4 ,,	·22	21,000	29	—
	b	5,110	20 ,,		23,000	27	1/4″
	c	4,650	14 ,,		21,000	24·5	5/8″
	d	3,940	12 ,,		18,000	19	1″
Red Oiled Paper	a	6,600	6 ,,	·25	26,000	30	—
	b	6,850	11 ,,		27,000	34	1/2″
	c	7,900	13 ,,		31,000	37	1/2″
	d	6,940	8 ,,		28,000	28	1 1/4″
Black Oiled Board	a	5,320	2 ,,	·30	17,700	59	—
	b	5,460	8 ,,		18,200	61	1″
	c	6,170	3 ,,		20,600	55·5	2″
	d	4,870	4 ,,		16,200	33	2 1/2″
Oiled Linen	a	4,850	10 ,,	·23	21,000	24·5	—
	b	4,390	7 ,,		19,000	28	1/4″
	c	4,050	4 ,,		17,500	27	3/8″
	d	3,440	2 ,,		15,000	20	1″
Excelsior Paper No. 1.	a	4,150	13 ,,	·12	35,000	25	—
	b	6,370	6 ,,		53,000	18·5	1/16″; 6
	c	4,950	6 ,,		41,000	14	1/8″
	d	5,170	20 ,,		43,000	11	1/2″
Excelsior Paper No. 2.	a	4,530	4 ,,	·13	35,000	32	—
	b	6,530	11 ,,		50,000	24	1/16″; 4
	c	6,020	8 ,,		46,000	18	3/16″
Excelsior Paper No. 3.	a	6,260	10 ,,	·20	31,000	31	
	b	> 9,000			> 45,000		
Excelsior Linen No. 3.	a	6,700	18 ,,	·25	27,000	45	—
	b	> 9,000			> 36,000		3/16″
	c	> 9,000			> 36,000		3/16″
	d	7,600	10 ,,		30,000	27·5	1″

Substance.	Con-dition.	Disruptive Voltage.	Ratio of max. diff. from mean to mean.	Thickness. Millimetre.	Volts per millimetre	Punching Pressure. Pounds.	Bending Test. Cylinder Diameter.
Excelsior Linen No. 6.	a	3,250	8 „	·15	22,000	36·5	—
	b	5,550	13 „		37,000	23	$\frac{3}{32}''$
	c	6,500	20 „		43,000	20	$\frac{3}{16}''$
	d	5,420	7 „		36,000	14·5	$\frac{1}{4}''$
Excelsior Silk No. 1	a	910	12 „	·10	9,100	19	—
	b	2,100	20 „		21,000	13	$\frac{1}{16}''$; > 10
	c	2,320	13 „		23,200	8·5	$\frac{3}{32}''$
	d	1,950	17 „		19,500	4·5	$\frac{3}{8}''$
Excelsior Silk No. 2	a	2,650	7 „	·13	20,000	19·5	—
	b	5,010	9 „		38,000	21·5	$\frac{1}{16}''$; 4
	c	4,650	18 „		36,000	16	$\frac{1}{4}''$
	d	4,560	2 „		35,000	12	$\frac{3}{8}''$
Dynamo Tape ...	a	500	2 „	·15	3,300	35	
	b	600	7 „		4,000	36	
	c	560	4 „		3,700	23	
	d	615	1 „		4,100	8	
Superfine Dynamo Tape and Standard Varnish.	a	640	9 „	·23	2,900	32	—
	b	790	9 „		3,600	24	$\frac{1}{16}''$; > 10
	c	700	7 „		3,200	23·5	$\frac{1}{16}''$; 2
	d	620	13 „		2,800	11	$\frac{3}{8}''$
Linotape	a	6,220	2 „	·27	23,000	38	—
	b	9,000*			33,000	41	$\frac{1}{16}''$; 8
	c	9,000*			33,000	37	$\frac{1}{4}''$
Grey Fibre	a	2,000†		·65	3,000	108	$\frac{1}{16}''$; > 10
	b	5,100	3 „		7,900	77	$3\frac{1}{2}''$
	c	3,950	13 „		6,000	45	8″
Grey Fibre	b	> 9,000		1·75			
	c	> 9,000					
Red Fibre	a	1,300†		·55	2,400	108	$\frac{1}{16}''$; > 10
	b	4,000	4 „		7,300	77	3″
	c	4,350	6 „		8,000	33	4″
	d	3,960	3 „		7,100	34·5	6″
"Berrite." Treated Paper "A."	a	1,810	5 „	·19	9,500	31	—
	b	1,280	4 „		6,700	28	$\frac{1}{8}''$
	c	1,240	7 „		6,600	27	$\frac{1}{2}''$
	d	1,140	4 „		6,000	26	$\frac{5}{8}''$
"Berrite." Treated Paper "B."	a	1,830	4 „	·47	3,900	57	—
	b	2,420	8 „		5,150	51	$\frac{5}{8}''$
	c	2,000	5 „		4,300	55	$1\frac{3}{4}''$
	d	1,940	3 „		4,100	45	$2\frac{1}{2}''$

* 9,000 breaks specimen down in short time. It may be started by sparking round edge of tape.

† The conductivity of Fibre in its normal state is sufficient to allow so much current to pass as to make Disruptive Voltage indefinite.

Substance.	Condition.	Disruptive Voltage.	Ratio of max. diff. from mean to mean.	Thickness. Millimetre.	Volts per millimetre.	Punching Pressure. Pounds.	Bending Test. Cylinder Diameter.
"Berrite." Fabric No. 1.	a	1,350	28 „	·37	3,500	33·5	—
	b‡	1,200	3 „		3,140	22	⅛"
	c‡	1,380	45 „		3,730	16	1"
	d‡	1,280	26 „		3,460	14	2½"
"Berrite." Fabric No. 2.	a	2,100	2 „	·38	5,500	67	—
	b‡	1,240	3 „		3,280	45	⅛"
	c‡	1,140	8 „		3,000	31	1½"
	d‡	1,200	5 „		3,150	32	3"
"Berrite." Insertion.	a	3,960	9 „	2·0	1,980		
	b	3,340	14 „		1,670		
	c	3,230	10 „		1,620		
	d‡	3,000	3 „		1,500		

‡ Pinholes numerous.

Insulation Resistance.

In addition to the disruptive tests, a series of measurements of insulation resistance were made.

It was considered possible that there might be some relation between the ohmic resistance and disruptive pressure, but this has not been found to be the case to any great extent.

A careful series of determinations of the resistance of the materials which had been heated at a temperature of 75° C.–100° C. were, however, carried out before determining the disruptive pressure on the same specimens.

The resistance of similar unheated specimens is given in some cases. The large difference was probably due to the presence of moisture, and, as the resistance of the unheated specimens depends so much on atmospheric conditions, the figures cannot be taken as in any way absolute, but merely as representing the order of magnitude. They are, however, useful in comparing the damp-proof qualities of the various specimens of the dielectrics tested.

The resistance of the specimens, which had been heated to 75° C.–100° C. was determined at 75° C., so to avoid possible change due to atmospheric conditions.

The electrodes used were two circular soft-rubber discs of 50 sq. cm. area, and of 1 cm. thickness covered with thin tinfoil. The specimen was placed between these electrodes, and on the upper one a brass disc of the same diameter was placed with lead weights, amounting in all to 20 kilos.

The resistance varied rapidly at first, after the application of the weights, and an interval of five minutes was usually allowed before the application of the test pressure. The deflection of the galvanometer was read one minute after the application of the pressure, and again one minute later, and in some cases at successive minute intervals,

if it seemed desirable. The resistance was calculated on the deflection after electrification for one minute. The deflection after two minutes did not often differ by more than about 5 per cent. from the deflection obtained after one minute. The galvanometer used was of the Broca type with a sensitiveness of about 2×10^{-10} ampere per scale division. This was used with an Ayrton Mather shunt giving ratios of 1 to 1,000, which in some cases was further supplemented (for low resistances) by an additional shunt increasing this value to 100,000, and even to 1,000,000. The galvanometer, shunt, and special high-insulation switchboard, designed by Mr. A. Campbell, were made in the laboratory workshop. In carrying out the experiments it was found desirable, as a result of much preliminary work, to earth one pole of the battery (a set of 500 small accumulators) and to protect all the apparatus from leakage by Price's guard-wire system, which has been found quite satisfactory. It will be noticed from the following table, that the resistance was first determined at a difference of potential of 200 volts, then at a difference of 1,000 volts, and in some cases again at a difference of 200 volts. The resistance was less at 1,000 volts, and did not recover completely again at 200 volts. It will be observed that the varnished substances are more damp-proof cold, but that when heated the untreated materials give the higher values.

INSULATION RESISTANCES.

	Thickness.	Volts.	Megohms.	Volts.	Megohms.	Kilo Megohms per cm. 3
		Unheated.		Heated at 75° C.		
Press-spahn	0·23	200	2·8	200 1,000 200	22,000 9,600 18,500	4,700 2,100 4,000
Press-spahn	0·56	200	5·7	200 1,000	37,000 32,000	3,300 2,900
Press-spahn and Standard Varnish	0·34	200 1,000	5,000 3,400	200 1,000 200	10,500 9,200 10,500	1,550 1,350 1,550
Manila Paper	0·28	80	0·96	200 1,000 200	12,500 7,600 11,000	2,200 1,350 2,000
Manila Paper and Standard Varnish	0·34	200 1,000	1,950 510	200 1,000	16,700 11,900	2,450 1,750
Waterproof Board ...	0·44	200	8·1	200 1,000	> 200,000 220,000	> 23,000 25,000
Red Oiled Paper ...	0·25	...	...	200 1,000 200	1,400 1,000 1,220	280 200 244

INSULATION RESISTANCES (*continued*).

	Thickness.	Volts.	Megohms.	Volts.	Megohms.	Kilo Megohms per cm.3
		Unheated.		Heated at 75° C.		
Black Oiled Board ...	0·30	200	1,300	200	4,850	810
		1,000	570	1,000	3,800	630
				200	4,600	770
Oiled Linen	0·23	...	...	200	220	48
				1,000	75	165
				200	185	40
Excelsior Paper No. 1	0·12	...	...	200	3,500	1,450
				1,000	1,900	830
" " No. 2	0·13	...	...	200	4,400	1,700
				1,000	2,400	920
				200	3,100	1,200
				1,000	1,800	690
" " No. 3	0·20	...	...	200	4,400	1,100
				1,000	2,500	620
				200	3,100	770
Excelsior Linen No. 1	0·22	...	...	200	4,200	950
				1,000	1,750	400
				200	2,600	590
" " No. 3	0·25	...	...	200	4,700	940
				1,000	2,900	580
				200	3,300	660
" " No. 4	0·22	...	...	200	2,100	480
				1,000	1,950	440
				200	1,350	310
" " No. 6	0·15	...	...	200	4,200	1,400
				1,000	1,800	600
				200	2,600	870
Grey Fibre	1·75	80	3·0	200	13,700	3,900
				1,000	11,000	3,100
Red Fibre	0·55	200	1·2	200	56,000	5,100
				1,000	31,500	2,900

The discrepancies which appeared to exist between the heated and unheated materials being probably due to moisture, the effect of desiccation was tried, and the insulation resistance was determined after various intervals of time.

In the ensuing pages, the subject of insulation resistance has been entered into, somewhat in detail, on account of its important bearing on the manufacture of electrical machinery.

EFFECT OF DESICCATION ON INSULATION RESISTANCE.

The desiccation was carried out in an ordinary desiccator over sulphuric acid at atmospheric temperature.

PRESS-SPAHN 0·62 MM. THICK.

Hours from Start.	Condition.	Megohms.	
	Normal	1·07	
	Specimen reversed ...	1·04	
0	Put into desiccator ...	—	
18	Removed and tested ...	6,600	Drop in resistance apparently due to absorption from atmosphere.
	Specimen reversed ...	7,000	
	" " again	5,200	
	" " "	4,100	
19	Put into desiccator ...	—	
49	Tested again	41,000	
	OILED CLOTH 0·20 MM. THICK.		
	Normal	75	
	Specimen reversed ...	84	
0	Put into desiccator ...	—	
2½	Tested	1,700	
18	"	48,000	
	Area of specimens ...	50 sq. cm.	

The relative efficiency of desiccation and heat with regard to their influence on the Insulation Resistance was then ascertained. Two specimens of press-spahn 1·07 mm. thick were cut from the same large piece. The one was put into a desiccator and the other into an oven at 100° C. with the following results.

Hours from start.	Desiccator specimen. Megohms.	Oven specimen. Megohms.
0	0·42	0·80
1	1·3	>4,000,000
2	1·5	(Tested cold)
6·5	3·4	
23	510	
76	70,000	
122	200,000	
12 days	800,000	

Four million megohms was the highest value which could be measured. This shows how much more efficient heat is than merely a dry atmosphere.

A specimen of oiled cloth 0·20 mm. thick.

Unheated ... ... 50 megohms.

Heated for 5 minutes at 100° C. (tested cold) 100,000 „

A specimen of press-spahn 0·53 mm. thick.

Unheated ... ... 1·8 megohms.

Heated for 10 minutes at 100° C. (tested cold) 250,000 „

It appeared to be of interest to ascertain the actual amount of moisture required to produce this great difference in the insulation resistance. A piece of the same quality of press-spahn 1·07 mm. thick, after being heated at 100° C. for one hour, lost 10 per cent. in weight, or 1·33 grammes per 100 square cms. Of this it regained 0·32 gramme after remaining three days in the closed oven to cool, and 1·19 grammes after a total period of a week.

The result of the experiments on heating for a few minutes at 100° C. shows what an immense change takes place in the resistance of such dielectrics on being cooled; though the result of experiments given below shows that sudden cooling is necessary in order to obtain high insulation resistance after a *short* subjection to a high temperature.

It has been suggested by Mr. Campbell, that, on heating a material such as press-spahn, the water is driven out of the fine capillaries of the wood fibre, where it naturally collects by reason of its large surface tension at low temperatures. On being heated it is driven into the interstices, where it is much more useful in conducting the electric current. This action is practically as rapid as change of temperature. If sufficient time is not allowed for it all to evaporate, the substance has, when hot, a low resistance. Evaporation commences at once and the resistance begins to rise after quickly attaining the lowest point.

This phase is shown in the following figures:—

Insulation Resistance after a Sudden Rise of Temperature.

The resistance of a piece of press-spahn was determined in its normal condition, and then placed between the electrodes of the duplicate apparatus, which had attained a steady temperature of about 100° C. in the oven. Six observations were taken at minute intervals. The press-spahn was then moved and placed at one side of the oven, where the air could circulate over its surface, and the resistance determined again after 23 and 32 minutes.

	Minutes.	Megohms.
Normal condition, *cold* ...	0	0·31
After one minute, *hot* ...	1	0·006
	2	0·010
Thickness 1·07 mm. ...	3	0·013
	4	0·016
	5	0·018
	6	0·021
	23	1·0
	32	1,200·0

If, however, the condition of the material, after being heated for a short period and suddenly cooled, is considered, the following would appear to be a feasible explanation of what occurs.

The first effect of the application of heat would seem to be to drive the moisture out of the capillaries, with a consequent drop in the resistance. The moisture near the surface then begins to evaporate, and if, after this has gone on for a short period, the substance is rapidly cooled, the instantaneous effect is to draw the moisture into the adjacent capillaries, and leave two comparatively high resisting surface layers, from which some of the moisture has evaporated. In this condition the resistance will have a much higher value than in the original state. The diffusion of the central moisture into the drier outer layers will at once commence and the resistance begin to fall at a gradually decreasing rate as equilibrium is established. The press-spahn used in these two experiments was cut from adjacent parts of the same sheet. The wide difference caused by simply altering conditions is well illustrated by the figures, obtained for the resistance at the fourth minute in the two experiments, given in the following table and in the table above.

	Minutes.	Megohms.	Minutes.	Megohms.	Minutes.	Megohms.
Initial Resistance, *cold*	...	·46	10	220	20	61
			11	180	21	58
Put in oven at 100° C.	0	...	12	150	22	55
Taken out	3	...	13	130	23	52
Tested, cold	4	1,400	14	115	24	49
	5	900	15	103	25	46
	6	630	16	92	26	45
	7	430	17	84	...	...
	8	330	18	73	960	6·6
	9	270	19	66	...	...

Experiments were carried out to ascertain the effect of heating the specimen which had been in the desiccator for 12 days, and had reached the high figure of 800,000 megohms, having started with an initial insulation resistance of 0·42 megohms. The similar piece which had been heated for an hour, reached a figure above 4,000,000 megohms, showing that the desiccated specimen still contained an appreciable amount of moisture. If the temperature were suddenly raised the resistance should decrease and then steadily rise again as the moisture was driven off.

	Time in Minutes.	Megohms.
Cold	0	800,000
Put into oven	0	...
Tested "	2	2,200
" "	15	6,000
" "	34	15,000
" after cooling all night in oven	...	300,000

This expectation was confirmed by the figures in the preceding table.

Press-spahn, desiccated for 12 days.

In all these experiments press-spahn was used, as owing to its thickness the changes take place sufficiently slowly to be observed.

It may be of interest to state the effect of sunshine as a desiccating agent.

A piece of press-spahn, 1·07 mm. thick, was exposed to intermittent sunshine out of doors (October 23rd). During the previous days there had been much rain, and the air could not be termed dry.

Resistance before exposure ... 0·84 megohms.
" after 70 minutes' exposure 630 "

Such results show how entirely fallacious comparative results may be, unless very great attention be paid to small details, and for this reason the actual conditions of the present tests have been related at some length in this report.

After the completion of the above tests, some further tests were carried out on samples of substances which had been treated with Berrite varnish. It was considered sufficient to test these after exposure to two temperatures only, 75° C. and 125° C.—the intermediate temperature being omitted. As a check on these, specimens similar to some of those which had been previously tested, were again examined in order that account might be taken of any difference in the conditions. In the heating of these substances special care was taken to diminish the temperature gradient in the ovens—which are provided with thermostats—especially in the case of the hotter one in which the thermometer would keep within one degree of 125° for several days, and in which the maximum temperature was some 10° to 15° above the thermometer reading.

The varnish on some of the new substances becomes viscous on the application of heat, and it was found necessary to place each between two pieces of tissue paper, to which it adhered. The tests for the disruptive voltage have been made through the paper and no allowance has been made for it. In the punching tests, however, small portions were used where the paper had not adhered, as it was found that punching through the tissue paper as well, would increase the pressure about five pounds.

The conditions of the test were as follows:—

The specimens were heated at the temperatures stated for a total of thirty days, the heating being discontinued at week-ends.

The specimens were tested for disruptive voltage between electrodes of circular shape, one inch in diameter. The punching test was carried out with a circular die, half-inch in circumference. The bending tests were done by noting the diameter of the largest cylinder, which caused the breaking of the specimen when the latter was bent round it.

The condition of the specimens is defined by the letters (*a*, *b*, *d*).

(*a*) Unheated.
(*b*) After being heated at 75°–85°.
(*d*) " " " 125°–135°.

As in the previous tables a figure is given, representing, as a percentage, the difference between the mean of the observations of the

Substance.	Condition.	Disruptive Voltage.	Ratio of Maximum Difference from Mean to Mean (per cent.)	Thickness, Millimetres.	Volts per Millimetre.	Punching Pressure, Pounds.	Bending Test. Cylinder Diameter.
Paper and "A" Berrite Varnish	*a*	4,100	17	0·12	34,000	25·5	—
	b	4,500	9		37,500	19	1/16"
	d	4,450	4		37,000	16	1/4"
Press-spahn "C" and Berrite Varnish	*a*	4,650	10	0·22	21,000	44·5	—
	b	4,200	12		19,000	44	1/16"; > 10
	d	3,400	9		15,500	36	3/8"
Linen and "D" Berrite Varnish	*a*	3,700	8	0·17	22,000	47	—
	b	1,600	25		9,400	34	3/32"
	d	1,600	19		9,500	20	1/2"
Linen and "E" Berrite Varnish	*a*	2,700	22	0·17	16,000	24	—
	b	3,300	40		19,500	13·5	3/32"
	d	3,400	9		20,000	15	1"
Linen and "F" Berrite Varnish	*a*	3,700	8	0·28	13,000	40	—
	b	5,000	16		18,000	24	1/16"
	d	3,650	22		9,500	22	3/4"
Press-spahn "G" and Berrite Varnish	*a*	4,200	5	0·31	13,500	74	—
	b	4,200	7		13,500	51	1/16"; > 10
	d	3,700	3		12,000	55·5	3/32"
Excelsior Linen No. 0	*a*	4,500	9	0·17	26,500	29	—
	b	4,900	22		29,000	33	1/16"; > 10
	d	4,900	10		29,000	21	1/8"
Excelsior Linen No. 3	*a*	6,000	9	0·21	28,500	32	—
	b	7,900	8		37,500	39·5	1/16"; > 10
	d	7,350	1		35,000	31	1/4"
Excelsior Paper No. 3	*a*	4,800	6	0·12	40,000	22	—
	b	7,800	5		65,000	24·5	1/16"
	d	7,300	3		60,000	22·5	1/8"
Press-spahn and Standard Varnish	*a*	4,200	5	0·29	14,500	70	—
	b	4,700	4		16,000	75	1/16"; > 10
	d	4,550	3		15,500	45	5/8"
Waterproof Board	*a*	3,200	12	0·30	10,500	67	—
	b	3,300	27		11,000	70	1/16"; > 10
	d	3,300	37		11,000	51	1/4"
Press-spahn and Berrite Varnish "G"	180° to 195°	3,700	11	0·31	12,000	17	3"

disruptive voltages, and the maximum difference from the mean. It is a measure of the irregularity of the substance. The punching pressures are much more concordant, and generally agree to a pound or two, as in the previous series of experiments.

In accordance with the request of the Committee further experiments have been completed on the effect of heat at a still higher temperature on insulating substances.

Specimens of the identical substances mentioned above were heated at a temperature of 180° C.–195° C. for twenty-five days, the temperature of the oven being kept constant by a thermostat.

It was found that most of them were charred so much as to have become disintegrated, and they crumbled to powder on being touched.

One specimen, however, though very brittle, could be subjected to the usual tests, and the results are given in the last line of the table on preceding page.

Part II.—On the Temperature Distribution in the Interior of Field Coils of Electrical Machinery.

This part of the paper deals with an investigation undertaken at the request of the Committee on Electrical Plant of the Engineering Standards Committee, who desired to know, what, in actual practice, was the difference between the maximum temperature reached in field coils and the mean temperature calculated from the increase of resistance of the coil.

There were two practicable methods available for the determination of the temperature at various points in the coil. The one was by the use of a number of electrical resistance thermometers, which would require a number of small and compact resistance coils of some metal which has a large temperature coefficient, such as copper or iron, to be embedded at various points in the field coil, the temperature being calculated from the increase in resistance of these small search coils.

This is the method which has actually been adopted by Mr. E. Brown, M.Sc. (*Journal Inst. Elec. Engineers*, 1901, part 152, vol. 30), who investigated the temperature distribution in a field coil of a small two-pole machine. The author wishes to acknowledge the use that he has been able to make of this paper, especially as regards the shape of the temperature curves, and in many cases he has found that the hottest point in the coil has been exactly as expected from a study of the curves given in the paper quoted.

The second method available was by the use of thermo-electric couples composed of two different metals or alloys. This is the common method used for the measurement of high temperatures by pyrometers, the thermo-junction being usually of pure platinum on one side and an alloy of platinum and iridium or similar metal on the other ; the voltage generated is measured on a millivoltmeter of the moving-coil type which may be graduated in degrees of temperature. The possible adaptation of such a method to the measurement of ordinary, instead of furnace temperatures, required considerable consideration; and the author believes that this is the

first time that a systematic temperature research has been carried out with portable instruments of this convenient type. A moving-coil instrument has the great advantage that it can be made actually to read degrees of temperature, and that it can also be used in the neighbourhood of electrical machinery. Further a thermo-junction is the simplest possible arrangement for temperature measurement, and from its nature far superior to any resistance coil for measuring the temperature at a point.

It will be advantageous to glance at the precautions to be taken in using thermo-junctions for temperature measurement, as the author considers that the application of the principle is one, the possibilities of which every electrical engineer ought to have some knowledge of,

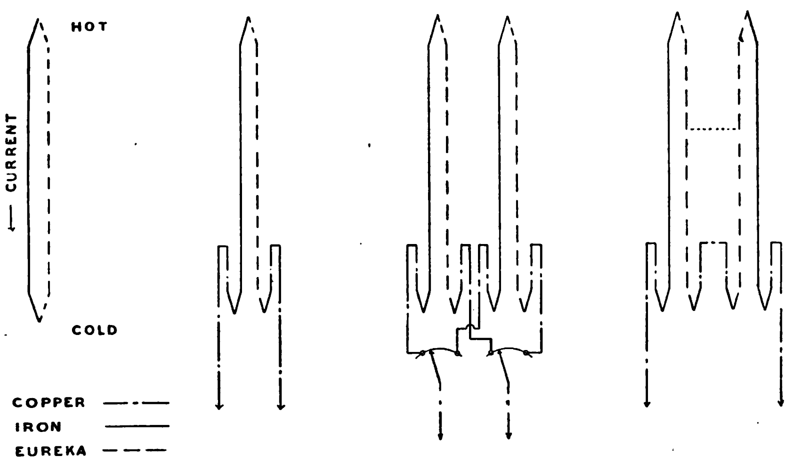


DIAGRAM OF ARRANGEMENTS FOR THE MEASUREMENT OF TEMPERATURE BY THERMO JUNCTIONS

DIAGRAM A.

with a view to the measurement of flue temperatures, increase in temperature of feed-water due to the use of economisers and of steam in superheaters, and for other similar purposes. The thermo-junction method will probably displace every other temperature-measuring apparatus for many purposes, and the convenience of the ability to determine temperatures at a considerable distance must make many useful temperature measurements possible which have hitherto been impracticable. In addition to this we can use the same instrument to measure all temperatures from that of the liquid gases to a white heat. The sensitiveness can be adjusted as required by adding resistance to the circuit, and the method is capable of all the refinements and precision associated with zero methods of electrical measurement.

The fundamental basis is the production of an E.M.F. in a bimetallic

circuit due to difference of temperature of the two junctions. For laboratory purposes one is usually kept at the temperature of melting ice. The circuit is not, however, completed at this "cold" junction, but other wires, usually copper, are there connected to the thermo-couple wires proper, and if the rest of the circuit be either all of the same metal or all at the same temperature, no other thermo-voltages will disturb the system (excepting possibly the very small Thomson Effect). It is obvious therefore that the leads to the measuring instrument may be as long as convenient, provided that their resistance be not of importance. The required factors in measuring temperature are: the temperature of the cold junction, the voltage generated and the voltage per degree difference of temperature of the thermo-couple used. The actual value of the latter two factors need not be known. To make the instrument read degrees of temperature with a set of similar thermo-junctions, it is sufficient to heat one of their number in an oil bath with a thermometer, and adjust the resistance in series with it, till the reading on the instrument is equal to the difference in temperature of the two thermometers, if a second be used to measure the temperature of the cold junction. This is practically the method which has been used in this investigation. The instrument then reads degrees of temperature above that of the "cold" junction when used with similar thermo-junctions. This assumes the relation between E.M.F. and temperature to be linear, which is not, in general, the case. In practice, with certain combinations of metals, if the apparatus be set to read correctly at any temperature, the error for some distance on each side will be quite negligible. In any case it is easily calculated. To confirm the temperature thus registered by a thermo-junction (embedded in a field coil for instance), it may be "backed" against one in an oil bath with a thermometer, the temperature of the bath being altered till the residual voltage is zero. The thermometer should then read the same temperature as the thermo-junction in the field coil. On this principle all the thermo-junctions were tested before being used by being heated with one of their number as standard.

In designing the apparatus these points had to be considered. The thermo-junctions used were of iron-eureka, as this combination has a high thermo-electric voltage, which is very fairly linear over the range required. The junction wires were No. 26 gauge, each double-silk covered and the pair covered again together with a single layer of silk, making a very compact and flexible arrangement for the measurement of temperature at a point. The junction of the two wires at the end was effected by silver solder. Each pair of wires, about 1½ metres in length, had a definite resistance and their thermo-electric constant was compared with one of their number, which was kept as standard. It was not found necessary in any case to reject a length of wire because of any difference in the thermo-electric constant, when compared with the standard, this difference being less than ¼ per cent. in all cases. The constant of different samples of wire will, of course, vary slightly, which is unavoidable in such an alloy as eureka.

The actual reading of the temperature was recorded by an extremely sensitive moving-coil galvanometer by R. W. Paul. The instrument

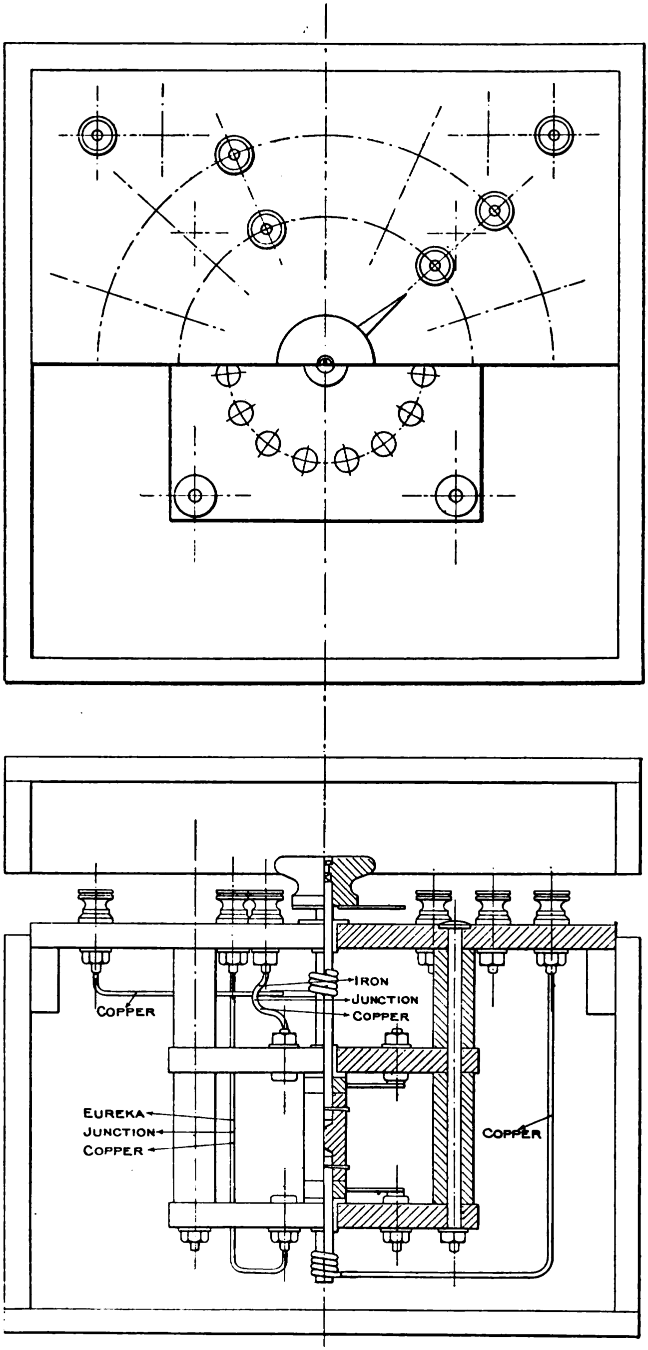


DIAGRAM B.—Thermo-junction Switch.

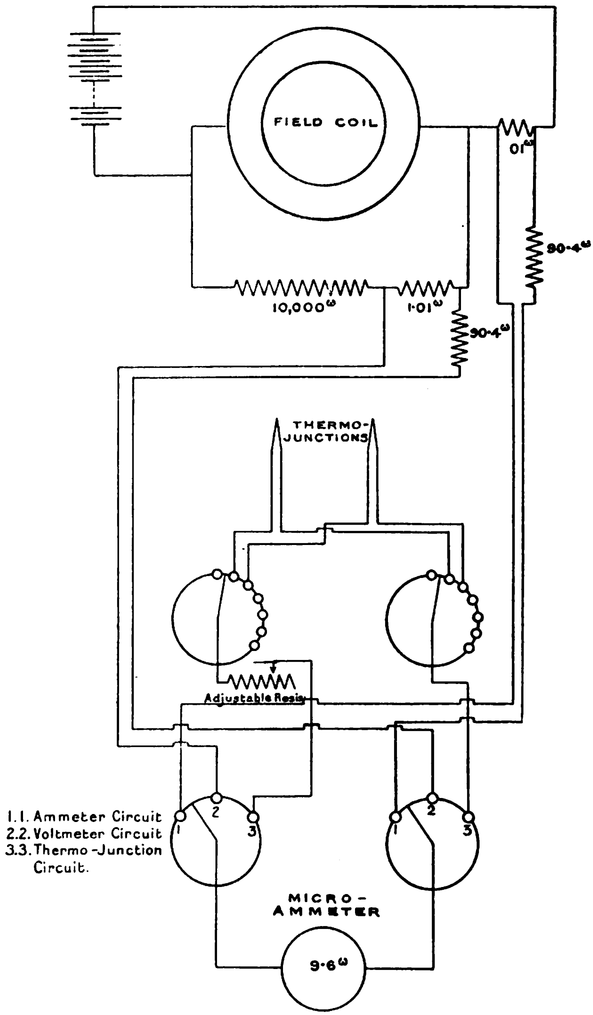


DIAGRAM C.

was designed with a particular resistance and sensitiveness for the special conditions under which it was to be employed.

The highest reading on the scale is 0·0002 ampere and less than 0·002 volt.

The scale unit is one-millionth of an ampere, there being 100 divisions of 2 micro-amperes. This is some 50 times as sensitive as the usual pattern of switchboard instrument of the same type, and the author wishes to sincerely thank and to congratulate Mr. Paul on the invention of the instrument, upon which the whole investigation has been built and to which any success has been due.

By the addition of a variable resistance in the thermo-junction circuit the instrument reads directly in degrees Centigrade.

One valuable feature of an instrument of such great sensitivity is that, of the total resistance of about 53 ohms in the circuit, as much as 40 ohms are added "dead" resistance without temperature coefficient; which "swamps" the change of resistance of the galvanometer and of the rest of the circuit due to temperature, sufficiently for any such change to be neglected. The "cold" junctions were placed in a box with fifteen pairs of terminals, which at the same time was used as a multiple switch for the various thermo-junction circuits. Though brass terminals have been used, the continuation of the circuit inside by iron and eureka wires has rendered negligible any possible small thermal effects due to the use of brass. A thermometer in the box gives the temperature of the "cold" junction, and this, added to the deflection of the galvanometer, gives the temperature in degrees Centigrade. The exact value of the resistance to be put in circuit to give direct readings in degrees Centigrade was determined by heating a sample thermo-junction in an oil bath with a thermometer, and in this manner the accuracy of the readings has been frequently checked. Various resistances have been specially made to suit the galvanometer, in order to be able to measure the volts and amperes in the coil under test, which was, of course, requisite for calculating the mean temperature of the coil at any time. A three-way switch was used, by means of which the thermo-junction-, the volt-, or ampere-circuits could be connected to the galvanometer. Besides this, resistances were arranged in the box so as to measure the resistance of the field coils cold, at a definite temperature, by a bridge method, thus avoiding the use of a large current and consequent errors due to the heating effect during observation.

Care was also taken in arranging the resistances so that the galvanometer could be calibrated by an ordinary potentiometer.

This arrangement for reading the temperature, the amperes and the volts on the same instrument has been of great assistance, especially in the experiments conducted at the works of the various manufacturers supplying the coils. The sensitiveness of this instrument has very largely contributed to the accuracy and ease with which the results have been obtained.

A diagram of connections is given showing the method adopted in carrying out the investigation.

The apparatus was ready for use by simply moving the multiple switch to read (for instance: at the 100th division in the centre of the scale) 100 volts, or 1·0 ampere, or 100° C. above the atmospheric temperature. The various other resistances were employed for giving other constants as desired.

The figures for the temperature have been read to 0·2 degree, and the points on any curve are in themselves of this order of accuracy, but the actual position of the whole curve is in all probability only accurate to one degree on account of various secondary errors due to temperature and stray magnetic field.

Special precautions were exercised to ensure equality of temperature

in the coil when measuring the "cold" resistance, as on this figure the calculations for the mean temperature were based. In some cases it was even found necessary to cover the coil with a blanket for two or three days before concordant figures could be obtained.

Drawings of the coils were obtained from the various manufacturers who had acceded to the Committee's request with regard to the testing of a coil of one of their machines. The positions in which it was desired that thermo-junctions should be inserted were then indicated on the drawing. About fifteen thermo-junctions for each coil were forwarded to the manufacturer with full instructions, and these were put into the positions marked, during the process of winding the coil.

The general method adopted throughout the investigation has been to carry out a preliminary test at the National Physical Laboratory on the coils, after which tests were made on the coils under actual working conditions at the manufacturers' works. In some cases, however, the preliminary test was not made with the working current on account of the abnormal temperature rise due to the absence of the fanning action of the armature. With regard to the actual positions of the thermo-junctions themselves, usually five were placed on a median cross section of the coil and three on the opposite side as a check. The five thermo-junctions were placed on the cross-section where the hottest temperature was expected to occur and the second thermo-junction from the core side was generally expected to record the highest temperature of all.

In a plane at right-angles to this, two or four more thermo-junctions were placed in line with the second thermo-junction from the core side. On the opposite side of the coil three other thermo-junctions were placed as a check on the first five, as above mentioned, and generally five more on the side of the coil nearest to, or furthest from, the commutator. As a general rule the difference in the temperature of corresponding points on the various cross-sections was found to be quite immaterial, and therefore no mention has been made of the temperature of these points.

Temperatures were also taken by means of a thermometer placed on the coil, but the results naturally varied with the amount of packing and local heating so produced, although with practice readings can be obtained agreeing fairly well with the curve of temperature inside the coil.

The excess of the maximum over the mean temperature varied according to the shape of the coil and the temperature at which it was run.

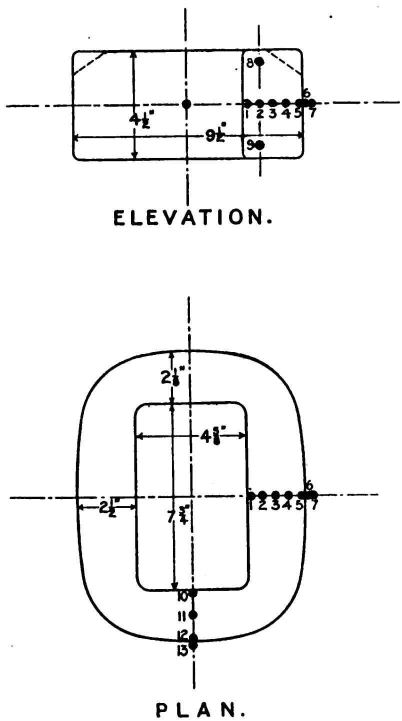
A considerable cooling effect on the core side was noticeable in the case of some of the coils wound on a metal former, where there was an air space between the core and the coil, and consequently some of the curves obtained show nearly as low a temperature on the core side as on the outside, when the machine was running.

In the following pages the coils have been designated by figures, the variations of Coil No. 1 being termed 1α, 1β, 1γ, &c.

The experiments have been indicated by the Coil No. followed by A, B, etc., the letter A being reserved for the test as nearly as possible under normal running conditions.

Details of Coils.

Coil No. 1a.—A field coil of a four-pole 35·5 B.H.P. motor, originally wound with single cotton-covered wire. When tested at the National Physical Laboratory the resistance rose normally for the first three or four hours, and then commenced to fall and continued to do so until the current was stopped. This effect was still in evidence after the coil had been returned to the manufacturers and short-circuiting was sus-



Layers 1 • 11 • 11 • 11 • 11 • 1 • Total 46.
Coil covered with Empire Cloth, and Taped.

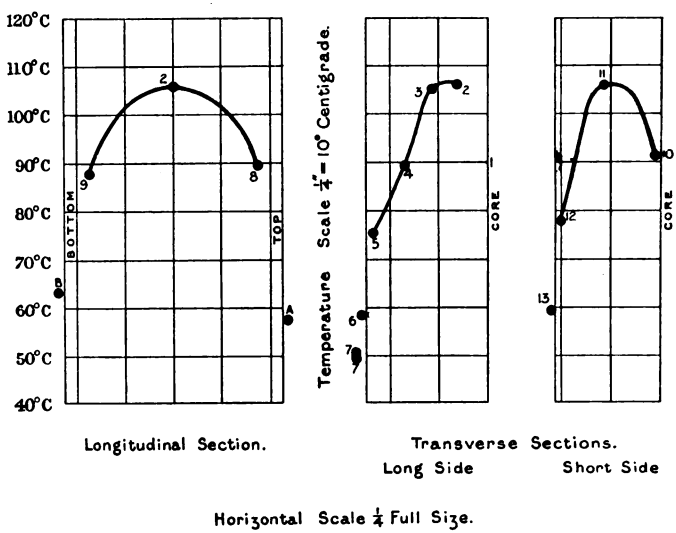
Coil No. 1*a*.

pected, especially as the resistance could be considerably altered by compressing the coil. The cause of the trouble was attributed to the use of single cotton-covered wire, and a similar coil was therefore supplied with double cotton-covered wire of 0·040 in. diameter wound on a temporary former and covered with a layer of Empire cloth, and a layer of varnished tape on the outside. This coil has been named No. 1*a* and was tested at the National Physical Laboratory at the rated voltage. The coil was then returned to the manufacturers, but it was

found impossible to fit it on to a machine on account of the curved yoke, which, when building the coil for this special test, had not been allowed for. A further coil with the corners cut off was wound to fit the machine, and this has been designated No. 1β.

Coil No. 1β.—As described above, this coil had the corners cut away and instead of being wound with wire of 0·040 in. diameter, the wire was 0·044 in. diameter.

This coil was tested on a machine at the manufacturer's works and behaved exactly as the previous coil did, the resistance rising and then



Conditions, Running light as a Dynamo.

Note, Point 7 upper value taken by Thermometer.
Points A & B were thermo-junctions under the taping but over the cloth.

COIL No. 1β.

falling until the machine was stopped, as shown by the following figures.

Hours from start.			Resistance in ohms.
0	...	...	65·2
1	...	...	74·7
2	...	...	79·5
4	...	...	84·0
5	...	...	82·5
5·5	...	...	78·5
6	...	...	75·8
7	...	...	73·0

In the meantime some experiments (see Appendix) had been carried out in order to obtain some information as to the safe temperature to which cotton may be subjected. The explanation which these experiments appeared to suggest was not that the fall in resistance was due to metallic short-circuiting, but to short-circuiting through the water actually driven out of the cotton by the rise in temperature. In the interior of the coil this water could not easily evaporate, and if present in sufficient quantity might well afford a passage for a considerable leakage current, which would gradually disappear as the coil slowly dried, but it is quite possible that in the meantime, it might cause damage by electrolysis.

In the case in point all the coils on the machine showed signs of being in a similar condition; they were therefore removed to an oven, and the resistance taken at intervals. After several days stoving, all signs of leakage disappeared, and the coils were again placed on the machine, and the test was satisfactorily completed.

No reading is given for one of the thermo-junctions (point No. 1) nearest to the core, as there was a break somewhere in the wire.

On the same section the fourth thermo-junction from the core side gave a considerably lower figure than would have been expected. This was probably due to the position of the end of the wire not being in the centre of the coil, but nearer to one flange than the other. It may also have been due to some damage in the wire itself, as this coil had to undergo an unusual amount of handling. In drawing the curve full value has, however, been given to the reading obtained at this point.

The readings of the thermo-junctions inserted in the layers of external taping will, it is hoped, be of especial interest to the electrical industry.

Coil No. 1γ.—During the tests of Coil No. 1*a* it appeared that some useful information could be obtained by running a coil, exactly similar to 1*a* at the same number of watts, but with different size wire.

The manufacturers kindly wound another coil with wire of 0·072 in. instead of 0·040 in. diameter. This was not made as an actual coil for a machine, but simply for the purpose above mentioned. The watts in Coil No. 1*a* had been 110 and the current was adjusted to give as nearly as possible 110 watts when the temperature had become steady. The actual watts were 107, and the following is an abstract of the results obtained:—

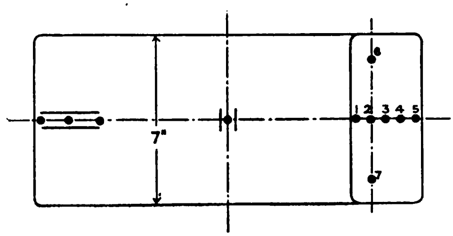
	Coil No. 1*a*.	Coil No. 1γ.
Wire, diameter in inches	0·040	0·072
Watts	110	107
Maximum Temperature above air in °C.	93·3	84·2
Mean Temperature above air in °C.	76·0	71·5

This shows an appreciably lower temperature-rise for the coil with the smaller amount of cotton.

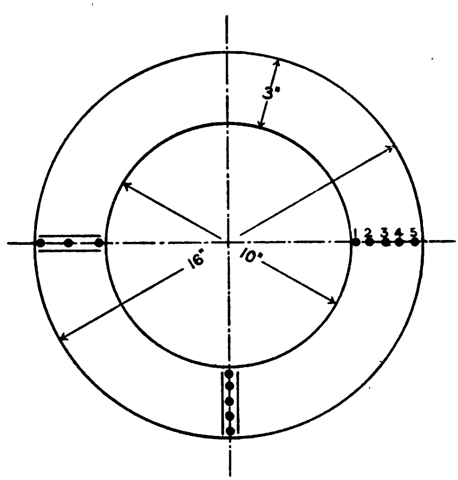
In addition to this, three other tests were carried out on Coil No. 1γ, the current used in one case being larger and in the other two cases smaller than was the case in the above test. If plotted the results

indicate that the temperature rise is proportional to the watts used in the coil.

Coil No. 1δ.—This coil was made to the same dimensions as the above. It was similar to Coil No. 1γ, being made of wire 0·072 in.



ELEVATION



PLAN

Layers 3•7•7•7•7•3. Total 34.

Coil impregnated & covered with tape

Thermo-junctions indicated thus ⊏⊐ were only employed to check the results shewn in the accompanying curves.

COIL NO. 2.

diameter. It was provided for the purpose of determining the effect on the temperature of winding the coil with d.c.c.-berrited wire, and differed also from Coil 1γ in having no taping or other covering.

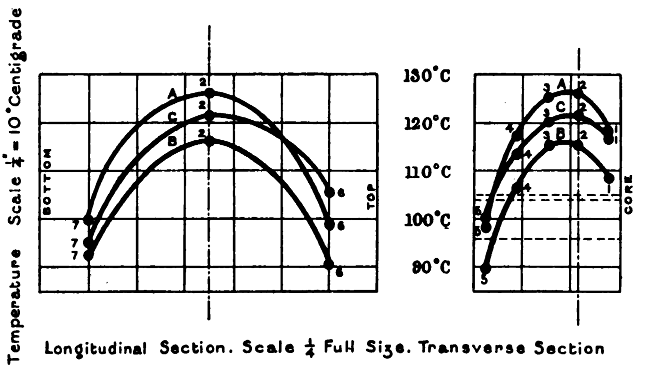
This coil and Coil No. 1γ were compared by being put in series, and the result showed a notable difference. The following are the most important figures which were obtained :—

		Coil No. 17.	Coil No. 18.
Maximum Temperature above air in °C.	...	80	54·9
Mean Temperature above air in °C. ...	...	63·6	46·9

Coil No. 2.—This coil was part of a six-pole, 94 B.H.P., 550 volt motor, with a speed variation from 300–550 r.p.m. by shunt regulation.

The coil was thoroughly impregnated and covered with two layers of linen tape, ¾ lap.

It was first tested at the National Physical Laboratory at a reduced current and the results are given in the curve (2.C.).



Curve	Conditions	Amps:	Mean Temp: in °C.	Max:-Mean in °C.
A	Running Loaded	4·3	103	21 · 5
B	Running Light	4·3	96	20 · 0
C	Standing	3·32	104	17 · 2

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines

Coil No. 2.

At the manufacturer's works the first run was with the motor running light, the results of which are given in curve (2.B.). The second run was with the motor on full load, showing a further rise of about ten degrees, due to the hotter air from the armature (2.A.)

It will be noted that the curves (2.A.) and (2.C.) cross each other. The reason for this was, that when tested at the National Physical Laboratory, the hot air rising from the bottom of the coil caused the top of the coil to be some ten degrees hotter than the bottom. For a similar reason the curves in the transverse section cut one another on account of the more efficient cooling when the armature was revolving.

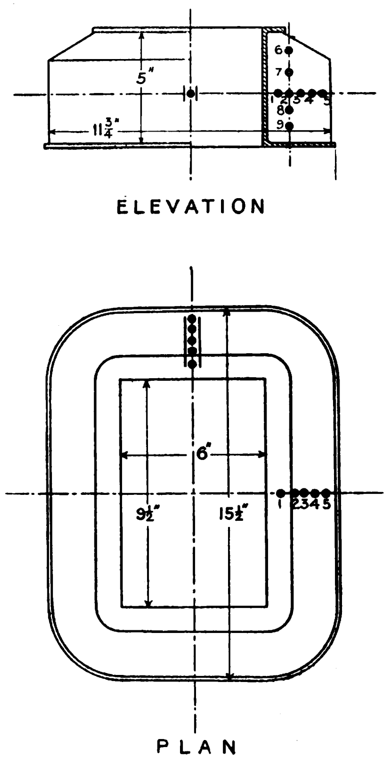
It is of interest to quote the temperature readings of this coil when tested at the National Physical Laboratory, for, being circular, and having three transverse sections with thermo-junctions in similar positions, they were a rigid check on one another.

The sections gave the following figures, counting from the core to the outside :—

TEMPERATURES IN °C.

116·7*	116·3	116·5
121·4	121·2	
120·0	120·0	119·8
113·2	113·0	
100·1	100·1	100·1

* See Curve C, transverse Section.



Layers 7·7·7·7·7·7 Total 42.
Sheet Iron Former Coil varnished on the Outside

Thermo-junctions indicated thus ⊞ were only employed to check the results shewn in the accompanying curves

COIL No. 3.

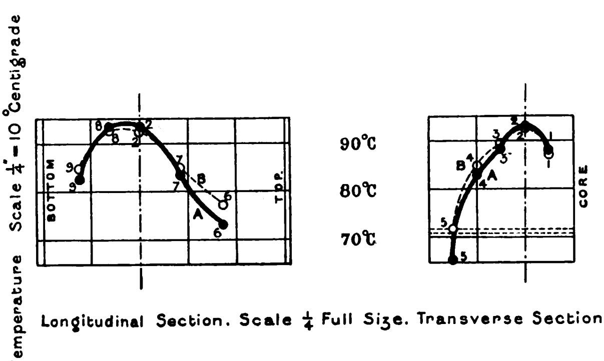
The longitudinal section, counting from the bottom upwards, gave the following : 95·2° C., 121·4° C., 105·7° C. (Curve C.)

Through a misunderstanding, the thermo-junctions in this coil had been soldered direct on to the copper wire instead of being carefully insulated from the circuit of the coil. This fact has not, however, affected the accuracy of the readings, the only precautions necessary being the careful insulation from earth of all apparatus used in carrying out the tests.

This machine, when running light, took five hours to reach a steady temperature, but as it was not cold when starting on full load no figures as to the time it took to reach a steady temperature can be given.

Coil No. 3.—This was a field coil of a six-pole motor. The output was 75 k.w., the voltage 500, and the speed 600 r.p.m. The coil was of irregular shape wound on a sheet-iron former and varnished on the outside. The normal current was 2 amperes, which during the test (3.B.) at the National Physical Laboratory was reduced for the reason mentioned previously. In this case the distribution of temperature was very similar to that produced by the full current under running conditions (3.A.) observed at the Works.

The experiments showed quantitatively, to some extent, by how



Curve	Conditions	Amps.	Mean Temp. in °C	Max.-Mean in °C
A	Full Load	2·0	70·3	23·5
B	Standing	1·68	71·8	20·8

Curve B is shown thus --o-- to avoid confusion with curve A.

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines

Coil No. 3.

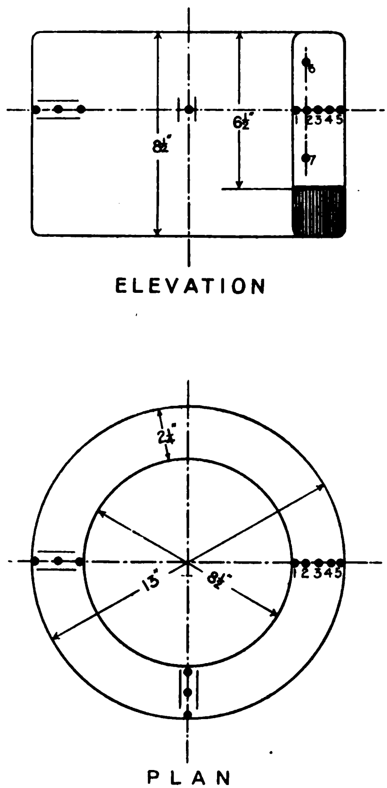
much the cooling of the coil was more efficient under running conditions than when deprived of the fanning action of the armature.

The peculiar shape of the temperature curves was partly due to the irregular shape of this coil, but more so to the use of two sizes of wire, the diameters being 0·048 in. and 0·052 in. This caused a relatively

higher temperature to occur near the core in the thinner wire owing to its higher resistance.

A practical constant temperature was reached in four and a half hours.

Coil No. 4.— A coil belonging to a compound-wound, six-pole,



Layers. 1•7•8•8•7•1. Total 32.
Series Winding 12½ turns of Copper Strip
Coil covered with 3 thicknesses of varnished Cambric and 4 thicknesses of Webbing.

Thermo-junctions indicated thus ⫲ were only employed to check the results shewn in the accompanying curves

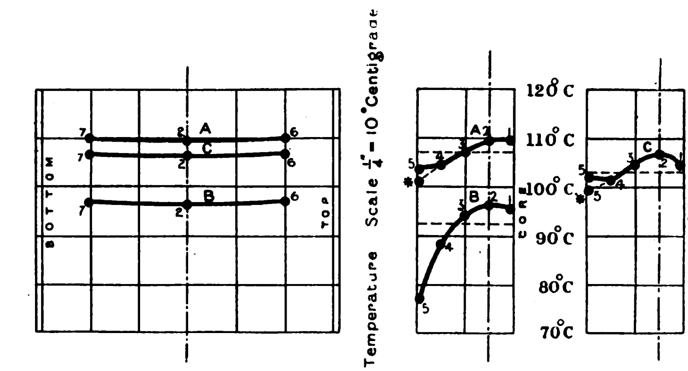
Coil No. 4.

semi-enclosed motor, rated at 100 B.H.P. at 500 volts, taking a current of 165 amps. The coil was covered with three layers of varnished cambric and four thicknesses of boot webbing. The rated voltage per pole was about 83, and when the coil was tested at the National Physical Laboratory, without a core, 86 volts were applied (4.C.). On

the running test the motor was driving an air-compressor for several hours a day, the load varying from half to full load about once a minute, the average voltage per coil being about 75 (4.A.).

A second test on this coil was carried out under running conditions, but in this case the tape covering, consisting of three thicknesses of varnished cambric and four of boot webbing, was removed and a considerable fall in temperature resulted (4.B.).

The anomaly in the curve was due to the local heating effect pro-



Longitudinal Section. Scale ¼ Full Size. Transverse Sections.

Curve	Conditions	Amps	Mean Temp in °C	Max.-Mean in °C
A	Running Loaded	1 52	107 0	3 3
B	Running Loaded	1 54	92 7	4 1
C	Standing	1·72	103 0	3 8

* Points obtained by the Thermo-junctions used for checking purposes

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines

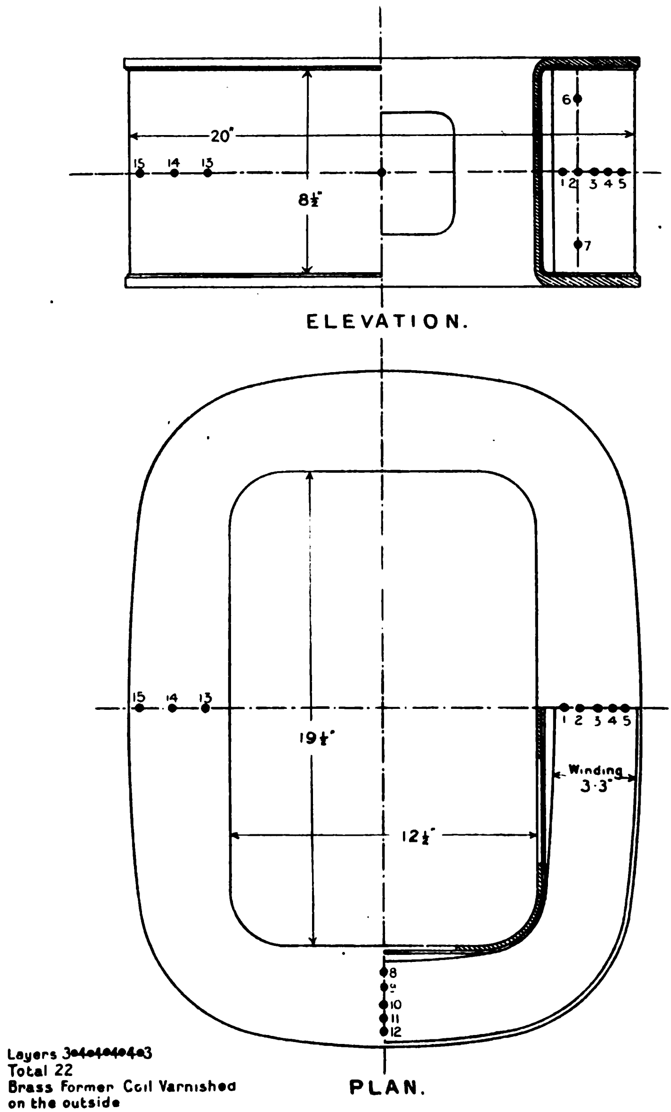
COIL NO. 4.

duced by a large brass terminal plate held in place by the taping on the outside of the winding. The dotted line in the curve shows the distribution of temperature obtained from a corresponding point at another cross-section. Removing the tape and loosening the brass plate produced normal cooling, as shown in the curve (4.B.). The almost linear curves on the longitudinal section were apparently due to the heating caused by the series winding at the bottom, and to some extra press-spahn at the top.

This was the only coil tested which was wound with single cotton-covered wire.

Coil No. 5.—A field coil belonging to a four-pole generator of about 150 k.w., the output being 700 amps. at 210 volts, and the speed 180 r.p.m. The coil was varnished on the outside.

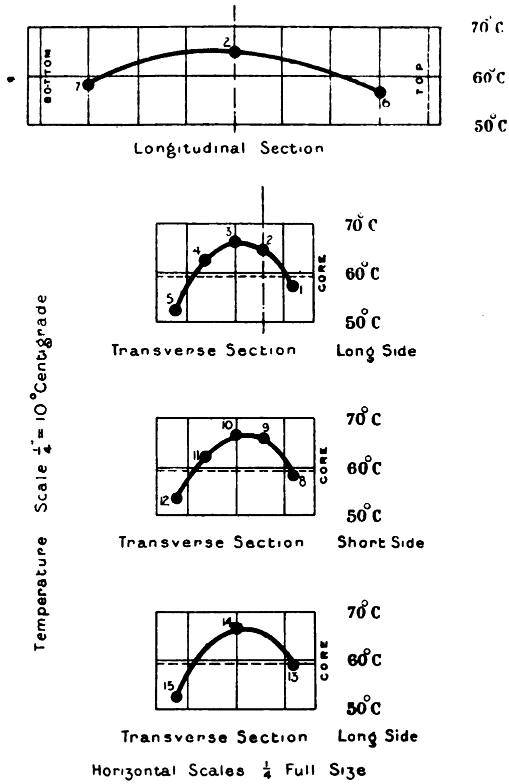
Only one test could be carried out on this coil, as the machine to which it belonged was in constant use on the load of a large machine



COIL No. 5.

shop. The observations were taken after a run of twenty-four hours. During the night the load on the dynamo was about 300 amperes, but in the morning this increased to 600 amperes, and was about 500 when

the final readings were taken. As the shunt current had to be altered to suit the different loads, the temperatures observed were less than would have been the case had full load been maintained constant. The



Conditions	Amps	Mean Temp in °C	Max-Mean in °C
Running Loaded	9·2 (Variable)	59 · 5	7 · 5

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines.

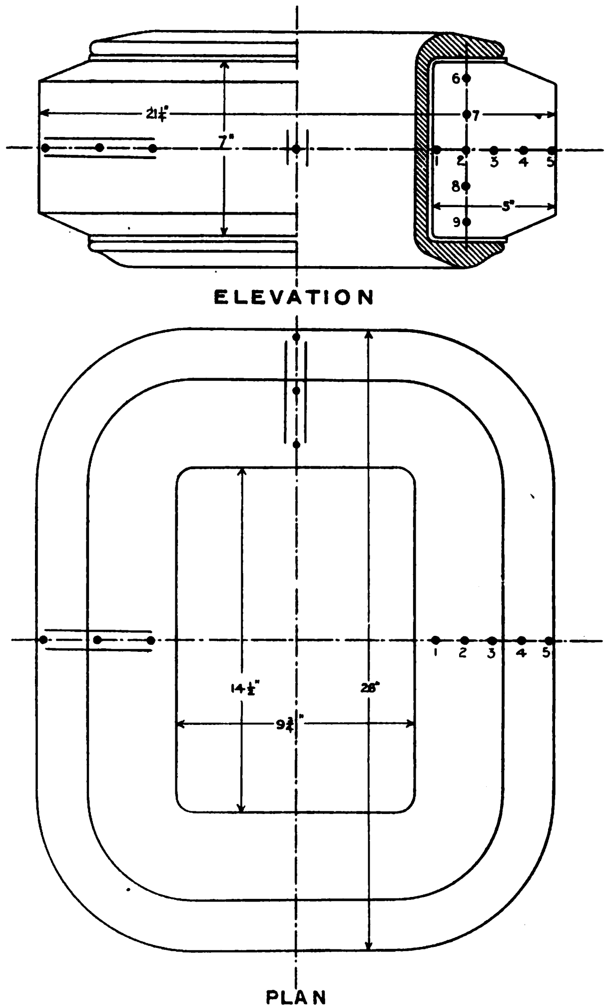
COIL NO. 5.

formers were arranged to secure considerable cooling effect on the core side, which is well shown by the curves giving the temperature distribution at both the transverse and longitudinal sections.

Coil No. 6.—This coil, which was varnished on the outside, belonged to a 200 k.w. generator at 220 volts, running at 350 r.p.m.

The load on the generator when the running test was carried out

was a little over half load, being that of an engineering workshop. The rated full load shunt current was 11·4 amperes, and whilst running



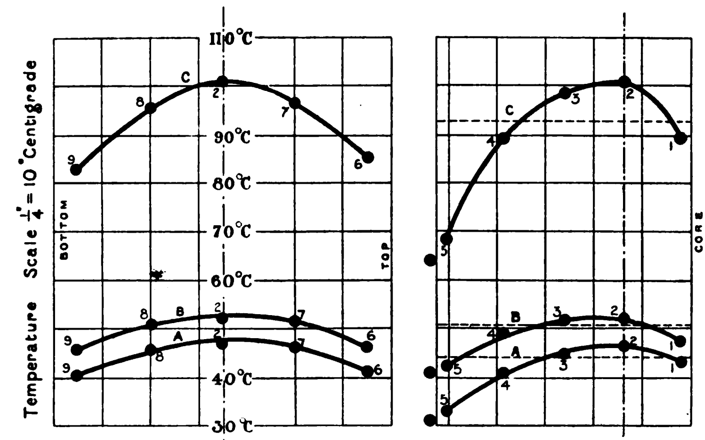
Layers 1•8•8•8•8•1, Total 34
Cast Iron Former. Coil varnished on the Outside.

Thermo-junctions indicated thus ⊡ were only employed to check the results shewn in the accompanying curves.

Coil No. 6.

the current was 7 amperes. The coil was also tested at 7 amperes with the armature not revolving (6.B.).

The results of the test at the National Physical Laboratory with 11·4 amperes are also given (6.C.).



Longitudinal Section. Scale 1/4 Full Size. Transverse Section

Curve	Conditions	Amps.	Mean Temp: in °C	Max:-Mean in °C
A	Running Loaded	7·07	44·0	2·4
B	Standing	7·09	50·2	2·1
C	Standing	11·12	92·5·	8·0

The points shewn to the left of the Curve above represent readings of temperature taken by thermometer outside the Coil.

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines.

Coil No. 6.

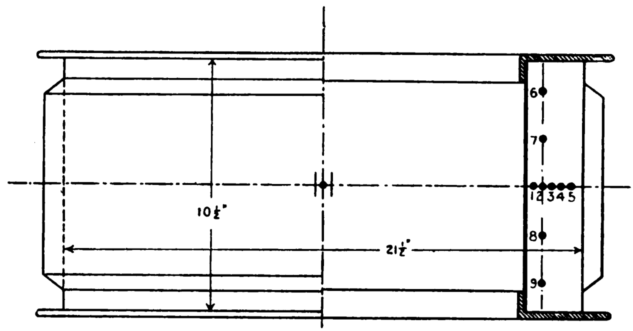
Coil No. 7.—A coil for an eight-pole, compound-wound generator of 200 k.w., the output being 436 amperes, 460 volts, and 108 r.p.m.

The series windings on the various coils were in parallel, and consisted of 43 turns in two layers of flexible conductor wound outside the shunt coils and nearly covering them. The coil was varnished on the outside.

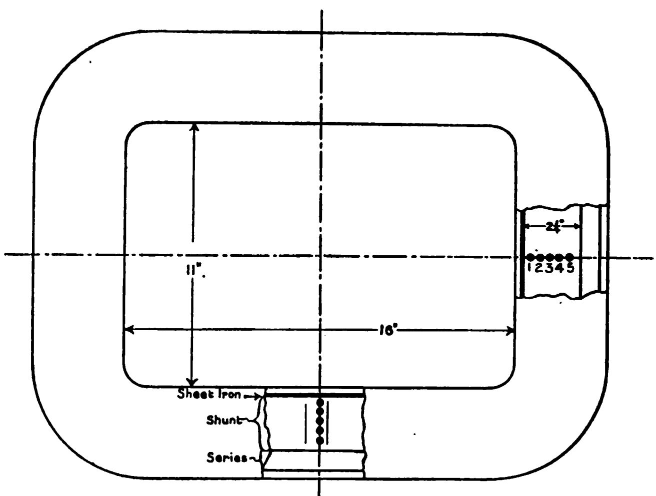
The coil was first tested on the machine coupled to a similar machine for an ordinary input-output test, the test coil being on the motor, the shunt current being 5·2 amperes. The dynamo shunt current was 6·5 amperes, and the boosting current was 80 amperes. The curves (7.A.) obtained were not the same as if the coil had been on the dynamo. The coil was also tested at the National Physical Laboratory with the core, but without any current in the series winding. The current was 5·2 amperes in one test (7.B.) and 6·5 amperes in the second test (7.C.).

It will be noted, that contrary to similar tests on the other coils, with 5·2 amperes a lower temperature was obtained when standing alone than when the coil was subjected to the fanning action of the

armature, the cooling action of the armature being more than compensated for by the heating of the series coils.



ELEVATION



PLAN

Layers 2•3•3•3•3•4. Total 18
Former of malleable & Sheet Iron
Series Winding outside.
Exterior surface varnished.

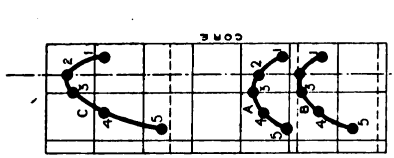
Thermo-junctions indicated thus ▪▪ were only employed to check the results shewn in the accompanying curves.

COIL NO. 7.

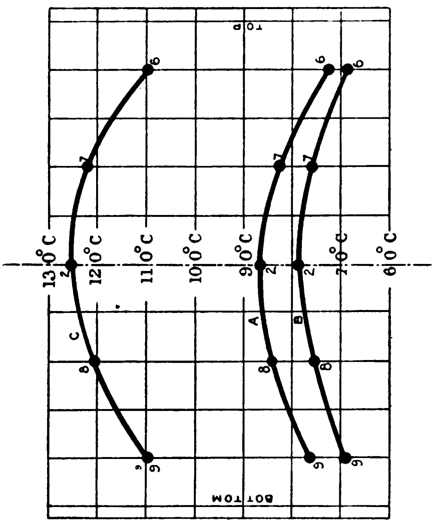
As the curves clearly show, there was considerable air space between the coil and the core.

A diagram is given showing the following temperatures during the run :—

(i.) Maximum temperature by thermo-junction.
(ii.) Mean temperature by rise in resistance.



Transverse Section

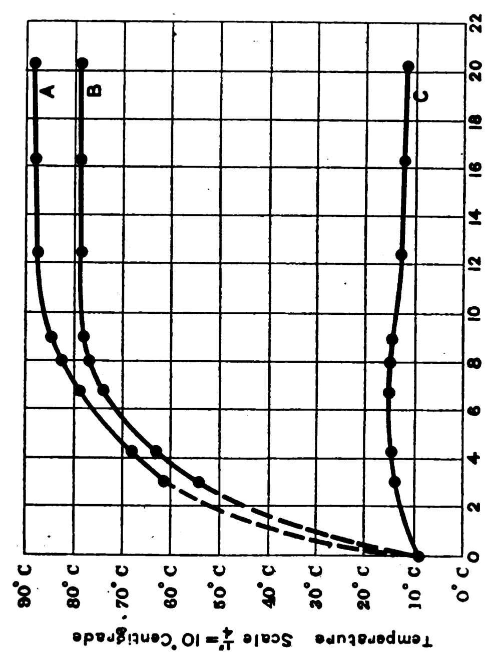


Longitudinal Section. Scale 1/4 Full Size.

Curve	Conditions	Amps:	Mean Temp: in °C	Max.-Mean in °C
A	Running Loaded	5·20	78·5	9·6
B	Standing	5·20	62·0	16·4
C	Standing	6·54	104·5	21·7

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines

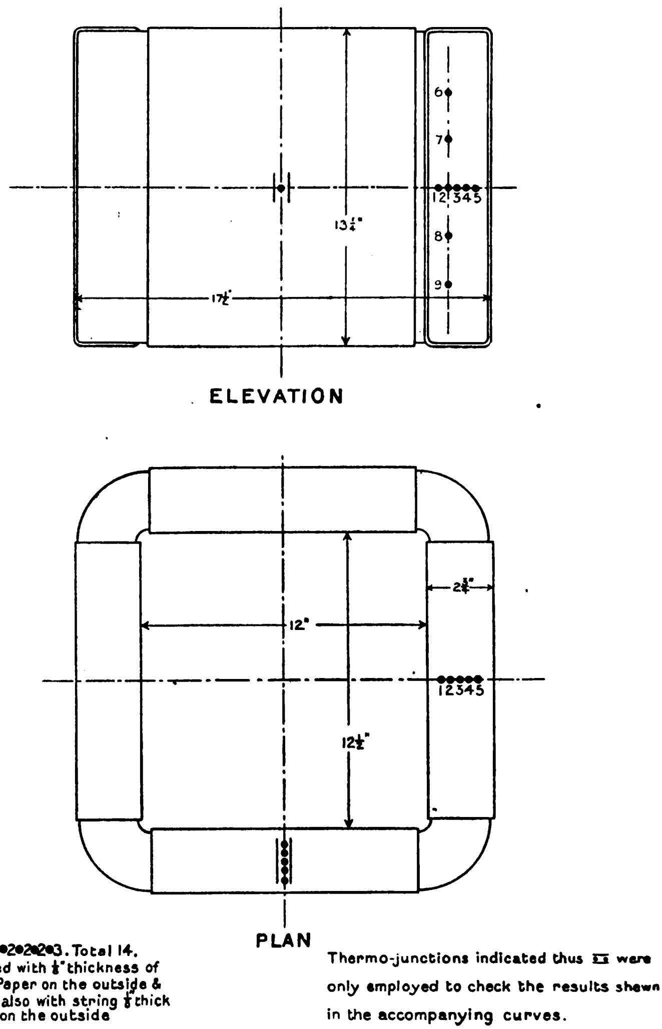
Coil No. 7.



Curve A - Highest temperature by Thermo-junction
Curve B - Mean Temperature from Resistance
Curve C - Temperature of the air

Coil No. 7.

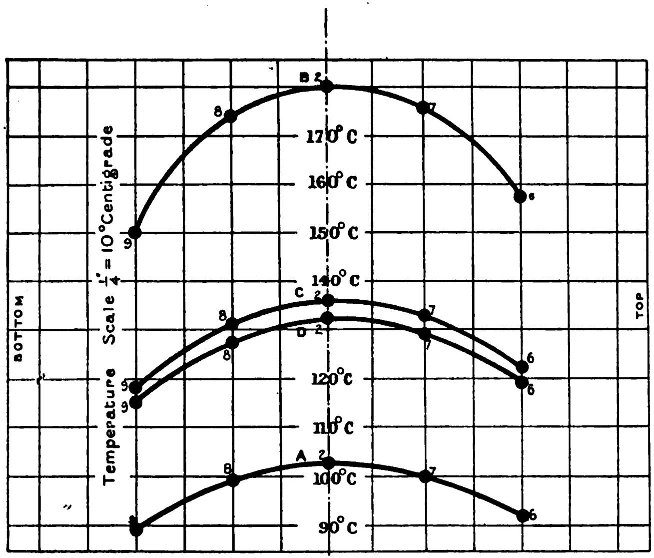
This was one of the largest coils tested, and steady conditions of temperature were reached in about twelve hours, as shown in the curves.



COIL NO. 8.

Coil No. 8.—A field coil of a 500 k.w. generator at 500 volts and 320 r.p.m.

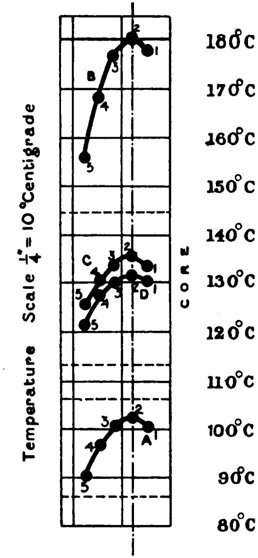
The shunt current was about 8 amperes, and the coil was covered with ⅛ in. of canvas, paper and leatheroid, and ⅛ in. of string, the whole varnished on the outside. When tested at the National Physical Laboratory with the core in place, the current in the first test (8.D.)



Longitudinal Section. Scale ¼ Full Size.

Curve	Conditions	Amps:	Mean Temp: in °C	Max: Mean in °C
A	Running Loaded	8·20	86·0	16 · 0
B	Running Loaded	11·1	144·0	36 0
C	Standing	8·12	113·0	22 · 4
D	Standing	7·60	106·3	25 · 3

Coil No. 8.



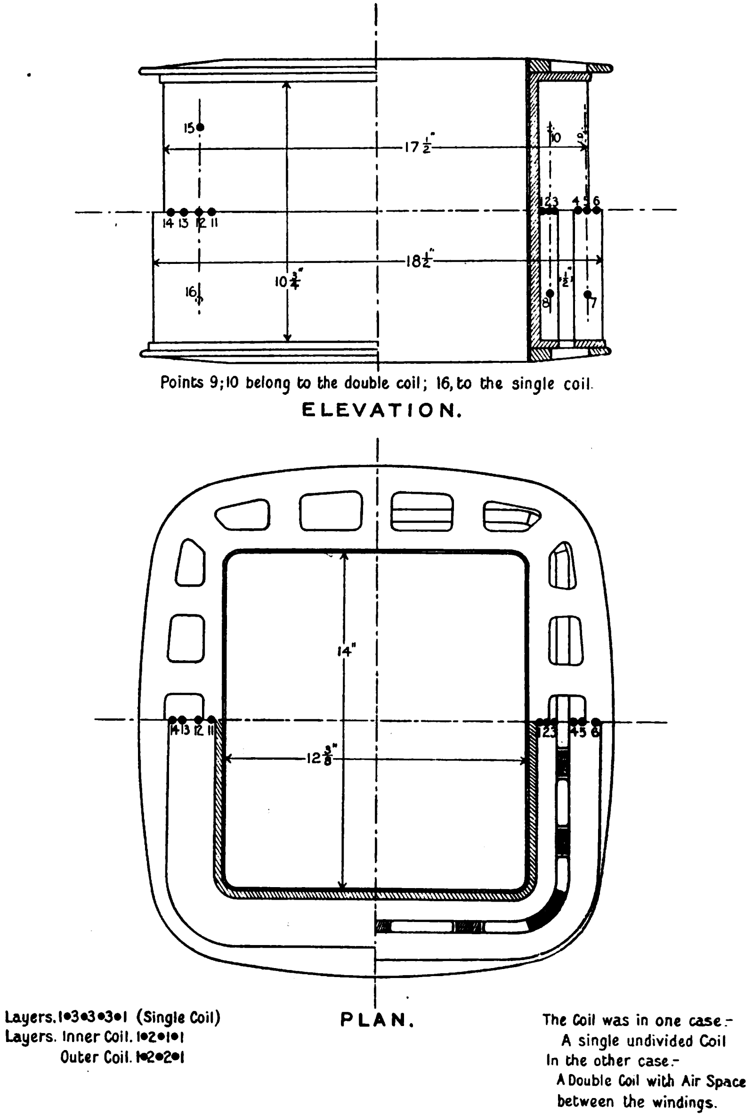
Transverse Section. Scale ¼ Full Size

Curve	Conditions	Amps:	Mean Temp: in °C	Max: – Mean in °C
A	Running Loaded	8·20	86·0	16 · 0
B	Running Loaded	11·1	144·0	36 · 0
C	Standing	8·12	113·0	22 · 4
D	Standing	7·60	106·3	25 · 3

Mean Temperature of the Whole Coil shewn by Horizontal dotted Lines

Coil No. 8.

was 7·6 amperes, and in the second test (8.C.) it was 8·1 amperes. The rest of the coils of the machine on which this coil was tested, were of a



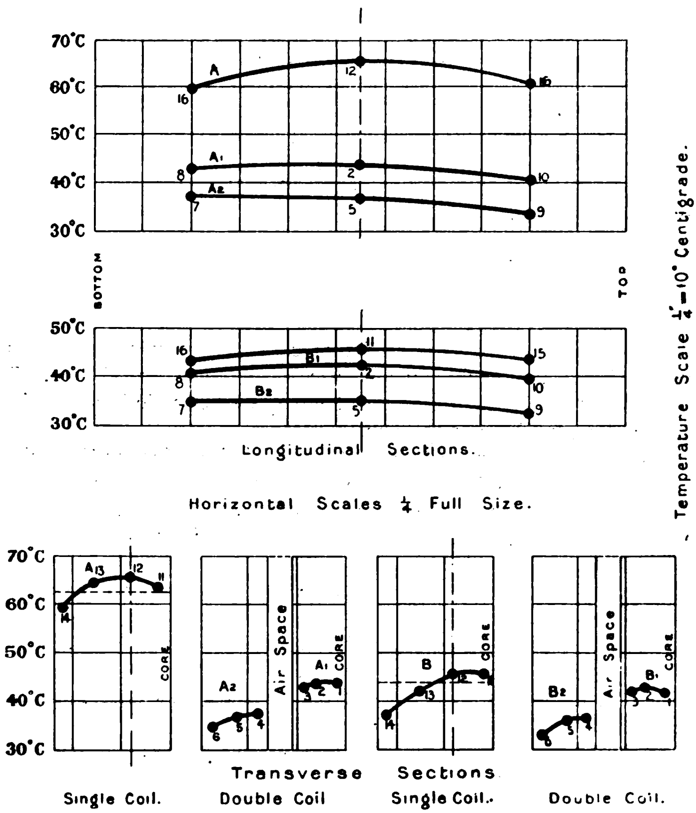
COIL NO. 9.

different design, involving a current of 11 amperes instead of the 8 amperes for which this coil was designed. The resulting maximum temperature was 180° C. (8.B.). At the close of the test run an external thermometer on the field coil registered 103° C.

On a second test run the shunt current was adjusted to 8·2 amperes the normal value, the results of which are given in the curve (8.A.).

This machine was run on full load during the tests.

The coil had an unusual amount of covering in the way of canvas, string, etc., and no doubt this considerably affected the temperature rise.



Curve	Conditions.	Amps.	Mean Temp: in °C	Max.-Mean in °C
A	Single Coil. Normal Covering	19·0	63·2	2·2
B	Single Coil. Covering Removed	"	44°C	1 7

COIL NO: 9.

Coil No. 9.—A field coil of a 500 k.w. generator at 200 volts and 200 r.p.m.

The shunt current was about 14 amperes and this coil was covered with canvas, fullerboard, millboard and string. The generator could not be run on load, and as with a shunt current of 14 amperes very little temperature rise was perceptible after two hours, the current was

raised to 19 amperes and the machine run until a steady temperature was obtained, the results being shown in the curve (9.A.). In addition to this coil wound in the ordinary manner, a second coil on the same machine was wound with an air space in the middle of the winding, in order to see if any economy might be effected in the amount of copper used. The total layers on the coil were eleven, with a half-inch air space between the fifth and sixth layers. The curves ($9.A_1.$) and ($9.A_2.$) give the figures for the heat distribution in the inner and outer portion respectively. As this coil had no external covering a second test (9.B.) on the other test coil, was made with the covering removed so as to make a fair comparison between the two.

The reduction in temperature rise is well shown in curve (9.B.). The corresponding results ($9.B_1.$ and $9.B_2.$) from the coil with the air space were identical with the results obtained in the previous test and show that the conditions of test were unaltered. The air space did not have very much effect on the temperature rise, due in all probability to the thickness of the wire and comparatively small number of layers of cotton insulation. Of greater practical interest would be the comparison of the temperature rise of two coils of fine wire with a large number of turns, one with an air space and the other without. The extra cooling might enable some economy to be made in spite of the increased length of the mean turn.

This concludes the report on the tests carried out for the Committee, but it will perhaps be useful to recapitulate the main points of importance with reference to the various coils.

Coils Nos. 3, 5, 6 were wound on metal formers and merely varnished on the outside.

Coil No. 7 was wound on a metal former, but covered to a great extent by the series winding.

Coils Nos. 1α, 1β, 1γ, were wound on temporary formers and finished with tape and other insulating materials.

Coil No. 4. The series winding on this coil was enclosed in the taping.

Coil No. 8. A layer of string was added to this coil.

Coil No. 9 was wound on a metal former, protective coverings being used.

All coils except No. 4 were wound with double cotton-covered wire, the latter being wound with single.

The figures in Column No. 18 of Table No. 2 give the essential results of the investigation, and it will be noticed that the figures for the difference between the maximum and the mean temperature vary from about 25° C. downwards. A value of 0·428 per cent. per degree Centigrade has been assumed for the temperature coefficient of the resistance of copper. This has been confirmed in the case of Coil No. 3. The somewhat small difference between the maximum temperature found by thermo-junction and the mean temperature, calculated from the increase of resistance, which is noticeable in a few of the experiments, may be due to this coefficient not being exactly the value assumed.

Explanatory Notes to Drawings.

The coils have been designated by numbers and outline drawings have also been given.

In each case the drawing of the coil gives a plan, a half-sectional elevation viewed in a line with the axis of the machine, the armature revolving below.

The positions of the thermo-junctions are indicated thus:—3·4·4·4·4·3, which signifies that starting from the core side, the temperature has been measured between the 3rd and 4th, the 7th and 8th, 11th and 12th, 15th and 16th, 19th and 20th layers, and that there were three layers outside.

The drawings are to the same scale throughout, and are therefore an indication of the relative size of the coils.

Explanatory Notes to Table No. 1.

Line No. 1. Coil number.
" 2. Dynamo or motor, shunt or compound wound.
" 3. Number of poles.
" 4. Normal load in kilowatts.
" 5. " volts.
" 6. " amperes.
" 7. " revolutions per minute.
" 8. Diameter of armature in inches.
" 9. Dimensions of coil in inches, parallel to machine axis.
" 10. " " " perpendicular to machine axis.
" 11. " " " length.
" 12. " " " breadth.
" 13. Wound on metal former or not.
" 14. Nominal size of wire.
" 15. Sectional area of wire in sq. inches.
" 16. " " " sq. mms.
" 17. Number of layers.
" 18. Turns per layer.
" 19. Total turns.
" 20. Outside area, including flanges, but excluding surface next to core, in sq. inches.
" 21. Do. in sq. cms.
" 22. Volume of winding in cubic inches.
" 23. " " " cubic cms.

The above refer to properties independent of the experiments.

Explanatory Notes to Table No. 2.

Column No. 1. Coil number.
" 2. Test number.
" 3. Resistance of coil cold, in ohms at 0° C.
" 4. Resistance of coil hot, in ohms.
" 5. Nominal amperes.
" 6. Amperes in coil during test.

Column No. 7. Experiment carried out with machine running, or on coil with its core, or alone, *i.e.*,

$\frac{\text{R. at F.L.}}{325}$ = Running at 325 r.p.m. at full load.

$\frac{\text{R.L.V.}}{180}$ = Running at 180 r.p.m., load varying.

$\frac{\text{R. Light}}{310}$ = Running light at 310 r.p.m.

$\frac{\text{S.}}{\text{No core}}$ = Tested standing without the core.

„ 8. Amperes per sq. inch.
„ 9. „ per sq. mm.
„ 10. Watts.
„ 11. Watts per sq. inch.
„ 12. „ „ sq. cm.
„ 13. „ „ cubic inch.
„ 14. Temperature of air during test in ° C.
„ 15. Temperature on coil by thermometer in ° C.
„ 16. Maximum internal temperature ° C.
„ 17. Mean temperature in ° C. calculated from the $R_\theta = R_0 (1 + \cdot004 28\theta)$.
„ 18. Maximum minus mean temperature.
„ 19. Maximum minus air temperature.
„ 20. $\frac{\text{Max.—Air temp.}}{\text{Watts per sq. inch.}}$
„ 21. Mean minus air temperature.
„ 22. $\frac{\text{Mean—Air temp.}}{\text{Watts per sq. inch.}}$

In all cases temperatures given are actual temperatures in degrees Centigrade, and only where stated do the figures represent temperature rise.

The figures given in Columns 20 and 22 are inversely proportional to the emissivity.

APPENDIX.

The results of the previous experiments have given some information as to the actual temperature reached in the field coils of electrical machinery. It seemed, however, desirable to obtain some information as to the actual temperature up to which cotton may be used.

From experiments made on samples of cotton-covered wire, there appeared to be no serious alteration in the cotton up to about 125° C.; above this, however, the cotton became darker, till at the highest temperature (180° C.) to which samples were subjected for a prolonged period, it became nearly black, but even then, from the electrical point of view, it proved to be an excellent insulator as compared with cotton at atmospheric temperature. Practically no leakage was caused by the carbonisation of the cotton when it had become almost black.

It is hardly possible to make any definite statement as to a limiting temperature for cotton-covered wire, but judging merely by change in colour, the temperature of 125° C., mentioned above, may be taken as approximately the safe limit.

This was, of course, a difficult matter to test, and whilst endeavouring to do so, a method was used which gave surprising results, not, however, on this point, but on one which it is hoped may prove of special interest.

A glass tube, 1½ in. diameter, and about one foot long, was wound with about 120 turns of No. 18 double cotton-covered wire; on this a second layer of 100 turns was wound, thus forming two solenoids. These were electrically separate, except for the leakage from the one layer to the other, through the two layers of double cotton covering.

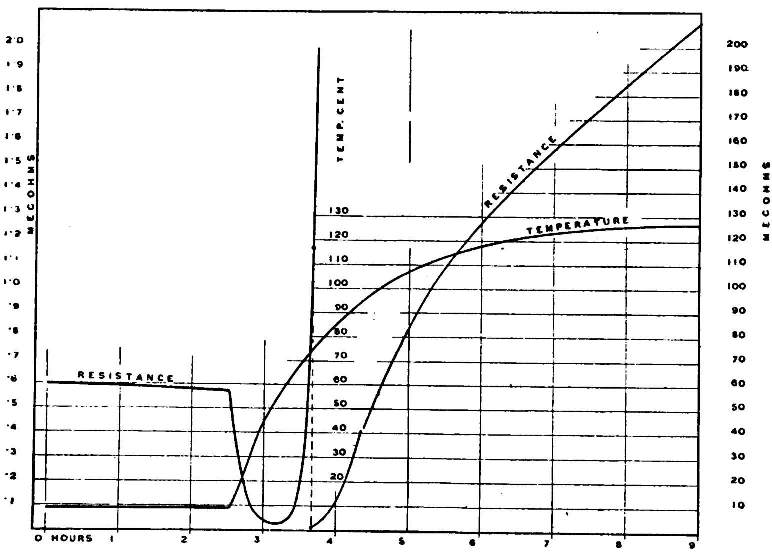


DIAGRAM D.

The tube was suspended horizontally in a larger brass tube, which was covered with several layers of asbestos paper. On this a spiral of high-resistance wire was wound, forming a heating circuit. Outside this and at the ends of the tube magnesia covering was used, metallic connection being made through the covering to the two copper wires, so that the insulation resistance between the two circuits could be continuously measured. The temperature was taken by an ordinary thermometer.

The first portion of the diagram shows the result of raising the temperature from that of the atmosphere up to about 126° C. in six hours. On account of the great change in the insulation resistance and length of time, various vertical and horizontal scales have been used.

The first effect on raising the temperature was a rapid drop in insulation resistance from about 0·6 megohm to 0·025 megohm in 40 minutes, followed by an even more rapid rise to 1·17 megohms in

70 minutes from the commencement of heating, and to 210 megohms in 9 hours. The rapid drop in the curve was doubtless due to the presence of moisture driven out of the cotton by the rise in temperature, resulting in the formation of a conducting layer on the surface of the cotton, which soon evaporated, and so the insulation resistance rapidly rose.

If, however, the moisture were unable to evaporate easily, as is generally the case in a field coil, a serious leakage current would be the result. This would probably be very noticeable when there is a considerable voltage per pole, as frequently occurs in the case of modern small four-pole, 400 volt motors. The leakage current would gradually diminish and finally disappear as the moisture in the coil dried out, but

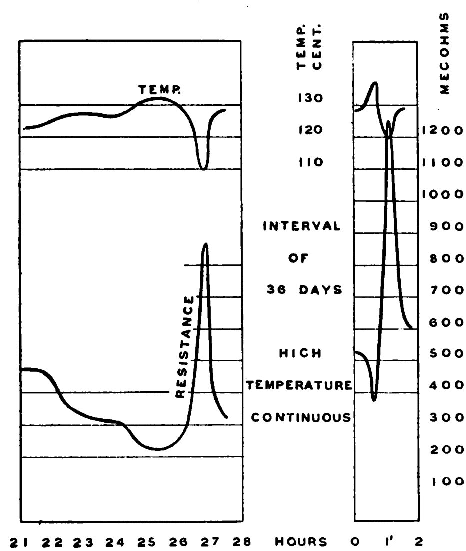


DIAGRAM E.

if the electrolysis were considerable the coil might be damaged, the leakage continuing in spite of many days' stoving, as occurred in the case of Coil No. 1.

The second portion of the diagram Fig. E shows the effect of a variation of the temperature at about 21 hours from the start, and the curve clearly indicates how exceedingly sensitive to changes in temperature the insulation resistance is; the shape of the two curves is very similar, as may be seen by inverting one of them.

The third portion shows a similar and more rapid change of temperature 36 days from the commencement, the oven being in the meantime kept at a temperature of about 115° C. to 140° C. There is obviously no change in this property of the cotton. This change of resistance is, however, the same as has previously been found in other insulating substances.

The amount of moisture present in the cotton is necessarily a matter

of interest, and a sample of the best quality cotton, such as is actually employed for covering wire, kindly supplied by Messrs. The London Electric Wire Co., Ltd., was subjected to a temperature of about 120° C., the cotton being in the form of a cop weighing about 30 grammes. The following figures were obtained :—

Loss after heating	0·5 hour	5·10 per cent.
,, ,, ,,	1·0 ,,	6·52 ,, ,,
,, ,, ,,	3·0 ,,	6·93 ,, ,,
,, ,, ,,	15·0 ,,	6·93 ,, ,,

After this the cotton was exposed to the atmosphere, and the following figures show the gain in weight which took place :—

After	0·5 hour	... ...	0·68 per cent.
,,	1·0 ,,	... ...	1·20 ,, ,,
,,	3·0 ,,	... ...	2·38 ,, ,,
,,	15·0 ,,	... ...	4·23 ,, ,,
,,	14 days	... ...	6·34 ,, ,,

This point has been dwelt on at considerable length on account of the fact that very little appeared to be known as to the possible leakage through cotton not thoroughly dried. Inquiry at various manufacturers has elicited the information that the characteristic curves obtained from machinery installed after standing exposed to the weather, subsequent to manufacture, have not been in accord with those obtained on test. This has usually been put down to faulty instruments, but in all probability it was the result of moisture. This theory is, to a certain extent, borne out by the fact that in one case in which the observations were repeated after some time had elapsed, an intermediate curve was obtained, showing that a partial drying out of the coil had taken place.

The author is glad to be able to acknowledge his indebtedness to Dr. Glazebrook, in his double capacity as Chairman of the Sub-Committee (for whom this work has been done), and as Director of the National Physical Laboratory; and also to many of the Laboratory assistants for their help and advice, among whom he would especially desire to mention Mr. Campbell and Dr. Harker.

He also desires to take this opportunity of personally thanking the various firms, which have partaken in these tests, for their hospitality; and the members of their staffs, who have, with invariable kindness, materially assisted in the investigation.

TEMPERATURE CURVES AND THE RATING OF ELECTRICAL MACHINERY.

By RUD. GOLDSCHMIDT, Associate Member.

(*Paper read March* 9, 1905.)

I.—SHORT TIME RATING.

It is a well-known fact that the energy lost in electrical machines is converted into heat, which causes the temperature of certain parts to rise above that of the surrounding atmosphere, and results in a dissipation of heat by means of radiation or convection. If no dissipation took place at all, the temperature would simply continue to increase in proportion to the time until either the insulation was burnt or the metal was melted. Given the specific heat of the material, the calculation of the temperature rise under any given conditions is a very simple matter, and in the case of the two materials, copper and iron, since their specific heat is nearly the same, we may assume an average value.

As a thermal unit, the kilowatt-hour or watt-second is a more convenient one than the B.T.U. to adopt, and by this means we obtain the following practical definition of the specific heat of copper and iron :—

"3·5 watts lost in 1 cubic centimetre of copper or iron raises its temperature 1 deg. C. per second, supposing that no radiation takes place," or in British units—"32 watts lost in 1 cubic inch of copper or iron raises its temperature 1 deg. F. per second."

If we call the watts lost in the unit volume W_v; the initial temperature increase per second V, and the watts per unit of volume for the unit of temperature increase per second, that is, 3·5 in c.g.s., and 32 in British units C_v; then

$V =$ initial temperature increase per second,
$W_v =$ watts per unit of volume,
and $C_v =$ rise constant.

The connection between these values is then given by the formula :—

$$\text{Temperature increase per second} = \frac{\text{Watts per units of volume}}{\text{Rise Constant}},$$

$$\text{or,} \qquad V = \frac{W_v}{C_v}.$$

In many cases in practice an instrument is used for a few minutes only, and not more than once or twice a day, for instance, with apparatus, such as starting transformers, the choking coils of single-phase motors, the small motors used for starting synchronous motors,

etc. In these cases it is useless to apply the well-known "watts per square inch" rule for calculating the temperature, and it is a mistaken idea in designing these machines, to take into consideration the providing of any cooling surfaces.

The heat developed during the time the apparatus is in use is almost entirely spent in heating up the material, and the average temperature attained is too low, and the time too short to dissipate any appreciable amount. In many other respects, as will be seen later on, it is of much practical value to study closely the behaviour of machines without taking account of the heat dissipated, but considering only "the initial temperature increase per second."

We may suppose a choking coil or starting transformer to be so dimensioned that, if used only once a day for a period of say five minutes (= 300 seconds) as a maximum, the temperature rise in any part will not exceed $16\frac{1}{2}$ deg. C. at the end of that period. The frequency is to be 100 cycles per second, and we may assume that the excessively long starting period of five minutes is due to the fact that heavy weights have to be accelerated with a very low starting current. The temperature increase per second is $\frac{16\frac{1}{2}}{300} = 0{\cdot}055$ deg. C. Since a rise of 1 deg. C. per second is caused by $3{\cdot}5$ watts per cubic cm., the energy lost in 1 cubic cm. must in this case be $3{\cdot}5 \times 0{\cdot}055 = 0{\cdot}19$ watts.

Considering first the question of the heating of iron parts, we have only to look at a hysteresis curve, showing the hysteresis loss per cubic cm. at different flux densities for the particular iron we are going to use. This curve shows that at 100 periods the flux density corresponding to $0{\cdot}19$ watts loss per cubic cm. ($3{\cdot}1$ watts per cubic inch) is about 15,000 c.g.s. per cm². (16 Kapp lines per square inch).

With regard to the heating of the copper winding, we must consider the following facts :—

A copper wire 100 cm. in length and 1 mm.² section has a resistance of about 1/55 ohm ; its volume is 1 cubic cm. To cause a rise in its temperature of $0{\cdot}055$ deg. per second $0{\cdot}19$ watts must be lost in the wire. The current causing this loss in 1/55 ohm can be obtained directly by Joules' law—

$$c^2 \times 1/55 = 0{\cdot}19 \text{ or } C = \sqrt{0{\cdot}19 \times 55} = 3{\cdot}2 \text{ amperes.}$$

The current per 1 mm.² section would therefore be $3{\cdot}2$ amperes, or 2,000 amperes per square inch.

Now to obtain a temperature rise of $16\frac{1}{2}$ deg. C. in five minutes we found that, quite independently of the size and surface of our apparatus, the flux density in the iron must be 16 Kapp lines per square inch, and the current density in the copper 2,000 amperes per square inch.

We have, therefore, with the aid of an example, been enabled to formulate the following rule :—

"Machines, which are to work only for a short time, with interruptions sufficient to allow them to cool down to the temperature of the atmosphere, must be so designed that the flux density and copper density do not exceed a certain amount, independent of size and cooling surface."

We can further conclude from our example that with short-period apparatus, the flux density is practically only limited by the permeability of the iron, and not by the heating, since it is possible even under the severe conditions of a frequency of 100, a five minutes' starting period and a permissible temperature increase of only 16½ deg. C. to work at the high flux density of 15,000 c.g.s. per cm.^{2}

In the example a current density of 3·2 amperes per mm.^{2} correspond to $W_v = 0{\cdot}19$ watts per cubic cm., and to a temperature increase of 0·055 deg. C. per second. As the watts are proportional to the square of the current density we can deduce direct a general rule for calculating volume watts and initial temperature increase from the current density. If we denote the current density by the symbol i, we have—

$$W_v = 0{\cdot}19 \frac{i^2}{3{\cdot}2^2} = 0{\cdot}019 \times i^2 \text{ watts per cubic cm.}$$

$$V \text{ per sec.} = 0{\cdot}055 \times \frac{i^2}{3{\cdot}2^2} = 0{\cdot}0055 \times i^2 \text{ deg. C. per sec.}$$

or,

$$V \text{ per min.} = 0{\cdot}0055 \times 60 \times i^2 = 0{\cdot}33 \times i^2 \text{ deg. C. per min.}$$

For convenient use Tables are appended, giving the temperature rise per minute at different current densities:—

TABLE I.

Heating of Copper.

Amps. per square milli-metre.	Temperature rise degree Cent. per minute.	Amps. per square inch.	Temperature rise degree Fahr. per minute.
0·5	0·08	250	0·09
1	0·33	500	0·36
1·25	0·50	750	0·82
1·5	0·75	1,000	1·45
1·75	1·0	1,250	2·25
2	1·3	1,500	3·25
2·25	1·5	1,750	4·4
2·5	2·0	2,000	5·8
2·75	2·5	2,250	7·2
3	3	2,500	9
3·5	4	3,000	13
4	5·3	4,000	23
5	8·3	5,000	36
6	12	6,000	52
7	16	8,000	93
8	21	10,000	145
10	30	20,000	580
15	75		
20	130		
25	205		

TABLE II.

Heating of Nickeline.

Amps. per square milli-metre.	Temperature rise degree Cent. per minute.	Amps. per square inch.	Temperature rise degree Fahr. per minute.
0·2	0·34	100	0·37
0·4	1·35	300	3·3
0·6	3·1	500	9·2
0·8	5·5	700	18
1	8·5	1,000	37
1·5	19	1,500	83
2	34	2,000	150
2·5	53	3,000	300
3	77	5,000	920
3·5	105	7,000	1,800
4	135	9,000	3,000
5	210		
7·5	480		
10	850		
15	1,900		
20	3,400		

TABLE III.

Heating of Iron.

Amps. per square milli-metre.	Temperature rise degree Cent. per minute.	Amps. per square inch.	Temperature rise degree Fahr. per minute.
0·5	0·5	250	0·55
1	2·0	500	2·2
1·5	4·5	750	5·0
2	8·0	1,000	8·8
2·5	12·5	1,500	19·7
3	18	2,000	35
4	32	3,000	79·5
5	50	4,000	140
7½	112	6,000	315
10	200	10,000	880
15	450	15,000	2,000
25	1,250	20,000	3,500
35	2,450		
50	5,000		

Table II. shows the same figures for nickeline wire with a specific resistance 25 times that of copper, and Table III. gives those for iron with a specific resistance equal to 6 times that of copper. The figures will be found useful in the calculation of starting resistances, the formula being $V = 8{\cdot}5 \times i^2$ deg. C. of temperature rise per minute for nickeline wire, and $V = 2{\cdot}0 \times i^2$ deg. C. for iron.

In determining the section of the wire by means of these data, we are sure of an ample margin of safety as the loss of heat by dispersion has not been taken into account. To be quite accurate, however, a slight increase should be allowed in the temperature values in these tables to compensate for the increase of resistance with rising temperature.

We see that for a starting period of one minute with a temperature rise of 70 deg. C. per minute, the current density of—

$$i = \sqrt{\frac{70}{8{\cdot}5}} = 2{\cdot}9 \text{ amperes per mm.}^2 \ (= 1{,}900 \text{ amperes per square inch})$$

in nickeline wire must not be exceeded. A starting transformer or other short-time apparatus would under these conditions have the current density of—

$$i = \sqrt{\frac{70}{0{\cdot}33}} = 14{\cdot}5 \text{ amperes per mm.}^2 \ (= 9{,}300 \text{ amperes per square inch})$$

in the copper. As already mentioned, the limit of the flux density in the iron is practically unaffected by heating.

There is still to take into account the effect of the insulation In a field coil, consisting of average-sized wire, the cotton insulation forms about 5 per cent. of the copper weight, and owing to the close contact between the wire and the insulation both will quickly attain the same temperature. Considering the field coil as a whole, we have no longer to deal with the specific heat of the copper alone, but with that of copper plus say 5 per cent. cotton. Assuming cotton to have 6 times the specific heat of copper, the average value of the specific heat for the coil is $1 + {\cdot}05 \times 6 = 1{\cdot}30$ times higher than that of copper alone. The average temperature increase per second for a given density might therefore be found to be $\frac{1}{1{\cdot}30} = 0{\cdot}77$. It should be noted that the temperature is lower with field coils of thin wire containing much insulation, and having a higher average specific heat than with those having thick wire.

Up to the present, it has been assumed that the apparatus gives out no heat to the surrounding atmosphere, but the extent to which this assumption is permissible depends on the kind of machine. With a totally enclosed stationary machine the heat dissipated is only a small percentage of that stored in the heated body itself, say during the first hour of heating. The temperature curve up to this point is an absolutely straight line, or, practically speaking, the temperature increase for the second half-hour is very nearly the same as that during the first half-hour. The following are approximate values for the time during which the apparatus can be considered as giving out no heat :—

Revolving machines,	open	... ...	20 minutes.
" "	totally enclosed	... ...	40 "
Stationary "	open	... ...	½ hour.
" "	totally enclosed	... ...	1 "
Starting resistances, and generally, single wires		...	½–2 minutes.

II.—The Temperature Curve.

Having dealt with the initial part of the curve first, we will now proceed to the other extreme. The temperature of an apparatus will be supposed to have attained a constant value. Theoretically, this state can only be obtained after an infinite time, but actually it is so nearly arrived at within a few hours that a small fluctuation in the temperature of the atmosphere leads to perfect constancy. Under these conditions, the temperature reached by the heated body is sufficient to dissipate all the heat developed, and no further storing of heat, *i.e.*, increase of temperature, will take place. This constant temperature is dependent on the lost watts, on the ventilation, on the cooling surface, and on the temperature of the surrounding atmosphere.

For the sake of simplicity we will assume in the whole of the following investigations that the air is at zero temperature. With a given amount of ventilation the difference in temperature between machine and atmosphere is directly proportional to the watts dissipated from one square inch of surface. For instance, in a field coil of an ordinary continuous-current generator, in order to obtain a temperature rise of 90 deg. F. (50 deg. C.) above that of the atmosphere, measured by resistance rise, about 4¾ square inches (= 30 cm.^{2}) cooling surface are required for every watt lost. In other words, a difference in temperature of 90 deg. F. dissipates 0·21 watts from every square inch of surface, or 1/430 watt per square inch per 1 deg. F. rise.

To state this in c.g.s. units: 1/1500 watt is dissipated by 1 square centimetre of surface for an increase in temperature of 1 deg. C.

Henceforward we shall only employ c.g.s. units, and thus apply the factor $\frac{1}{1500}$, which we will denote by the symbol C_h (heating constant).

The watts dissipated per unit of surface we will call "surface watts," symbol, W_s.

Surface watts = heating constant × temperature difference :—

$$W_s = C_h \times t.$$

With the definitions of volume watts (W_v) and surface watts (W_s), the rise constant (C_v) and heating constant (C_h), it will be possible to solve any problem in connection with the temperature rise of electrical machines, for intermittent or continuous load, without complex mathematical calculations. And it is proposed to follow the method previously employed of demonstration by the aid of a definite example, which will take the form of a field coil with a heating constant $C_h = 1/1500$ and a rise constant $C_v = 4·5$.

In determining C_v, the cotton insulation has been allowed for by increasing the figure from 3·5 to 4·5. The surface watts are assumed

to be $W_s = 0{\cdot}033$ watts per square centimetre, corresponding to a final temperature—

$$t_f = \frac{{\cdot}033}{1/1500} = 50 \text{ deg. C.}$$

The current density may be 1·7 amperes per mm.²

The initial temperature increase according to Table I., without taking into account the cotton insulation, would be 0·71 deg. C. per minute. We have assumed in this case that the cotton insulation raises C_v in the ratio 4·5/3·5. Consequently—

$$V = 0{\cdot}71 \times \frac{3{\cdot}5}{4{\cdot}5} = 0{\cdot}55 \text{ deg. C. per minute.}$$

When the final temperature of $t_f = 50$ deg. C. is reached, the heat developed, *i.e.*, that amount which would increase the temperature 0·55 deg. C. in one minute, neglecting cooling, is completely dissipated in

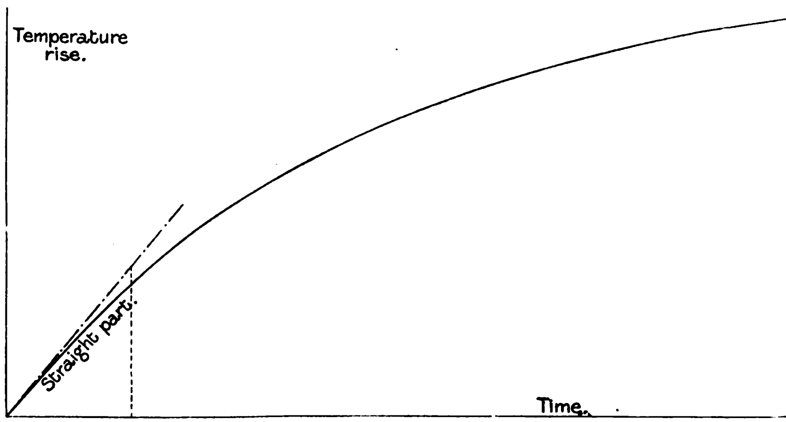


FIG. 1.

the atmosphere in the same time of one minute. The whole of the 0·55 deg. C. rise per minute is neutralised by the cooling action.

From this fact we may deduce the method for calculating the temperature at any desired instant. For example, at a given moment the body may have a temperature of say $t = 35$ deg. C. in excess of that of the atmosphere. Then the cooling action is less than that in the final state in the ratio 35/50, and thus compensates for $35/50 \times {\cdot}55 = 0{\cdot}39$ deg. increase per minute. These 0·39 deg. are completely neutralised by the cooling action, therefore, at the instant when the temperature of the body is 35 deg. C. in excess of the air temperature, the temperature increase per minute is $0{\cdot}55 - 0{\cdot}39 = 0{\cdot}16$ deg. C. Thus we obtain the rule: "If the temperature of the body has attained say a per cent. of the final temperature, the increase of temperature per minute is $(100 - a)$ per cent of the initial increase."

Assuming that the temperature at a certain instant was known to be 35 deg. C., this rule enables us to calculate the temperature, say 20 minutes later. At the beginning of this 20 minutes period the rise is,

as we have just seen, 0·16 deg. per minute. If the rate of increase were the same right through, after 20 minutes the temperature would be $0·16 \times 20 = 3·2$ deg. C. higher, that is, it would now be 38·2 deg., but the average temperature during the 20 minutes would be $\frac{38·2 + 35}{2} =$ 36·6 deg., so that the corrected rise is $\frac{0·55}{50}(50 - 36·6) = \frac{1}{91} \times 13·4 =$ 0·15 per minute, or a total of $0·15 \times 20 = 3$ deg. The corrected temperature after 20 minutes would accordingly be $35 + 3 = 38$ deg. This value differs so little from the uncorrected one (38·2 deg.) that further correction is unnecessary.

The full temperature curve may be calculated in the following manner :—

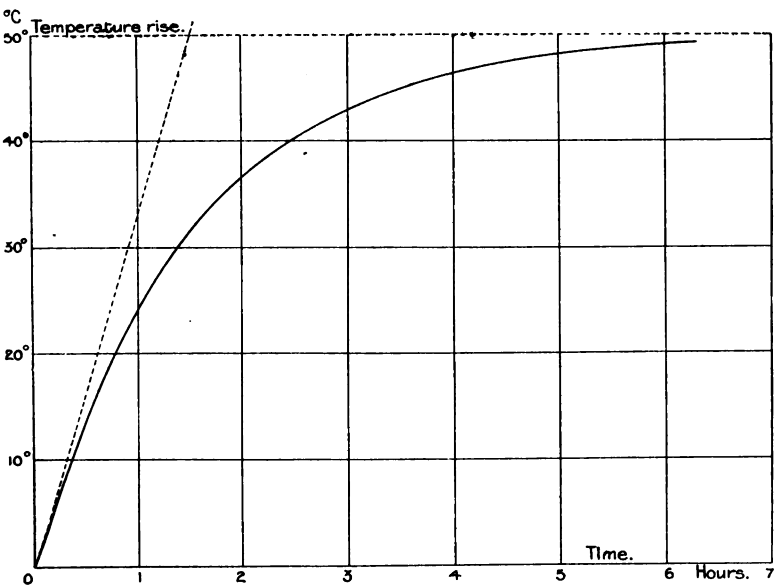


FIG. 2.

We first record our constants once more :—

Surface watts, $W_s = 0·033$ watts per sq. cm. of surface.
Current density, $i = 1·46$ amperes per sq. mm.
Heating constant, $C_h = 1/1500$ watts per sq. cm. per degree.
Rise constant (with cotton), $C_v = 4·5$.
" " (without cotton), 3·5.

Final temperature, $t_f = \frac{W_s}{C_h} = 1500 \times 0·033 = 50$ deg. C.

Initial temperature increase from Table I. for a current density of 1·46 amps. per sq. mm. $= 0·71$ deg. per minute without cotton, or $0·71 \times \frac{3·5}{4·5} = 0·55$ deg. C. per minute with cotton. The temperatures at

intervals of 20 minutes, as determined by these data, are given in Table IV.

TABLE IV.*

Time.		Temperature in the beginning of the 20 minutes interval t_1.	Uncorrected Temperature increase $E_1 = \left(1 - \frac{t_1}{50}\right) \times .55 \times 20 = 11 - .22 \times t_1$.	Average Temperature during the interval $t_m = t_1 + \frac{E_1}{2}$.	Corrected Temperature increase $E = 11 - .22 \times t_m$.
Hrs.	Min.				
0	0	0	11	5·5	9·8
0	20	9·8	8·8	14·2	7·9
0	40	17·7	7·1	21·2	6·3
1	0	24	5·7	26·8	5·1
1	20	29·1	4·6	31·4	4·1
1	40	33·2	3·7	35	3·3
2	0	36·5	3	38	2·6
2	20	39·1	2·4	40·3	2·1
2	40	41·2	1·9	42·2	1·7
3	0	42·9	1·5	43·6	1·4
3	20	44·3	1·2	44·9	1·1
3	40	45·4	1	45·9	0·9
4	0	46·3	0·8	46·7	0·7
4	20	47	0·6	47·3	0·6
4	40	47·6	0·5	47·8	0·5
5	0	48·1	0·4	48·3	0·4
5	20	48·5	0·35	48·7	0·3
5	40	48·8	0·3	48·95	0·25
6	0	49·05			

These results can be obtained even more easily in a simple graphical way (see Fig. 3).

Draw line O A to represent the initial rise, which in our case is 11 degrees per 20 minutes, and B C at a distance, O B, equal to the final temperature (50 deg. C in our case) parallel to the axis of the abscissæ. The lines cut one another at the point D. We next divide both the axis of the abscissæ and line D C (starting from point D) into intervals of 10 minutes, and draw verticals through every division point on this axis. The first of these verticals cuts O D at the point *e*. Join point *e* with point 10' on line D C. Line *e*–10' cuts the 20 vertical at *f*, then join *f* with 20' on line D C. Line *f*–20' cuts the 30 vertical at *g*. Join *g* with 30' on D C, and so on.

In this manner we get a number of lines which "envelop" the temperature curve. To construct this curve we have chosen intervals of 10 minutes only, whilst with the analytical working 20-minute intervals were used. The greater exactness is necessary because we now neglect the "correction" taking $E_1 = E$, and thus regard the cooling action as being dependent on the initial temperature of the interval. A comparison with the calculated values shows that the graphic method gives results which are sufficiently accurate for practical purposes.

* See Curve Fig. 2.

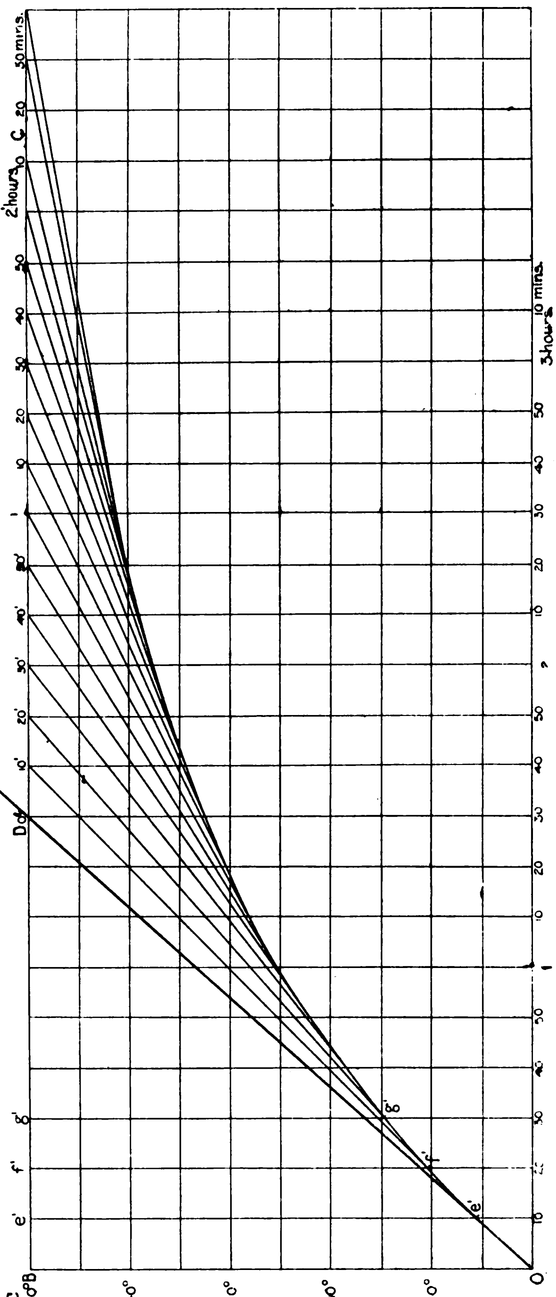


FIG. 3.

The two lines O D and D C determine the whole temperature curve. Therefore the form of all curves is similar, and having once plotted one it is applicable to all other cases. The only alterations necessary are the scales of temperature and time.

An enlarged temperature curve is given in Fig. 4, which can be used on any given scale by simply choosing the scale of temperatures so that O B is equal to the final temperature, and the scale of time so that $\frac{OB}{BD}$ is equal to the initial temperature increase. B D is the time in which the body would have reached the final temperature if no cooling had taken place during the period. In our example the initial rise per minute is $V = 0{\cdot}55$ deg. C., and the final temperature is $tf = 50$ deg. C. Therefore the "ideal heating time" B D is $\frac{50}{0{\cdot}55} = 90$ minutes $=$ $1\frac{1}{2}$ hours. To the ideal heating time we will give the symbol T_i.

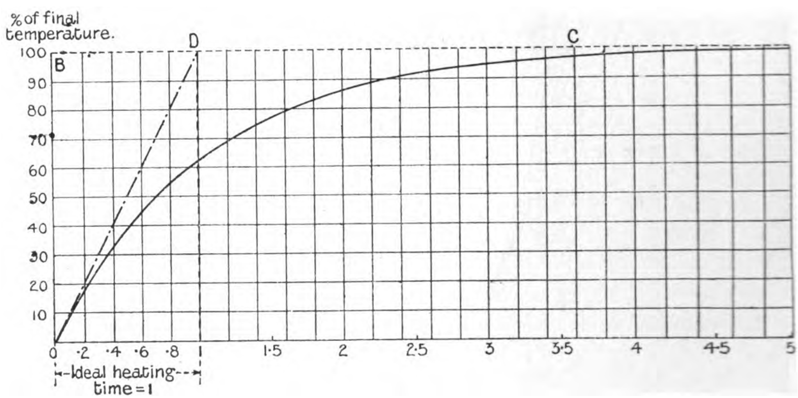


FIG. 4.

$$T_i = \frac{tf}{V} = \frac{W_s}{C_h} \cdot \frac{C_v}{W_v} = \frac{\frac{\text{Watts}}{\text{Surface}}}{C_h} \times \frac{C_v}{\frac{\text{Watts}}{\text{Volume}}}$$

$$T_i = \frac{\text{Volume}}{\text{Surface}} \times \frac{\text{Rise Constant}}{\text{Heating Constant}}$$

T_i is dependent only on the constant of the apparatus and on the ventilation, and is independent of the watts lost. This will be the basis for all our further calculations; for instance, in order to determine the very important practical question, after what length of time will a given machine approach its final temperature within a few per cent., we have only to examine the curve in Fig. 4, which shows that the final temperature is attained within 5 per cent. at the end of three times T_i, the ideal heating time. For a commercial temperature test this time ought to be sufficient, as the temperature would be raised only $1\frac{1}{2}$ deg. more after another hour, assuming the final temperature to be 50 deg. C.

A table giving the constants of this curve is appended :—

TABLE V.

Time of Heating. Unit: Ideal Heating Time.	Percent. of final Temperature attained.	Time of Heating. Unit: Ideal Heating Time.	Percent. of final Temperature attained.
0	0	1·25	71·5
0·1	9·5	1·5	77·8
0·2	18·1	1·75	82·8
0·3	26	2	86·6
0·4	33	2·5	92
0·5	39·5	3	95·5
0·6	45·2	3·5	97·3
0·7	50·5	4	98·4
0·8	55·1	4·5	99
0·9	59·5	5	99·5
1	63·3		

On examining Fig. 3, we notice that the projection of the different tangents O D $e - 10^1$, $f - 20^1$, $g - 30^1$, etc., viz.: B D, $e^1 - 10^1$, $f^1 - 20^1$, $g^1 - 30^1$, etc., are all equal to the ideal heating time T_i. This fact enables us to construct the temperature curve starting at any point.

Assuming the apparatus to be at an initial temperature t_1 before starting the actual temperature test, and if the final temperature tf and the ideal heating time T_i are known, we can construct the temperature curve on the basis of the assumption that the air temperature is t_1 degrees higher.

Make (Fig. 5) E F $= t_1$, O G $= tf$, K H $= T_i$, draw F H, and continue the construction as explained with reference to Fig. 3. In order to find

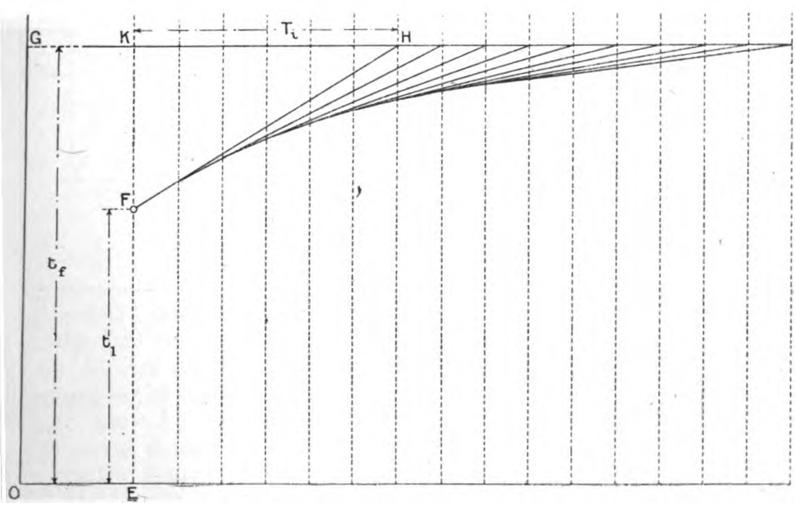


FIG. 5.

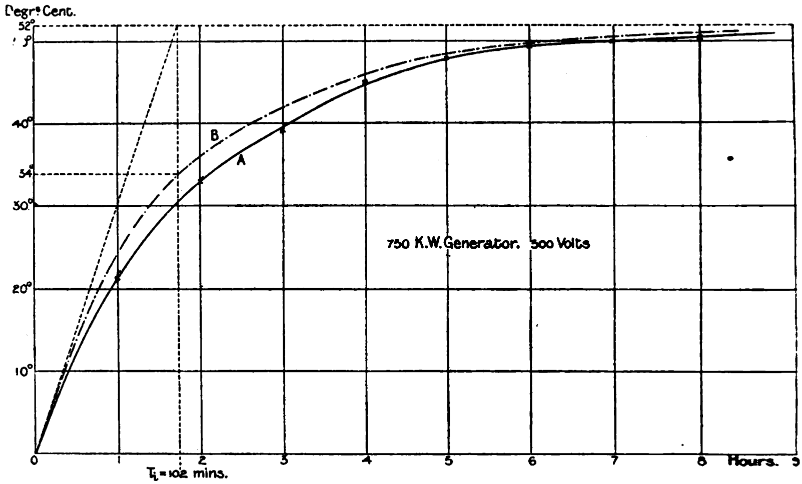

Degr? Cent.
52°
40°
34°
30°
20°
10°
B
A
750 K.W. Generator. 500 Volts
0
1
2
3
4
5
6
7
8
Hours.
9
T_L = 102 mins.

FIG. 6.

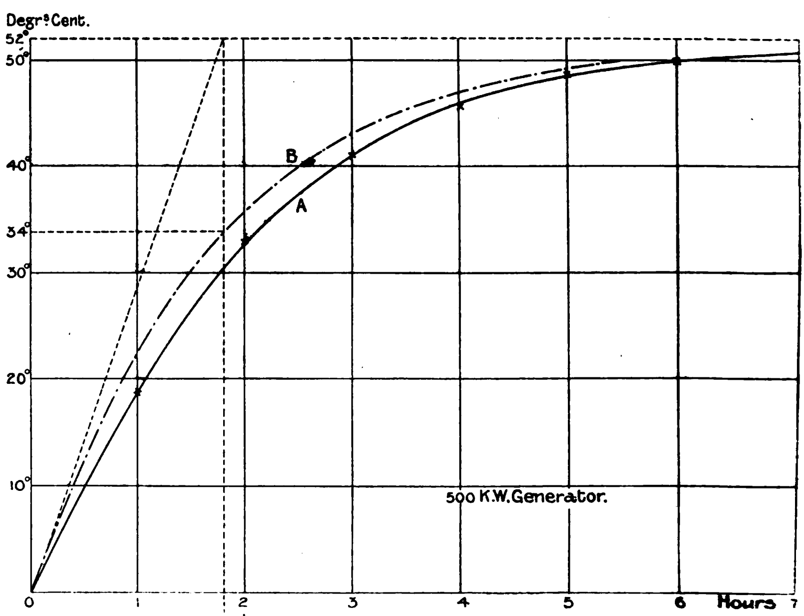

Degr? Cent.
52°
50°
40°
34°
30°
20°
10°
B
A
500 K.W. Generator.
0
1
2
3
4
5
6
Hours
7
108 mins.

FIG. 7.

the time when the final temperature will be reached within a certain percentage, Table V. can be used.

To find the time in which the field coil considered above will reach $47\frac{1}{2}$ deg., starting with a temperature of say 30 deg. C., we need only consider the air temperature to be 20 deg. higher, or the field temperature to be 20 deg. less, that is, $50 - 20 = 30$ deg. C. = total range of rise $= tf^1$.

The temperature under consideration, viz., $47\frac{1}{2}$ deg., becomes $47\frac{1}{2} - 20 = 27\frac{1}{2}$ deg. The latter as a percentage of the new $tf^1 = \frac{27\frac{1}{2}}{30}$

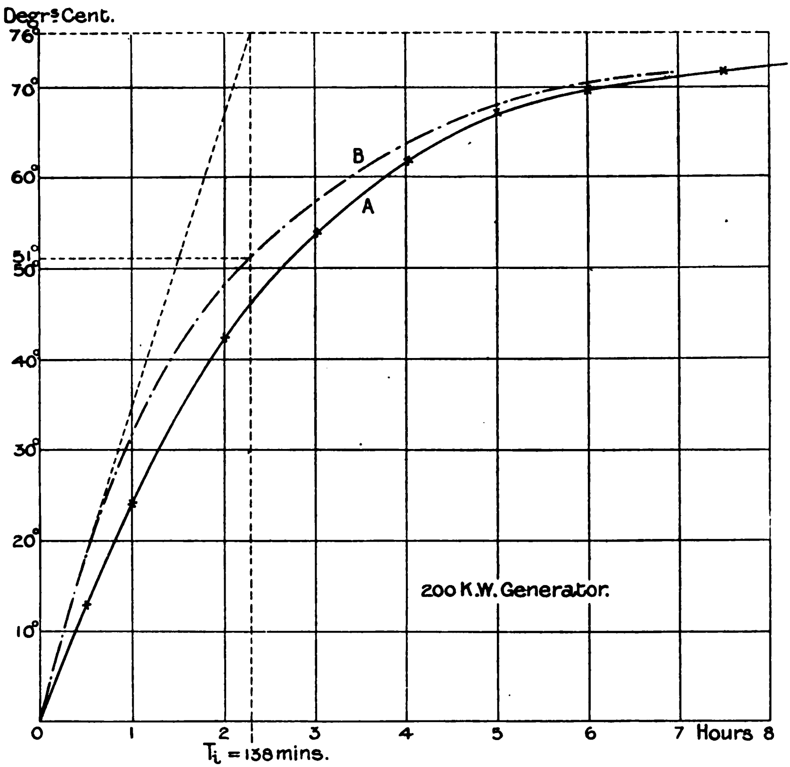


FIG. 8.

$= 0{\cdot}92$. From curve 3 or Table V. we observe at once that $0{\cdot}92$ of the final end temperature is reached in $2\frac{1}{2} \times T_i = 2\frac{1}{2} \times 1\frac{1}{2} = 3\frac{3}{4}$ hours.

A very useful rule for determining the length of time of the temperature test of a machine can be deduced as follows: It is only required to know (1) what the final temperature is likely to be and (2) the current density. The initial temperature rise is proportional to the square of the current density. In our example we had $1{\cdot}7$ amps. per m/m.^{2} (1,080 amps. per square inch) and an initial rise of $0{\cdot}55$ deg. C. per

minute. For 50 deg. C. the ideal heating time, T_i, was $1\frac{1}{2}$ hours. From this we can calculate tables of the ideal heating time (T_i), say for final temperatures of 50 deg. C. and 75 deg. C. (measured by resistance rise).

TABLE No. VI.

Current Density.		Ideal heating time in minutes for a final temperature of :	
Amps. per square millimetre.	Amps. per square inch.	50° C. (90° F.).	75° C. (135° F.).
·25	160	4,200	6,300
·5	320	1,040	1,550
·75	480	460	690
1	640	260	370
1·5	960	115	173
1·56	1,000	107	160
2	1,280	65	97
3	1,920	29	43
5	3,200	$10\frac{1}{2}$	16

The application of Table VI. may be shown by some examples :—

Example 1.—A continuous-current machine is designed for 50 deg. C. final temperature rise. The current density in the field coils is 1,000 amps. per square inch. How long is it to be tested in order to produce a rise of temperature equal to 95 per cent. of the final temperature ? Due to its considerably smaller heating constant, the armature has, as a rule, reached the end of its ideal heating time, T_i (see formula for T_i), and in consequence, its final temperature, more quickly than the field coils have. Therefore the field coils determine the time of test for a continuous-current dynamo, and if their temperature is practically constant, one can be sure that of all other parts is also constant.

From Table VI. we take $T_i = 68$ minutes for 1,250 amperes per square inch, and from Table V. the time of test necessary to bring the parts within 5 per cent. or $2\frac{1}{2}$ deg. C. of their final temperature is $3 \times T_i = 3 \times 68 = 3\frac{1}{2}$ hours. If we were to test this machine for only a little over 2 hours $= 2 \times T_i$, only 86·6 per cent. of its final temperature would be reached. In further illustration of this example, which is of great practical importance, some temperature curves of "Crompton" continuous-current machines are added (Figs. 6–13).

Fig. 8, for instance, refers to a 100 k.w., open type, 440 volt dynamo with a current density of 1·25 amperes per mm.^{2} (830 amperes per square inch) in the field when hot. The curve A is the curve actually observed, the curve B for constant final loss being developed from A for the reason that during the test the exciting current was kept constant, and in consequence the loss in the field increased with the heating up of the copper. We see that the final temperature rise is 58 deg. C. (measured by resistance increase).

Table V. shows that in the "ideal heating time" about $\frac{2}{3}$ of the final

temperature is reached. In our case $\frac{2}{3} \times 58 = 37$ deg. C. Then from curve B it will be seen that this temperature is reached in 2 hours 12 minutes = 132 minutes, thus:—

$$T_i = 132 \text{ minutes,}$$

the initial rise is $\dfrac{58 \text{ deg. C.}}{132} = 0{\cdot}44$ deg. C. per minute.

A current density of 1·25 amperes per mm.² corresponds to an initial rise of 0·50 deg. C. per minute (Table I.), or making allowance for

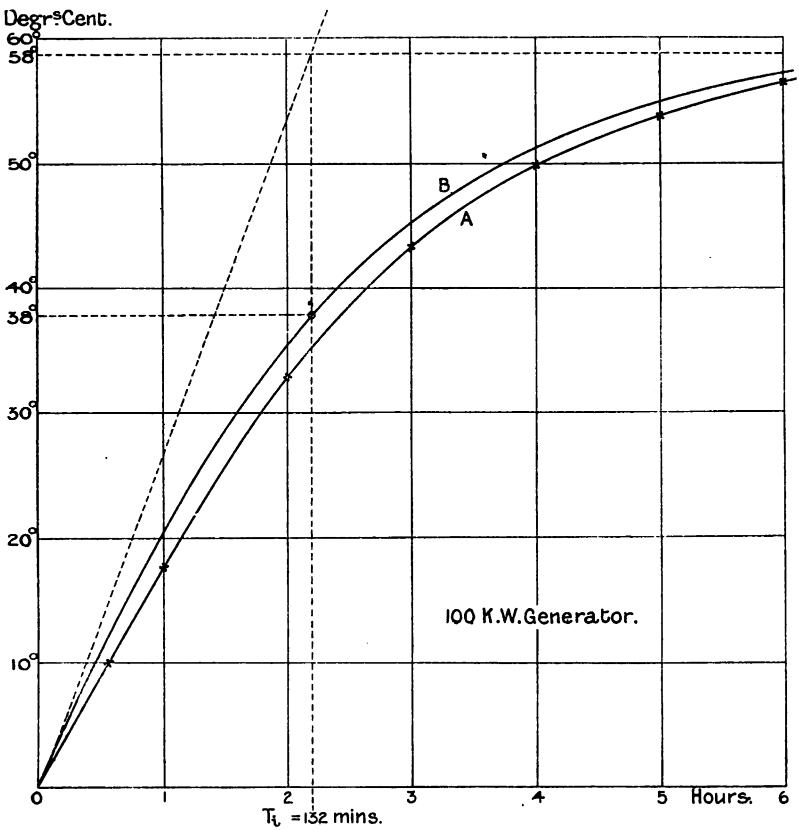


FIG. 9.

the heating of the copper, 0·60 deg. C. per minute. Therefore the specific heat of the field coil owing to the influence of the insulation is $\dfrac{60}{44} = 1{\cdot}36$ times that of pure copper. The size of the wire was 0·088 inch.

Temperature curve, Fig. 13, refers to a 440 volts, 4 H.P., semi-enclosed motor. Curve A is actually observed. Curve B gives the temperature curve reduced to constant loss. We gather from the

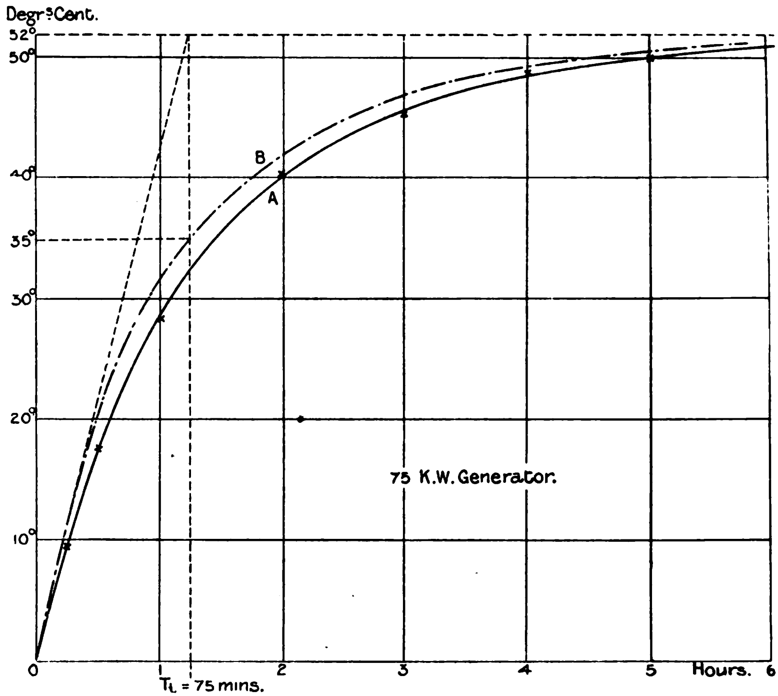

Degr.s Cent.
52°
50°
40°
35°
30°
20°
10°
B
A
75 K.W. Generator.
0
1
2
3
4
5
Hours. 6
T_L = 75 mins.

FIG. 10.

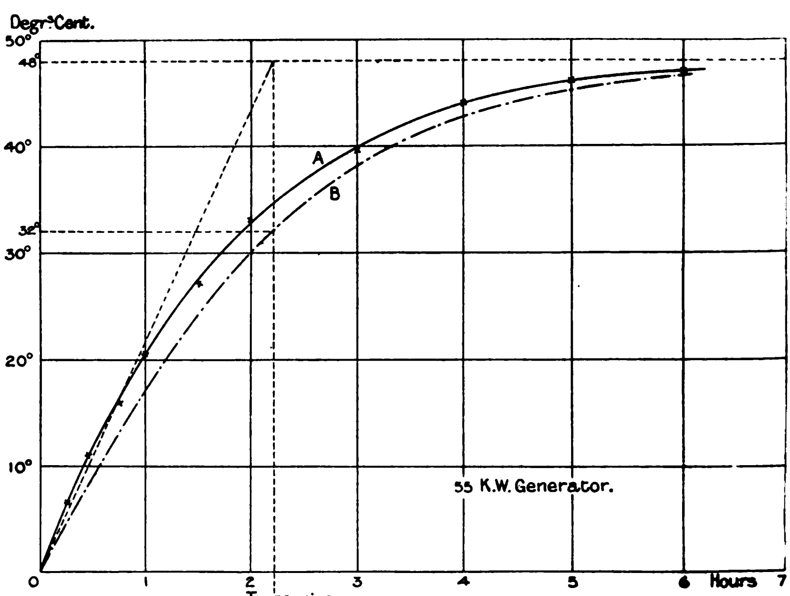

Degr.s Cent.
50°
48°
40°
32°
30°
20°
10°
A
B
55 K.W. Generator.
0
1
2
3
4
5
6
Hours 7
T_L = 132 mins.

FIG. 11.

curve that the final temperature, t_f, $= 64$ deg. C. and that $\frac{2}{3}$ of this final temperature, or 42 deg. C., is reached in 72 minutes. The ideal heating time, T_i, is therefore 72 minutes. Initial temperature rise $\frac{64}{72} = 0{\cdot}89$ deg. C. per minute.

The current density when the copper was hot was 1·8 amperes per mm.², which according to Table I. corresponds to 1·05 deg. C. per minute for cold copper or 1·26 deg. C. per minute for hot copper. Through the influence of the cotton insulation the specific heat of the field coil has been increased $\frac{1{\cdot}26}{0{\cdot}89} = 1{\cdot}42$ times. The size of wire was 0·028 inch. With very small high-volt motors it was observed that the specific heat of the thickly insulated thin wire of the field coils (say 0·012 size) was nearly double that of pure copper.

The following table refers to the curves given in Figs. 6–14 :—

Curve Fig.	Output of Machine.		Volts.	Type.	Size of wire in inches.	Ideal heating time in minutes.	Current density. Amps. per mm.²	Increase of the specific heat through the cotton insulation.
	H.P.	K.W.						
6	—	750	500	Open	$\frac{1}{8}\square''$	105	1·28	1·22
7	—	500	500	"	·109	108	1·25	1·25
8	—	200	440	"	·102	138	1·25	1·25
9	—	100	440	"	·088	132	1·25	1·36
10	—	75	500	"	·048	75	1·58	1·42
11	—	55	85	"	·088	132	1·05	1·22
12	35	—	110	Totally enclosed	·065	260	1·00	1·35
13	4	—	440	Semi-enclosed	·028	72	1·8	1·42
14	1½	—	110	Totally enclosed	·028	85	1·65	1·60

It is well known that large machines require a very much longer test than small ones. This is very clearly shown by the formula for T_i. Whilst heating and rise-constants do not vary very much with the size, the ratio $\frac{\text{volume}}{\text{surface}}$ increases with an increase of the size.

Fig. 15, *a*, *b*, *c*, shows sections of field coils of 2 k.w., 50 k.w., and 300 k.w. Crompton dynamos. In the case of the 2 k.w. machine, the section of the coil is 25 cm.², and the circumference of the section 19·5 cm. For the purpose of comparison $\frac{\text{section}}{\text{circumference}}$ may stand for $\frac{\text{volume}}{\text{surface}}$.

As an average it may be assumed that 50 per cent. of the total section of the field coils is copper ; thus we find—

For 2 k.w. machine $\frac{\text{section}}{\text{circumference}} = 1{\cdot}3$; $\frac{\text{volume}}{\text{surface}} = 0{\cdot}65$;
for 50 k.w. " " $= 2{\cdot}0$; " $= 1{\cdot}0$;
for 300 k.w. " " $= 2{\cdot}7$; " $= 1{\cdot}35$.

If for the 2 k.w. machine a four hours' temperature test is considered sufficient, the 50 k.w. machine must be tested 6¼ hours, and the 300 k.w. machine must be tested 8¼ hours.

For practical use, the current density will be more convenient for determining the test time than the dimensions. We may therefore conclude our investigations on this point by stating that :—

Dynamos with stationary field coils, which are designed for about 50 deg. C. measured by resistance rise, reach their final temperature within two or three degrees.

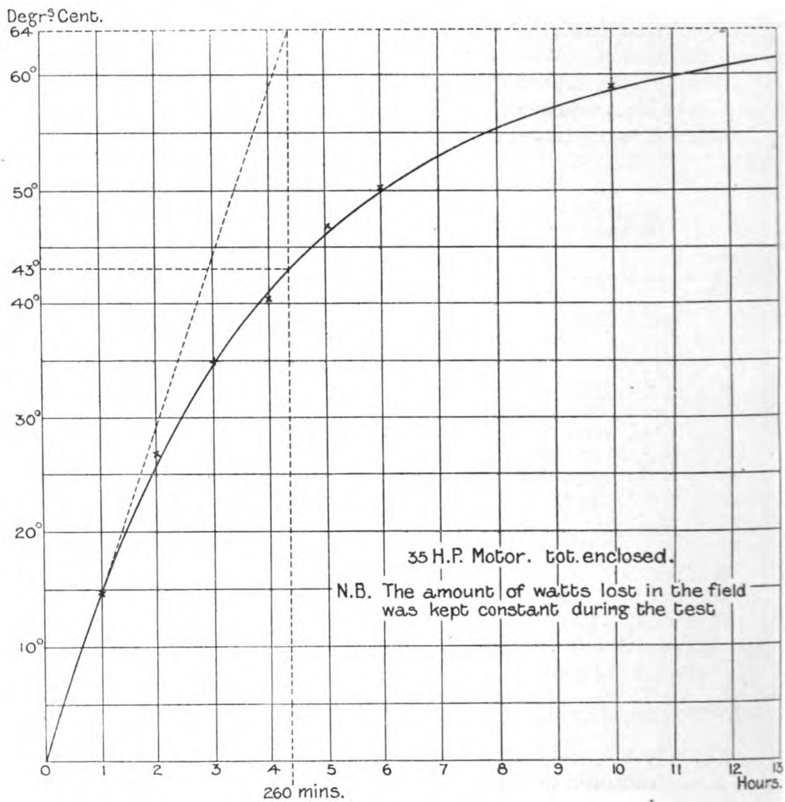


FIG. 12.

At the end of	1 hr.	with field	current density	of	3,000	amps.	per sq. in.
,,	3	,,	,,	,,	1,500	,,	,,
,,	6	,,	,,	,,	1,000	,,	,,
,,	9	,,	,,	,,	800	,,	,,

Example 2.—A motor has a temperature rise of 50 deg. F., with a current density in the field of 1·7 amperes per mm.² What current density should be used if we apply the same machine as a crane motor with 75 deg. F. after one hour?

From our original example we know that the ideal heating time is $T_i = 1\frac{1}{2}$ hours. One hour is, therefore, equal to $0{\cdot}67 \times T_i$. At $0{\cdot}67 \times T_i$ (see Table V.) 50 per cent. of the final temperature (tf) is reached; therefore, for the crane motor $tf = 2 \times 75 = 150$ deg. F. The watts lost can be increased in the ratio $150/50 = 3$ times, and thus the current density may be increased $\sqrt{3} = 1{\cdot}73$ times. As a crane motor with 75 deg. F. temperature rise after one hour (measured by

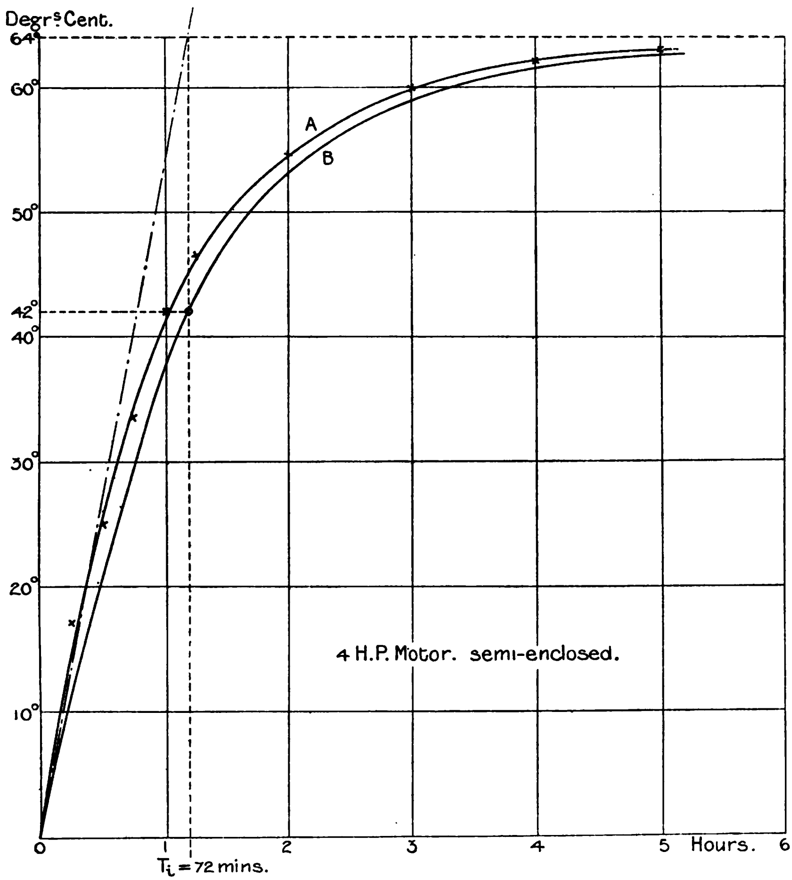


FIG. 13.

resistance), the current density must be $1{\cdot}73 \times 1{\cdot}7 = 2{\cdot}9$ amperes per mm.², that is, 1,850 amperes per square inch.

A test of a 10-H.P. Crompton motor is also given. Curve A (Fig. 16) is the field temperature rise for continuous running (current density $1{\cdot}7$ amperes per mm.²), while Curve B is a one-hour test with 42 per cent. higher current density, that is, $2{\cdot}4$ amperes per mm.² We see that the final temperatures of the curves A and B are the same (46 or 48 deg. C.) As regards the temperature of the armature, it attained

42 deg. C. during the continuous run. The flux went up about 20 per cent. during the one-hour test, in consequence of 42 per cent. higher excitation, and the armature current was kept 42 per cent. higher than for the continuous run. The temperature rise after one hour was 38 deg. C. This temperature is lower than the continuous running temperature, due to the core loss not having increased in the same ratio as the copper loss.

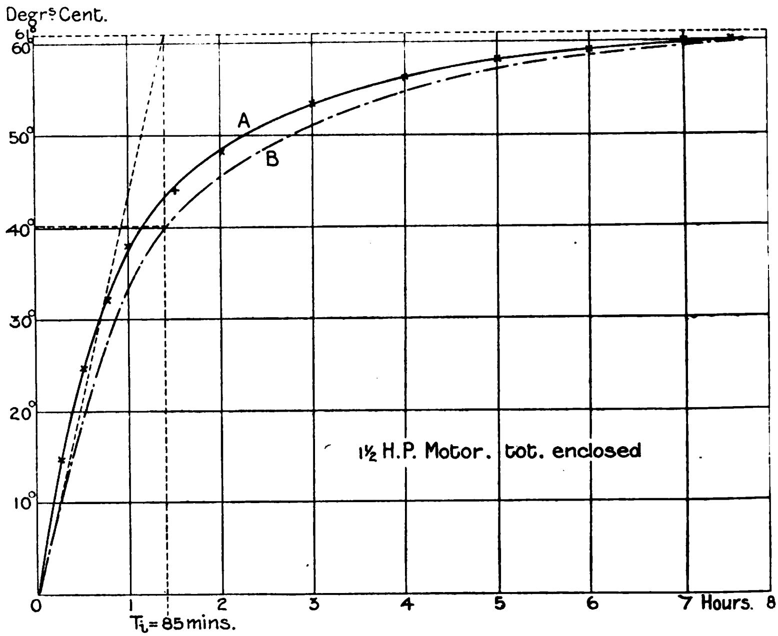


FIG. 14.

III.—HEATING AND COOLING.

If a heated body has attained the temperature which we have called the "final temperature," all heat subsequently produced is given out to the surrounding atmosphere.

Referring to our previous example again, we stated that if the body has a temperature of 50 deg. C. above that of the atmosphere, it gives out by radiation and convection as much heat as would increase the temperature of the body at the rate of 0·55 deg. C. per minute. With a temperature of only 35 deg. C. we found the state to be inconstant, the temperature increasing at the rate of—

$$\frac{0\cdot055}{50}(50-35)=\frac{1}{90}\times0\cdot15=0\cdot165 \text{ deg. C. per min.}$$

Now we know that the ideal heating time—

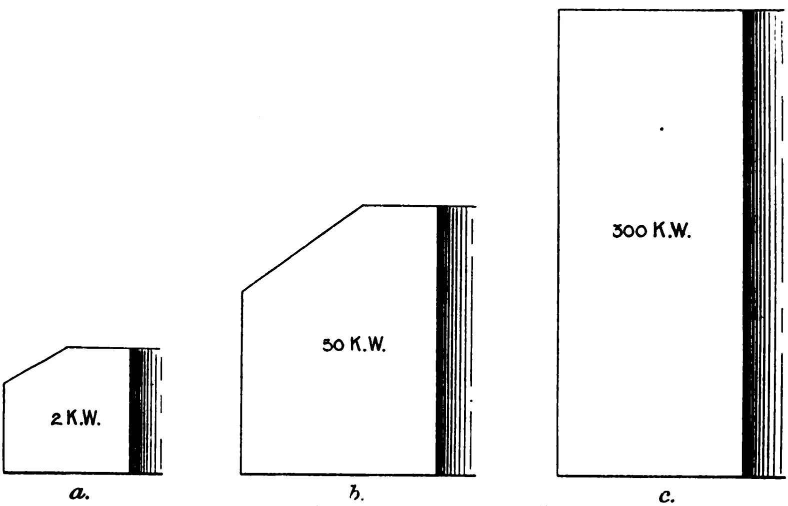
$$T_i\,\frac{50}{0\cdot55}=\frac{\text{Final temp.}}{\text{Initial rise}}=90 \text{ min.}$$

and is a constant of the apparatus, so that we may say :—

$$\text{Temp. increase per min.} = \frac{\text{Final temp.} - \text{Momentary temp.}}{\text{Ideal heating time}}$$

$$E = \frac{tf - t}{T_t} = \left(\frac{50 - 35}{90}\right).$$

We will suppose that the temperature was more than the temperature of equilibrium, 50 deg. C., say 70 deg. C. We may assume that this temperature was obtained by overloading the apparatus at some time. Then the cooling action is $70/50 = 1{\cdot}4$ times greater than that which just balances the temperature increase of 0·55 deg. C. per minute. Thus at the instant when the temperature of the body cools down at the rate of $E = \frac{50 - 70}{90} = 0{\cdot}22$ deg. C. per min. we see that



Section of field coils. About $\frac{1}{3}$ full size.
FIG. 15.

there the calculation of the cooling is the same as that of the heating, and if E is positive it indicates temperature increase, while if negative it indicates a cooling action.

It is important to consider the cooling curve in cases where the development of heat suddenly becomes less. Supposing the lost watts were suddenly reduced by half (Fig. 17), then the new final temperature would be $\frac{1}{2} \times 50 = 25$ deg. C., and the initial rise $\frac{1}{2} \times 0{\cdot}55 = 0{\cdot}275$ deg. per min. If the body had already attained its end temperature of 50 deg. C. during the first instant after the change, the temperature would drop at the rate of $\frac{25 - 50}{90} = -0{\cdot}275$ deg. per min. At any other temperature the calculation of the temperature drop is equally easy; at 35 deg. C., for instance, we find $E = \frac{25 - 35}{90} = -0{\cdot}11$ deg. per min.

Proceeding by intervals of 10 or 20 minutes, we can trace the whole cooling curve, which tends to reach the new final temperature of 25 deg. C.

By a complete heating and cooling curve we understand the heating curve rising up to the final temperature, and the drop curve from this temperature down to $0 =$ air temperature. The rate of the drop in the first moment is $\frac{50 - 0}{90} = 0{\cdot}55$ deg. C. per minute, being the same as the initial rise. Therefore, we see that the initial rise and the initial drop are identical. The "total rise" was 0 to 50 deg. C.; the "total drop" is naturally the same amount, *i.e.*, 50 deg. C. to O.

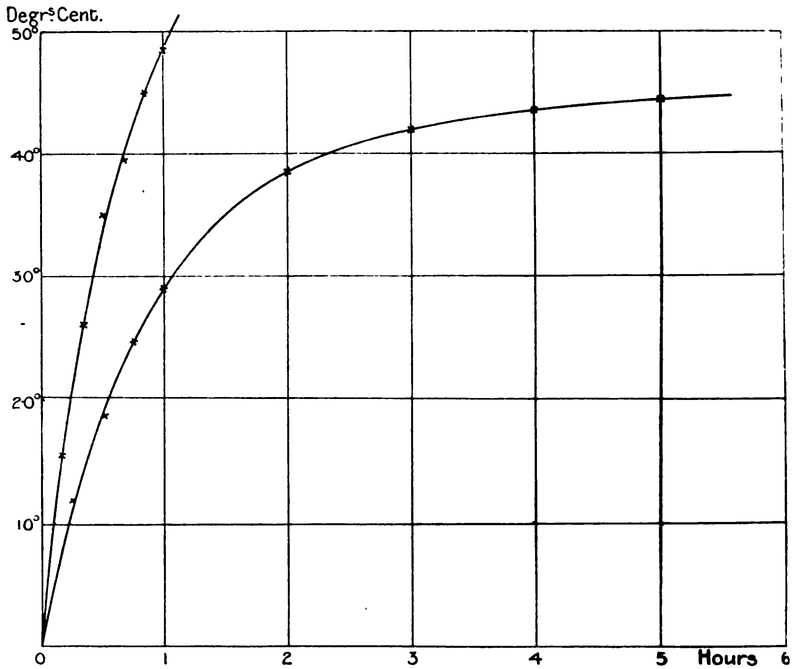


FIG. 16.

This is sufficient to show us that the falling curve and the rising curve are identical, in form, only reversed. The consequence is that we are able to apply the same graphic method as that described in connection with Fig. 3, and that after finding the correct scale in the manner previously described, we can use Fig. 4 upside down as a standard cooling curve for every possible case.

The tables can also be easily adapted to the cooling curve. Table V. shows for instance that the machine is heated up from 0 to 75 per cent. of the final temperature (in our example $0{\cdot}75 \times 50 = 37\frac{1}{2}$ deg. C.) in $1\frac{1}{2} \times$ the ideal heating time T_i ($1\frac{1}{2} \times 90 = 135$ minutes). Bearing in mind that the cooling curve is a heating curve (only reversed), we may

say that in $1\frac{1}{2}$ times T_i the body is cooled down from 50 deg. C. to 100 per cent. — 75 per cent. = 25 per cent. of the final temperature, that is, in the example, to $0{\cdot}25 \times 50 = 12\frac{1}{2}$ deg. C. If the load is only reduced in such a degree that the losses are halved, the new final temperature to which the body would tend to cool down is $\frac{1}{2} \times 50 = 25$ deg. C.

With reference to the above, we found that the initial rise in this case is 0·275 deg. C. per min. The total amount to be cooled down is $50 - 25 = 25$ deg. C., thus the "ideal cooling time" is $T_i = \frac{25}{0{\cdot}275} = 90$ minutes, just as for the heating time, and consequently in, say, $1\frac{1}{2} \times T_i = 135$ minutes, the body has cooled down $0{\cdot}75 \times 25 = 19$ deg., and so reached a temperature of $50 - 19 = 31$ deg. C.

Before concluding our investigations on cooling, it may be pointed

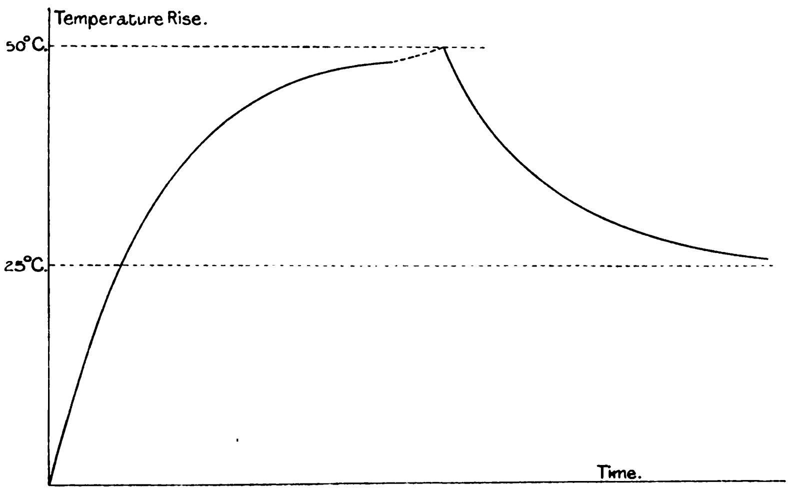


FIG. 17.

out that with rotating machines during the cooling period, when the machine is shut down, the cooling conditions generally are worse than those existing when the machine is working. This is due to there being no ventilation when the machine is at rest. When the machine stands still the heating constant (equivalent to cooling constant of the field coils is about $\frac{2}{3}$ of that with the machine revolving at normal speed. This ratio naturally depends on the sort of machine, and on the influence of the armature losses on the field temperature, etc. It would, however, be beyond the scope of our real subject to consider this question fully in the present paper. We will, therefore, only study in principle how a change in the degree of ventilation influences the temperature curve.

Below an example is given to show the application of the rules just formulated. We intentionally adhere to the simple case of a field coil, only just referring to the subject of machine parts in which we have to

deal with the separate heating of copper and iron, and in which, especially before the final temperature is reached, the copper may be quite hot, whilst the iron is still cool, a subject which it is intended to deal with more fully in a separate paper. In principle there is no difference whatever in calculating the temperatures of field coils or armatures, induction motor stators and rotors, or transformers.

Example.—A continuous-current generator has a current density in the field of 1·35 amperes per mm.², corresponding to an initial increase of 0·55 deg. per min. The field coil is dimensioned so that at normal watts the temperature rise is $tf = 45$ deg. C. $= 81$ deg. F., total ideal heating time $T_i = \frac{45}{0{\cdot}55} = 82$ minutes $= 1{\cdot}36$ hours.

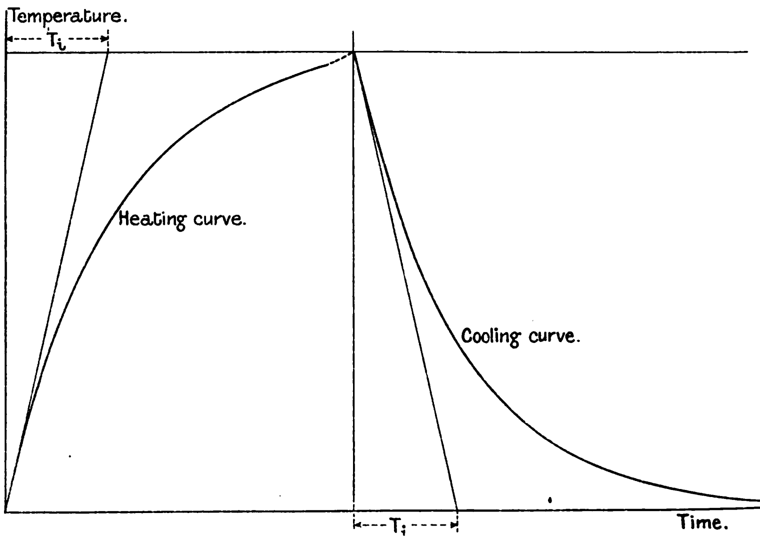


FIG. 18.

The machine is to be submitted to the following tests:—

(1) 4 hours normal watts.
(2) $1\frac{1}{2}$ hours 56 per cent. higher watts (25 per cent. overload in current).
(3) $\frac{1}{2}$ hour 125 per cent. higher watts (50 per cent. overload in current).
(4) 2 hours normal watts.
(5) 4 hours shut down.
(6) 2 hours $\frac{1}{2} \times$ normal watts.

What is the temperature rise $t_1 \ldots t_6$ at the end of the different periods?

(1) $T_i = 4$ hours normal watts.

$tf_i = 45$ deg. C.

$$T_1 = 4 \text{ hours} = \frac{4}{1{\cdot}36} = 3 \times T_i.$$

From Table V. we find at end of period (1) the final temperature $t_1 = 0{\cdot}95 \times tf_i = 0{\cdot}95 \times 45 = 43$ deg. C.

(2) $t_2 = 1\frac{1}{2}$ hours 56 per cent. higher watts.
New $tf_2 = 1{\cdot}56 \times 45 = 70$ deg. C.
Initial temperature $t_1 = 43$ deg. C.

Total range of rise $70 - 43 = 27$ deg. C. $t_2 = 1\frac{1}{2}$ hours $= \frac{1{\cdot}5}{1{\cdot}36} = 1{\cdot}1 \times T_i$.

From Table V. we find that at $1{\cdot}1 \times T_i$, $0{\cdot}65 \times 27 = 17$ deg. C. is reached. Temperature at the end of the second period is $t_2 = 43 + 17 = 60$ deg. C.

(3) $t_3 = 0{\cdot}5$ hours at $2{\cdot}25 \times$ normal watts.
New $tf_3 = 2{\cdot}25 \times 45 = 102$ deg. C.
Initial temperature $t_2 = 60$ deg. C.

Total range of rise $102 - 60 = 42$ deg. C. $\frac{1}{2}$ hour $= \frac{0{\cdot}5}{1{\cdot}36} = 0{\cdot}37 \times T_i$.

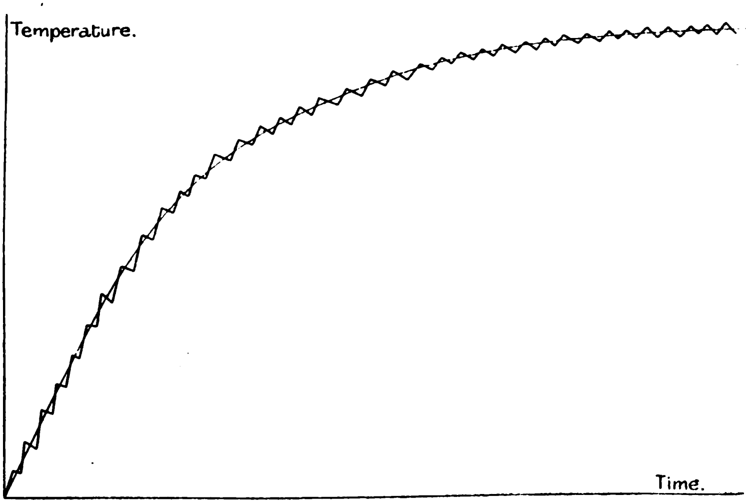


FIG. 19.

From Table V. we see that at $0{\cdot}37$ T_i $0{\cdot}28 \times 42 = 12$ deg. C. is reached. The temperature at the end of the third period is $t_3 = 60 + 12 = 72$ deg. C.

(4) $t_4 = 2$ hours at normal load.
New $tf_4 = tf_1 = 45$ deg. C.
Initial temperature $t_3 = 73$ deg. C.

Total range of drop $73 - 45 = 28$ deg. C. $t_4 = 2$ hours $= \frac{2}{1{\cdot}36} = 1{\cdot}47 \times T_i$.

By Table V., in $1{\cdot}47$ times $T_i - 0{\cdot}73 \times 28 = 20$ deg. C. of cooling down. Temperature at the end of the fourth period, $t_4 = 73 - 20 = 53$ deg. C.

(5) $t_5 = 4$ hours shut down.
New $tf_5 = 0$.

Starting temperature $t_4 = 53$ deg. C.

Total range of drop $53 - 0 = 53$ deg. C.

The cooling, when the machine is shut down, is in the ratio of 2 to 3 worse than with the machine revolving. Thus our ideal heating or cooling time T_i goes up in this ratio, say to T_i^{I}. $T_i^{I} = \frac{3}{2} \times 1{\cdot}36 = 2{\cdot}04$ hours. $t_5 = 4$ hours $= \frac{4}{2{\cdot}04} = 1{\cdot}96 \times T_i$.

By Table V., in $1{\cdot}96$ times $T_i^{I} = 0{\cdot}83 \times 53$ deg. C. $= 44$ deg. C. are cooled down, and the temperature at the end of the fifth period is $t_5 = 53 - 44 = 9$ deg. C.

(6) $t_6 = 2$ hours $\frac{1}{2}$ normal watts.

With this reduced excitation the motor has to run faster, and in consequence the ventilation is improved, say 20 per cent.

$$\text{New } t f_6 = \frac{0{\cdot}5 \times 45}{1{\cdot}2} = \frac{22{\cdot}5}{1{\cdot}2} = 19 \text{ deg. C.}$$

Initial temperature $t_5 = 9$ deg. C.

Total range of rise $19 - 9 = 10$ deg. C.

$$\text{New } T_i^{II} = \frac{1{\cdot}36}{1{\cdot}2} = 1{\cdot}13 \text{ hours.}$$

$$T_6 = 2 \text{ hours} = \frac{2}{1{\cdot}13} \times T_i^{II} = 1{\cdot}78 \times T_i^{II}.$$

By Table V. in $1{\cdot}78$ times T_i^{II}, $0{\cdot}8 \times 10 = 8$ deg. C. are reached The temperature at the end of the sixth period is $t_6 = 9 + 8 = 17$ deg. C.

IV.—Crane Rating.

These investigations are of great importance in the design of machines which are only on full load for a few hours, such as lighting transformers, and machines which require an overload for an hour or two. In such cases, the time during which the load is on can generally be determined with tolerable exactitude, and for dealing with this problem our next examples give full information.

This kind of rating is often called the "Short Time Rating," from which we have to distinguish machines which are designed for intermittent use for short periods of application and interruption (of a few minutes) briefly called "Crane Rating." This latter kind of machine is quite distinct from a machine designed for short periods of use and long interruption, which we dealt with in the first part of our study.

The characteristic qualities of crane rating are as follows—

(1) The load has a more or less regular periodic character. This distinguishes crane rating from the short-time rating.

(2) The time of use and the time of interruption is so short that during the former the temperature obtained does not come anywhere near the final temperature, and during the time of interruption the apparatus does not cool down to anywhere near the air temperature. In fact after some hours' run, the maximum and minimum temperature attained must remain within a few per cent. of the average temperature.

We will first assume that the cooling conditions when the machine

is shut down are the same as when the machine is running, and study the question of crane rating on our field coil, the data for which are—.

Current density 1·35 amps. per $mm.^2$
Final temperature for continuous run, 50 deg. C.

As we found from the current density, the initial rise is 0·55 deg. per minute, and the ideal heating time $T_i = \frac{50}{0{\cdot}55} = 90$ mins. $= 1\frac{1}{2}$ hrs. If we submit the coil to a crane load, consisting of periods of 3 minutes' heating and 7 minutes being shut down, the total period lasting 10 minutes, then in order to obtain the average temperature curve round which the actual temperature fluctuates, we have only to consider that a heating time of 3 minutes is followed by a cooling time of 7 minutes. Since during these short spaces of time the temperature increase or fall in the unit of time can be considered constant, the 7 minutes' cooling is as effective as a 3 minutes' cooling with $\frac{7}{3}$ times increased intensity.

To find the final average temperature we have only to multiply the final temperature for continuous run, 50 deg. C. by $\frac{3}{10}$, and so obtain $\frac{3}{10} \times 50 = 15$ deg. C.

Considering that 3 minutes is $3/90 = 3{\cdot}3$ per cent of the ideal heating time, and that in the short times of 3 and 7 minutes the time and temperature (taken as percentages) are practically proportionate to one another, the positive temperature fluctuation during 3 minutes (see Table V.) is $0{\cdot}033 \times$ total range of temperature $= 0{\cdot}033 \times (50° - 15°) = 0{\cdot}033 \times 35$ deg. C. $= 1{\cdot}15$ deg. C. The negative fluctuation during 7 minutes is $0{\cdot}077 \times$ total range of drop $= 0{\cdot}077 \times (15° - 0) = 1{\cdot}15$ deg. C. Consequently the final temperature is 15 deg. C. $\pm \frac{1{\cdot}15°\ C.}{2}$ The average initial rise is $\frac{3}{10} \times 0{\cdot}55 = 0{\cdot}165$ deg. C. per minute, so that the ideal heating time is $\frac{15}{0{\cdot}165} = 90$ minutes, the same as with continuous rating. Thus the crane motor requires the same time for heating up as the machine designed for continuous rating. The final temperature is directly proportional to the ratio—

$$\frac{\text{time of use}}{\text{time, full period}}$$

under the assumption that when the machine is shut down the ventilation is the same as when it is running.

As stated above the cooling is much worse while the machine is shut down, because we considered that a reduction of the ventilation of the field coil in the ratio of $\frac{2}{3}$ would occur.

Taking this into account we have—

3 minutes heating,
3 „ cooling with full intensity,
7 „ „ „ $\frac{2}{3}$ of full intensity,

which is equivalent to—

$$\frac{2}{3} \times 7 = 4{\cdot}7 \text{ cooling with full intensity.}$$

Total equivalent time of cooling with full intensity $4.7 + 3 = 7.7$ minutes.

Final temperature $\frac{3}{7.7} \times 50 = 0.39 \times 50 = 19.5$ deg. C.

The ventilation of the *armature* is, at full speed, 3 to 5 times better than when it is stationary, so here we have—

3 minutes heating,
3 „ cooling with full intensity,
7 „ „ with say $\frac{1}{4}$ of full intensity,

equivalent to $\frac{1}{4} \times 7 = 1.8$ minutes cooling with full intensity. Total equivalent time of cooling with full intensity $= 3 + 1.8 = 4.8$ minutes.

Final temperature $\frac{3}{4.8} \times 50 = 0.63 \times 50 = 31$ deg. C.

If an ordinary continuous rating motor is to be used as a crane motor, the losses in the badly ventilated parts (field coils) can be increased considerably more than in the well ventilated ones (armature). This only holds good if the machine is to be designed for actual crane-working conditions, the time of working and stopping being definitely given.

Usually a crane motor has to stand a one hour's test on full load, independently of what it has to do in actual practice. It is naturally very much simpler to carry out a test of this kind than a test in which the motor has to be alternately switched on and off. It is even an easier test than a continuous rating test. This is the chief reason why we adhere to the one hour's test, and long experience in actual practice has proved that in nearly every case, a motor which stands this test with a moderate temperature rise, is large enough for crane work. One disadvantage, however, is seriously felt, namely, that the time of test is the same for small as well as for large machines, and still more that the time is the same for open and totally enclosed ones.

Applying the formula for T_i, we find that for a 2-H.P. motor the ideal heating time of the field is about one hour, and of the armature six hours, therefore in one hour 63 per cent. and 80 per cent. respectively of the final temperature is reached (Table V.). In the case of a 50-H.P. motor, the ideal heating time of the field is say 12 hours, that of the armature 75 hours, and thus the field has in one hour attained 56 per cent. and the armature 75 per cent. of the final temperature.

Now under a certain condition of crane work, given by the load actor $= \frac{\text{time of working}}{\text{total period,}}$ the machine reaches a temperature which is quite in proportion to that which we have called the final temperature, and which would be obtained if the machine ran continuously with its crane B.H.P. Taking the ventilation, with the machine at rest, to be $\frac{2}{3}$ in the case of the field, and $\frac{1}{4}$ in the case of the armature, of that when the machine is at full speed, the one hour's test corresponds—

In the field of the 2-H.P. motor to a load factor of 60 per cent.
„ armature „ 2 „ „ „ 50 „
„ field „ 50 „ „ „ 47 „
„ armature „ 50 „ „ „ 42 „

In the case of totally enclosed machines, taking the ventilation when the machine is at rest, as 0·8, for the field, and for the armature ½ of that of the machine when at full speed, the one-hour's test would mean approximately—

In the	field	of the	2-H.P.	motor a	load factor	of	38 per cent.
,,	armature	,,	2	,,	,,	,,	38 ,,
,,	field	,,	50	,,	,,	,,	31 ,,
,,	armature	,,	50	,,	,,	,,	31 ,,

Thus we see that the one hour's test is more severe on small and open machines than on large and totally enclosed ones.

Considering a one hour's crane test to be correct (load factor, say, 50 per cent.) for small open-type machines, the time of test ought to be:—

For large open-type machines, say	... ...	1⅓ hours.
For small totally enclosed machines	... ...	1⅓ ,,
For large ,, ,, ,,	... ...	2 ,,

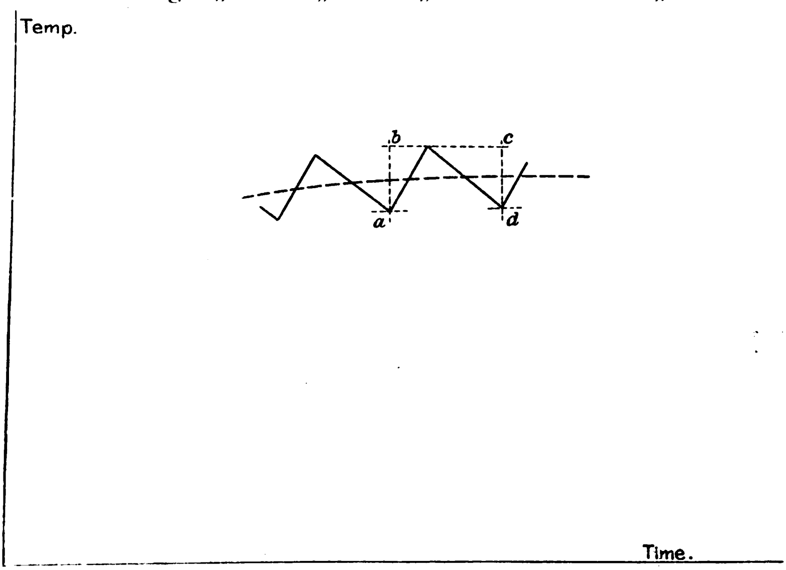


FIG. 20.

Naturally, a difference would also come in for different speeds, and different percentages of cotton in the field coils, but we cannot consider this question on the present occasion.

Our investigation as to the meaning of the one hour's crane test shows that its simplicity involves a very great inequality, having a different value for every single machine. It would be desirable, therefore, to find a test which would give accuracy as well as simplicity. A test with the exact conditions given, for instance, with a period of 3 minutes' running and 7 minutes' stopping is, however, somewhat troublesome.

Now, within rather wide limits, the temperature curve can be considered a straight line, and in consequence the temperature is only dependent on the load factor. Therefore, we could replace the 3

minutes' running and the 7 minutes' stopping by saying 9 minutes' running and 21 minutes' stopping respectively, which would require less attention than the shorter period. We could extend the length of the period a little further still for large machines, and shorten it a little for small machines. This "crane test with extended periods" certainly gives more exactness than the one hour's test, and enables us to make the expression "crane rating" more definite, and to allow for different load factors. The following is suggested as a simple method of crane-motor testing:—

The final temperature of a crane motor depends:—

(1) on the load (B.H.P.)

(2) on the load factor, $\dfrac{\text{time of working.}}{\text{total period.}}$

With an ordinary machine for continuous rating we have only to consider two figures, viz., the load and the temperature rise. In the

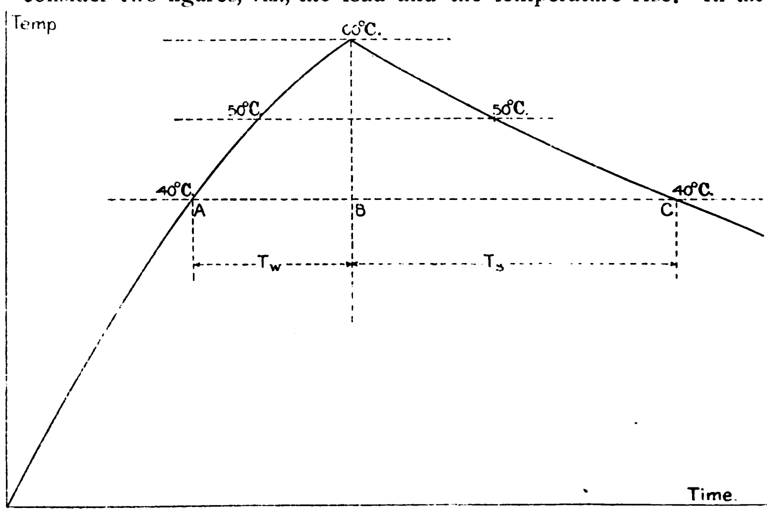


FIG. 21.

case of the crane machines a third is added, viz., the load factor. Having to deal with three figures, instead of asking, "What will be the final temperature at a given load and load factor?" we can reverse the question, and ask "For how many minutes out of 10 can the motor give a certain output if the temperature rise is not to exceed 50 deg. C.?"

The answer to the latter question can be very easily supplied by a test. When a crane motor has reached its final temperature, the temperature increase (during the working period *a b*, Fig. 20) is exactly equal to the temperature drop during the stopping period (*c d*). If, therefore, we take the temperature curve of the motor on full load, up to a little more than 50 deg. C., or to 60 deg. C., and carefully observe the time (T *w*) required for the rise from 40 deg. to 60 deg. C. (Fig. 21), then if we shut down the machine, and observe

the time required for cooling down from 60 deg. to 40 deg. C. (T *s*), the load factor is given by the ratio—

$$\frac{\text{Heating time}}{\text{Heating} + \text{cooling time}} = \frac{t\,w}{\mathrm{T}\,w + \mathrm{T}\,s} = \frac{\mathrm{A\,B}}{\mathrm{A\,C}} \text{ (Fig. 21).}$$

It is very easy to take this test for all stationary parts to which a thermometer can be left attached during the working period. For the armature, one could stop, say, half-hour after starting, take the temperature at that moment (point 1, Fig. 22), and easily judge in what time the 40 deg. rise will be approximately reached. At the end of the period stop again, take the actual temperature, which need not necessarily be exactly 40 deg. C. (point 2, Fig. 22), run on again for some time until 60 deg., or a little more, are reached (point 3, Fig. 22), and then finally stop to observe the cooling of the armature (points

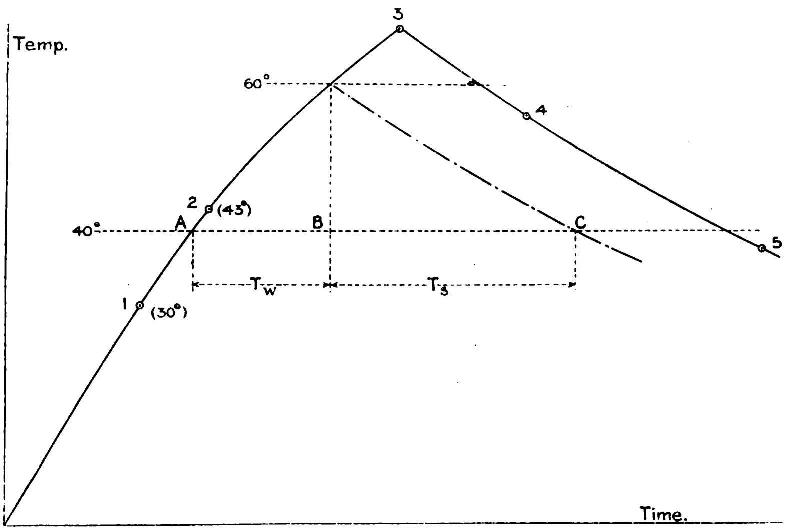


FIG. 22.

4 and 5). If point 3 does not coincide with 60 deg. C., we transfer the cooling curve horizontally until it passes the 60 deg. point (dotted curve, Fig. 22), and draw the horizontal line A C through the 40 deg. point, then $\frac{\mathrm{A\,B}}{\mathrm{A\,C}}$ is the load factor.

If the temperatures are taken with a thermometer, the latter should have a very small mercury bulb so that it records the temperature of the armature as quickly as possible.

It will be found that in these investigations I have preferred approximate methods to exact mathematical ones, the former enabling us to keep in closer touch with the real physical phenomena, and being more easily adaptable to any irregular case ; on the other hand, they are precise enough for practical purposes. For investigations on a more mathematical basis, I refer to the excellent article of E. Ölschläger, "On intermittent rating" (*Elektrotechnische Zeitschrift*, p. 1058, 1900).

Joint Discussion on Mr. Rayner's and Mr. Goldschmidt's Papers at Meeting of March 9, 1905.

Dr. Glazebrook.

Dr. R. T. Glazebrook, F.R.S.: I do not know that one who has had so much to do with the experiments as I have is the proper person to open this discussion, but I take it we have only a few minutes left for the meeting to-night, and possibly one or two words of explanation by me as to the objects of the experiments, and as to how far those objects have been realised, may be of use in helping the discussion which will take place at the next meeting. Mr. Rayner has, I think, explained those objects almost sufficiently. When the Motors and Generators Sub-Committee of the Electrical Plant Committee of the Engineering Standards Committee began its work, one problem which was before them was to define as clearly as they could the permissible temperature rise in machines of various patterns under working conditions. The first question which it seemed to some members of the Committee desirable to ask and if possible to answer was, what is the highest temperature that the materials which are ordinarily used in those machines can be allowed to reach, and what are the conditions that the materials will be under at those high temperatures? Do the resistance and the dielectric strength of the material increase up to a certain temperature, and if so up to what temperature? It is clear that the brittleness of the material increases as the temperature rises; but up to what limit of temperature may the materials be used without the brittleness becoming so excessive as to render them dangerous? It was not altogether easy to devise experiments to test these points. The questions of the dielectric resistance, and of the ordinary insulation resistance, were not difficult to settle, but the question of tests which would determine the changes of brittleness, the changes of mechanical strength, were more difficult to arrange. Mr. Rayner has explained, I think, clearly the methods that were used in making the various experiments, and the members have before them for their consideration in the various tables that are published in the paper the results of those experiments. Treating the matter quite generally, it would appear (as I take it, it would ordinarily be supposed to be the case), that the properties of the materials are all somewhat improved by slight heating, or to be more precise, by heating up to 75°. Perhaps the resistance to punching and the brittleness does not always improve up to that temperature; but at any rate I think there are few cases in which it is seriously damaged; and even when we come to temperatures of 100° C. in many cases the dielectric strength of the various materials used improves and the mechanical properties are not seriously damaged. But when the highest temperatures of all, 125° C. or from that to 130° C. are reached, then weaknesses begin to show themselves. So it appeared from the results generally of these experiments (and I may say, that the experiments were confirmed by a similar series undertaken by Messrs. Crompton at their works, and as far as I have seen the results of a similar series undertaken by Messrs. Siemens), that up to temperatures of 100° C. or 110° C., or perhaps a little over that, the materials which were likely to be used in field coils in practice did not show any

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such sign of deterioration as might condemn those materials for use in the various coils. However, these are matters for the members to discuss, and there will be members speaking who have much more intimate practical knowledge of the matter than I have. The figures are all given in the report. One or two very interesting facts are there noticed, especially the fact which has been the object of one of Mr. Rayner's experiments this evening; the way in which the temperature of cotton and indeed all other insulating material changes as the moisture is dried out. There is one very striking point to which attention is called in various parts of the paper, viz., the way in which after the material has been heated for some appreciable time, and kept at that high temperature for some moderate time, and then is suddenly allowed to cool, there is in a great many cases a very large sudden rise of resistance. The reason for that I think is perhaps indicated in the paper, the way in which the moisture is drawn back into the pores in the material, a suggestion which was made by Mr. Campbell. Then assuming that it had been shown in the experiments detailed in the first part of the paper that such temperatures as I have mentioned might be allowed in machines without damage, the question arose as to what temperatures were actually reached in machines under practice. It was possible, of course, by means of thermometers to measure the external temperature; but then very large variations (as Mr. Rayner has pointed out) can be got by the way in which you pack the thermometer. It is possible also to measure the average temperature of the coil by means of the change in resistance, and I think it will be found that the very simple portable apparatus which Mr. Rayner has devised—which is on the table for inspection afterwards—I believe the current is still running in the coil so that members will be able to plot for themselves, by turning the switch, the actual distribution of temperature in one of the coils that was used—by means of that portable apparatus it is quite easy to measure the amperes and volts in the coil, and so to know its temperature within a reasonable degree of accuracy. But that alone is not sufficient, because it is clear that the temperature at one point of the coil may exceed the average temperature by a very considerable amount, and by a varying amount. The experiments which were laid before the Institution some years ago by Mr. Brown had shown that clearly, and it was just a question as to whether there was quite sufficient variety in those experiments to allow the Standards Committee to work on them alone, or whether it might not be desirable to repeat those experiments, modifying the conditions on a series of coils of various sizes and various patterns. Thanks to the ready help which we received from the various manufacturers who have combined to produce this work, the latter course proved possible. Eight or ten coils were wound and had the thermo-junctions inserted in the special way described by Mr. Rayner. They were then, as he told you, tested at the laboratory to see what the distribution of temperature would be when they were not on the machine and not subject to the fanning action of the armature, and then they were tested under various conditions in the makers' works. So that the report has been the outcome of a combination between the

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resources, such as they are, of the National Physical Laboratory, and the very great and valuable help that has been given by the makers and others who have supplied the coils and assisted. I will leave it to others to discuss the results; and will in conclusion merely remind you that the work was initiated and carried out, as I have said, at the suggestion of one of the Committees of the Engineering Standards Committee, and the thanks of the Institution, I think it will be recognised, are due to the Engineering Standards Committee for the help they gave in carrying out this work, which I trust will be of real value to designers, and also for the readiness with which they consented to allow this piece of work to be laid before the Institution for public discussion, in order to make it as valuable and of as great use as possible. If suggestions are made, we will do our best to fall in with those suggestions, and to make the work still more useful than it is at present?

Discussion at Meeting of March 23, 1905.

Professor Silvanus Thompson.

Professor SILVANUS P. THOMPSON: The Institution is greatly indebted to the authors of these two papers. Mr. Rayner's paper, which recounts the work done at the National Physical Laboratory, is a most important piece of work, and I think all of us who have to use data for further calculations in machinery will appreciate it very highly, and appreciate it more as time goes on. It is a storehouse of facts, to which we shall go back again and again. I venture to say, Sir, that this being, I think, the first contribution which the National Physical Laboratory has made to the Journal of our Institution, the first long contribution, at any rate, of data worked out in the National Physical Laboratory, we shall look forward to future contributions with greater interest because of the importance that attaches to this. I do not propose to say anything about the first part of this paper with all the data as to the properties of dielectrics under heating, although there are a number of extremely valuable points that are brought out. When one goes through the numerical results, the further part concerning the heating of coils is also of great interest; and no doubt it has not escaped either Mr. Rayner or Dr. Glazebrook, that there are previous results which do not altogether tally with those they have found with reference to the heating of coils. There are also discrepancies between these observations and those communicated to this Institution some years ago by Mr. Brown, and also some made in America by Messrs. Neu and Levine. Their results differ in detail, probably because of some difference in the conditions under which the experiments were made. However, in the main point they agree that the hottest part of a coil is not at either of the ends of the coil (and in some cases is nearer to the outside than to the inside in the body of the winding), and in proving once more what had been already more or less shown by these earlier researches, that if a coil is fixed upon an iron core, the iron core does more than the air surface per square inch in promoting the getting rid of the heat. That result being confirmed and emphasised, one ought not, I think, to ignore it in calculations that are made concerning the temperature rise, or probable temperature rise, of coils that are being estimated.

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This takes me to the other paper by Mr. Goldschmidt, in which that very matter turns up. If I criticise Mr. Goldschmidt's paper I hope that he, at any rate, and the audience generally, will not suppose that I do not appreciate the paper for the good and useful things in it. I have not been able to gather by carefully reading the paper how Mr. Goldschmidt estimates the amount of cooling surface. For example, in the middle of page 665 he says: "In a field coil of an ordinary continuous-current generator, in order to obtain a temperature-rise of 90° F. (50° C.) above that of the atmosphere, measured by resistance-rise, about $4\frac{3}{4}$ sq. ins. (= 30 cm.^{2}) cooling surface are required for every watt lost." What I want to know is what that means? Does cooling surface there mean simply the outside cylindrical surface of the coil, or does it mean the outside cylindrical surface plus the surface of the end cheeks; or does it mean the outside cylindrical surface plus the surface of the end cheeks plus the interior surface of the coil where it comes up against the core? For if the core is more important as a cooling factor than the air-cooled surface on the outside, we ought really to have regard to the inside surface more than the outside. I therefore ask, merely for information, what it definitely does mean? Going back to the first page of the paper, I have found it a little difficult to understand the author's meaning in several passages. One difficulty that I have found—it may be my stupidity—has arisen because I have not always found it quite clear in certain sentences whether that which was being laid down was being laid down as a general principle or as a mere instance taken to illustrate some general principle. For instance, the author says at the end of the second short paragraph on page 660:—" By this means we obtain the following practical method of measuring the specific heat of copper and iron." Where is the practical method? I do not see it. What follows is a sentence about a certain number of watts being lost in a cubic centimetre of copper, which can be found out, quite irrespective of any measurement, from the known facts concerning copper and iron. On the remainder of the page there are some formulæ which I have found difficult to follow because of the terms employed. There are three symbols given, the first of which is V, which is described as the initial temperature increase. I think the author means there the initial temperature increase *per second*, which is a time rate, not simply a rise of temperature. The next symbol is W_v = volume — watts. Does the author mean us to read that as volume watts, with a hyphen in between, or is it volume minus watts? It cannot be, I know, from the context. Really what it means is this: it is the watts that are spent per cubic centimetre or per cubic inch as the case may be. Then the third quantity is C_v = rise constant. There, if I am not mistaken, the actual meaning of the quantity is this—the watts per cubic inch that will cause a rise of 1° per second as the temperature rate. The relations between them are certainly those that are set down at the bottom, $V = \frac{W_v}{C_v}$; which in language is this, that the rate at which the temperature rises per second, V, is equal to the number of watts spent per unit volume divided by the watts that must be spent per unit

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volume to produce a unit rise of temperature in one second. That hangs altogether, and is consistent with the dimensions in length, mass and time of those units. If one goes back to first principles, one can calculate quite simply the number of watts that must be spent to produce any given rate of temperature-rise (on the assumption that no heat is being given off by radiation or otherwise from the surface) purely from the specific gravity, the specific heat, the heating effect of one watt per second. In working that out for copper and for iron, we may take the numbers that are usually adopted—8·8 for the density of copper, 7·7 for the density of iron; 0·092 for the specific heat of copper, and 0·111 for the specific heat of iron. The numbers that are then obtained are these. The number of watts that must be spent to raise the temperature 1° C. per second in copper is 3·41, and in iron 3·557. I presume it is between those two that Mr. Goldschmidt has struck a mean when he says that "3·5 watts lost in one cubic centimetre of copper or iron raise its temperature 1° C. per second." The numbers I have given are simply deduced from the known facts and general principles. When one goes on further to the question of temperature rise per second it is very necessary to keep clear ideas about the time-rate. Going on to page 661, the author says at the end of the second paragraph—"but considering only 'the initial temperature increase,'" that again should be "temperature increase *per second*," otherwise it makes nonsense of the whole thing. Then, at the end of that very paragraph, the "0·19 watts" should be "0·19 watts per cubic centimetre," not a definite number of watts, but a definite number of watts per unit volume of the material.

I am sorry to appear captious over these matters, but I do not want anybody who has not worked out these things to find himself blocked in understanding the paper. About three quarters of the way down page 664 there is a rather obscure passage as to how the effect of cotton is to be taken into account. The author assumes cotton to have six times the specific heat of copper. Is that specific heat per unit mass or per unit volume? I presume it is unit mass, and if so it is too high, and four would be nearer than six. If we take it, as the author takes it, at six, the average value of the specific heat for the coil would not be $1 + \cdot 05 \times 6$; because you have to take off 5 per cent. from the copper. It will be 0·95 of copper $+ \cdot 05 \times 6$; or, if, as I think, it ought to be four instead of six, it makes the 1·30 times come down to 1·15, a much lower increase. A 30 per cent. increase is supposed as the effect of 5 per cent. of cotton being present, and I think that is much too high; it is, I think, about a 15 per cent. increase. The sentence that follows is again a little obscure because it wants defining. "It should be noted that the temperature is lower with field coils of thin wire containing much insulation, and having a higher average specific heat than with those having thick wire." That assumes something that is not stated. It would be quiet untrue for field-magnet coils if one assumes that the volume of the coil remains the same; it would be quite untrue if you assumed equal ampere turns and equal volume of coil. What it does assume is equal ampere turns in the coil, but the coil will have a bigger volume; so that unless some allowance is made for the increase of

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volume the statement as it stands clearly will not hold good. Take, for instance, this case. Suppose we had a small coil with one actual square inch of winding section, and it was all full of copper with a negligible amount of insulation. You send a current through it; there would be a certain current density. Suppose instead of having no cotton you used ordinary round wire, about half the space would be cotton or air and half copper; then certainly if you sent the same current as before round the same number of turns, and there is only half the cross section, the actual amount of heat developed there will be doubled, because the resistance will be doubled. The same number of turns with only half the cross section and the presence of the cotton there will certainly, with double the amount of heat developed, not make the temperature lower than before; it will be higher than before most certainly. I do not say the sentence that the author has written there is wrong; it is probably right for the case that he is contemplating, but it is not clear what case he is contemplating, and I ask him to make clearer what he really does mean.

Now I come to the paragraphs in which the temperature curve is taken into consideration. There, I venture to think, that in the effort to avoid mathematics, he has loaded his paper up with a great deal that is less easy to understand than the mathematical explanation would have been. For surely there is not very much trouble in following the curves of heating and cooling, following the ordinary laws. The way in which the temperature of the substance of a coil supplied with heat at a definite rate rises up to its final value by the expotential law is precisely the same as the well-known law by which that current rises to its final value when there is a self-induction in the circuit; the curves for the one would do for the other. Apparently the author makes the discovery when he has got a good way through this paper that all the curves he will have will be exactly the same shape, and that it is only a question of scale. It is so. All the curves of heating are of the same shape, and surely they might be treated a little more simply than he has treated them. Supposing we plot, as the author has done, in his Fig. 4, the time horizontally, and the excess of temperature over the surrounding air vertically. At the beginning the temperature rise starts off at a fairly uniform rate; and if there were no cooling surface getting rid of the heat the temperature would rise perfectly regularly, depending on the specific heat of the material, and would attain to the particular height (represented by the horizontal line through B) at the end of a certain limited time, which I observe the author calls the "ideal time" T. But if there is surface cooling the result is that the curve goes off to the right and goes up to the top value asymptotically. If now we write M for the maximum or final temperature rise, and θ_t for the value to which the temperature-excess has risen at the end of any time t, the relation between these quantities may, as is known, be represented by the equation

$$\theta_t = M\left(1 - \varepsilon^{-\frac{t}{T}}\right);$$

where ε is the basis of the natural logarithms, or $= 2{\cdot}718$. Theoretically this curve takes an infinite time to attain the maximum M; but at any

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time short of this, the temperature-rise will be a fraction of that maximum, namely, that fraction indicated by the factor in the brackets, 1 minus exponential minus $\frac{t}{T}$. The "ideal time" T, which is simply the "time-constant" of the equation, is a measure of the delay in reaching the top value: it indicates the time during which the rise will reach to $\frac{\varepsilon - 1}{\varepsilon}$, that is to $\frac{1.718}{2.718}$, or 63·4 per cent. of its final maximum.

If you look at any of the author's diagrams you will find that this is exactly what they show. The author takes as his "ideal time" the time during which it would get up to the top temperature if it went on as it starts; and he also draws the exponential curve to exhibit the effect of the surface cooling. If you look you will notice that the place where that curve crosses the vertical line drawn up to D through the end of the time-constant is always just 63 (or 63·4) per cent. of the top value. If we therefore find out for any coil what is the temperature to which it goes as its final value, we know that the time during which it gets up to 63 per cent. of that top value will be the "time-constant" of that coil, and it starts off at a rate that will carry it to the top value in the time which we call the "time-constant." I have ventured to translate into British units one example that the author has given. Suppose you have a coil which is losing 0·214 watts per square inch; suppose also that the current density is 1,100 amperes per square inch in the copper, and that the number, which he calls the heating constant, is, in centimetre units $\frac{1}{1500}$, or in inch units $\frac{1}{232}$, the final temperature will be the number of watts lost per square inch multiplied by 232. If you multiply those two together you will find it comes to exactly 50° C. as it is here. Now the initial rate of rise of temperature is determined solely by the specific heat of the materials and by the density of the current in the copper. In British units the initial rise can be calculated by simply squaring the ampere density and multiplying by 0·00000082. Multiplying the square of 1,100 by this long decimal you will find that the rise of temperature per minute, owing to there being specific heat in the copper, is 0·97 of a degree C. That is the initial rate of temperature-rise. If we are going to get up to 50° C., the product of these two, and it rises 0·97° in a minute, it will take just 52 minutes to get up to the top value. The time-constant, therefore, is 52 minutes. If there were no cooling, if it were merely a case of warming the thing up, it would rise to 50° in 52 minutes; but there is cooling; and therefore we know it will in that time not get up to 50°, but only to 63 per cent. of 50°. We get other useful points on the curve: at a time equal to half the time-constant, the heating goes up to 40 (or more exactly, 39·9) per cent. of the final value; in twice the time-constant the value goes up to 87 per cent., and in three values of the time-constant it goes up to 95·5 per cent. Those numbers, as a matter of fact, the author has inserted in Table X.

Finally, with regard to this heating constant which he gives us, of $\frac{1}{1500}$, which in square inch units becomes $\frac{1}{232}$. I prefer to use formulæ in which we use the reciprocal or integer 232 instead of working with

fractions, and for the following reason. For twenty years now, ever since Mr. Esson read the paper on Dynamo Design, where he gave us some formulæ, we have had heating formulæ of the very simplest kind, quite good enough for reckoning out the probable rise of portions of electrical machinery. He wrote for the final excess temperature

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$$\theta \text{ deg. Centig.} = \frac{\text{watts lost}}{\text{sq. inches cooling surface}} \times 55.$$

In other words, if you provided only 1 square inch of cooling surface per watt lost, the probable final rise will be 55° C. Other people say that is not enough, and you must admit that the rise will be greater. At the Oerlikon Works they find the constant to be 66. According to Messrs. Neu and Levine, that number, if measured by the internal resistance, ought to be 110, exactly twice as much as Mr. Esson's figure; and now this 232, which is the square-inch unit corresponding to the reciprocal which Mr. Goldschmidt gave, is another value for the same quantity. That is to say, according to Mr. Goldschmidt, if a coil is losing heat at the rate of 1 watt per square inch it will experience, as measured by resistance increase, a temperature rise of 232° C.; four times as much as Esson gave us (or in centimetre units it will heat 1,500° C. if losing 1 watt per square centimetre). I want to know from Mr. Goldschmidt to what this discrepancy in the numbers 55, 66, 110, and 232, which have been given by the different authorities, is due? I beg his pardon for having seemed to criticise his paper so much in detail, but I want to make it more useful than it is, and it certainly is a very useful paper.

Mr. H. M. HOBART: I propose to confine my remarks to the insulation section of the papers, but I will preface them by saying that I have no hesitation in pronouncing Mr. Goldschmidt's paper as, in my opinion, the best on the subject of temperature curves that has yet been published, and I believe it will be of great use to practical designers. Mr. Goldschmidt has done the work, and we, the readers, can readily see still further simplifications such as those which Dr. Thompson has so ably deduced from it. On the subject of the insulation sections of the paper I have made a few notes which will emphasise the importance of the investigation undertaken by Dr. Glazebrook and Mr. Rayner. The results they have obtained are certainly most interesting; but I would urge upon the authors the necessity of continuing their investigations until thoroughly reliable data on the subject have been obtained. To some curves deduced from their paper I have added others compiled from data already in my possession. These are all embodied in Fig. A. If time permitted, many interesting comparisons could be pointed out from these curves. In no instance, however, are the conditions of the test sufficiently specific to ensure results of maximum utility. The curves from my own data are equally faulty with those embodied in the paper, in that two important conditions have not been observed.

Mr. Hobart.

A great many interesting relative considerations can be deduced from these curves, but it is more for the purpose of the permanent record that I thought they might be useful. The two important con-

Mr Hobart. ditions which were overlooked constitute the subject for my two principal criticisms of the paper.

In order to obtain thoroughly reliable data the following amongst other conditions should be carefully observed. (1) The materials

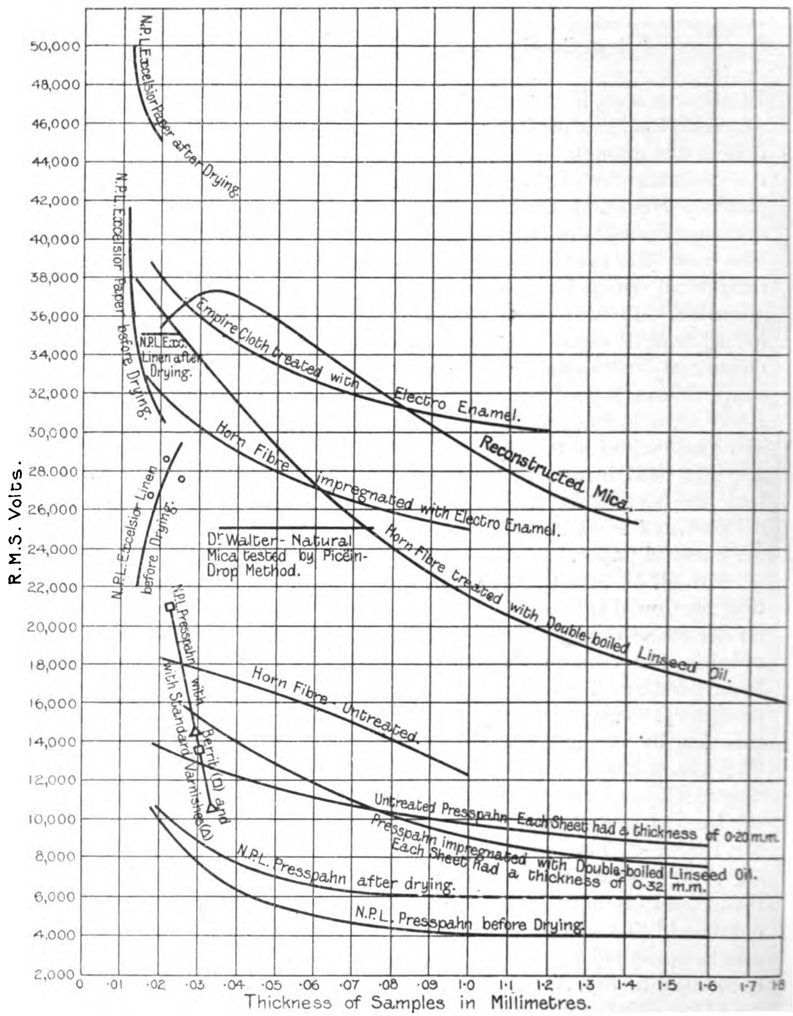

FIG. A.

should be thoroughly dried in a vacuum oven of moderate temperature for some considerable time immediately prior to the tests. (2) The voltage should be increased by small steps, and should be maintained constant at each step for definite intervals of, say, at least one minute, since the time of application often exerts a considerable influence on the disruptive voltage. The omission to observe either

of these conditions would, in my opinion, detract greatly from the usefulness of the results. Mr. Hobart.

The study of the behaviour of insulating materials with regard to heating may be brought into three principal subdivisions: (1) The permanent effect sustained as the result of prolonged exposure to high temperatures. For this purpose the measurements should be made on the samples after cooling to the ordinary temperature. (2) The disruptive voltage as a function of the temperature of the samples at the time of test. To determine this the samples should be tested in calorimeters maintained at given high temperatures. (3) The determination of the voltage at which perceptible heating is developed in the sample

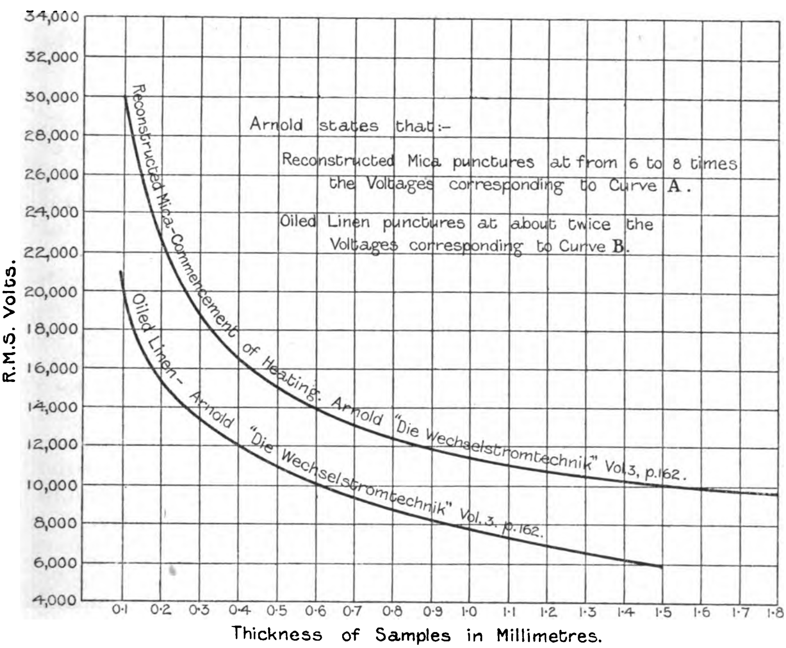

FIG. B.

as the result of the dielectric stress, and the determination of the voltage corresponding to given increases in temperature from this cause. In this connection, Arnold has given data from which I have compiled the curves in Fig. B. This phase of the question is of very great importance, and requires to be thoroughly investigated for these and many other materials. While the change in the insulation resistance due to moisture, as determined by measurements of the ohmic resistance, is an interesting subject which has been ably treated in the paper, there is no information given in the paper with regard to the effect of moisture upon the DISRUPTIVE strength. This can be conveniently tested by placing the samples in a compartment filled with steam of low pressure and temperature, and measuring the change

Mr. Hobart. in the disruptive strength with the lapse of time. This test is the more important for English manufacturers, since the humidity of the atmosphere is greater in this than in most other countries. Tests as to the influence on the disruptive strength, of prolonged immersion in oil, should also be made. On the whole I am of the opinion that the author's investigations could with advantage be extended on the lines I have indicated, so as to increase our knowledge of a subject in which a vast amount of information is needed. The results of such work must be of much interest to engineers in general, and to manufacturers of electrical plant in particular, and that this would be the case is borne out by an analysis of components of the cost of production of such plant. Thus for a 1,500 k.w., 600 volt slow-speed continuous current dynamo, the total cost for insulating material and for the labour of applying it, amounts to 5 or 6 per cent. of the total works cost of the machine. The armature insulation for this machine costs some 30 per cent. of the cost of the enveloped copper, and the field insulation costs some 12 per cent. of the cost of the enveloped copper. Mr. Goldschmidt makes a very interesting contribution to the study of the heating of insulating materials in pointing out that cotton has some six times the specific heat of copper, and in showing what important consequences this has on the temperature rise of apparatus for intermittent service.

Mr. Miles Walker. Mr. MILES WALKER : I should like to make a few remarks with regard to the heating of field coils. The conditions which affect temperature rise may be enumerated as follows : (1) The number of square inches per watt, (2) The velocity of the air past the coil, (3) The conductivity of the material of which the coil is made, (4) Whether the coil is ventilated or not, (5) The conductivity of the material between the coil and the pole, (6) The provision made for the cooling of the pole, (7) The closeness of the coils together, and (8) The hot bodies in the vicinity of the coil. Some of these conditions have been dealt with in the paper by Mr. Rayner, but the ordinary designer has to take not only the first three in consideration, but all the others. I thought it might be of interest to some of the members of the Institution to look into a method that has been adopted by the Westinghouse Company for investigating all these factors together, and for finding out by actual experiment which of the factors are the most important.

Supposing, in the first place, you have a large number of machines from which you can get data, we will say about 300 or 400 tests on machines with revolving field coils, and you want to find out from the tests what are the conditions which conduce to cool running of the machines. First of all, one has to eliminate the two most important factors, namely, the square inches per watt and the play of the air against the coil. That can be very well done in this way : Work out for all of the machines what the number of square inches per watt are; then find out what the actual temperature rise is. Then assume—and it is a very fair assumption which everybody makes—that if the condition is the same, the rise of temperature multiplied by the square inches per watt will be a constant; you can then plot that constant against any other factor you wish to inquire into.

Mr. Miles Walker.

Supposing, for instance, we make a curve like that shown in Fig. C. The abscissæ are the products of the rise in temperature multiplied by square inches per watt. The product is assumed to be a constant, provided everything else is the same; that is to say, if the number of square inches per watt is large, then there is a small rise. You assume that is so, but of course you find it is not a constant, and the thing that makes the variation is what you want to find out. The points come out in a constellation rather than in a curve. That is the

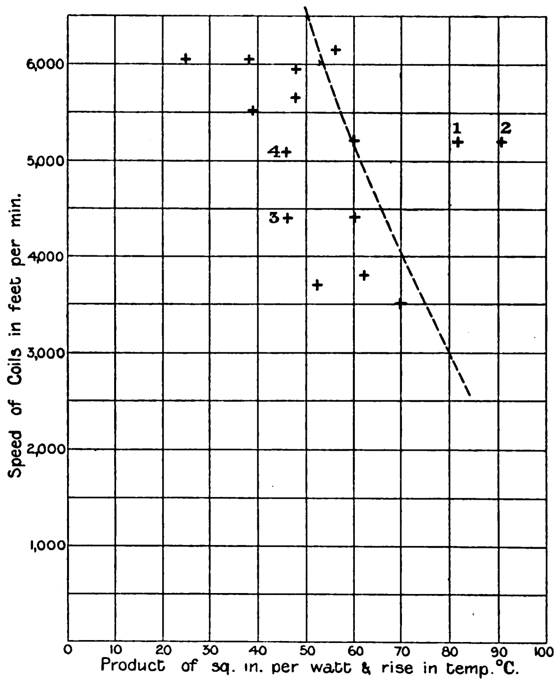

Fig. C.

most useful kind of diagram one can have, because one does not know where to draw the line; and if a line is drawn, a lot of points do not lie on it, and it is an interesting thing to find out why they do not lie on it. It can be seen that the machines 1 and 2 are not in the reckoning at all; there is something radically wrong by the look of them. A curve can be drawn as shown by the dotted line which fairly represents the limiting law for all the machines. Supposing I have a point like No. 3, I know that the cooling conditions of that particular machine were very good, because, notwithstanding the comparatively low peripheral velocity, the rise of temperature sq. in. per watt is only small. Supposing I had a curve of this kind, and that I wanted to find out the effect of some of the other factors, let us say the nearness of the coils together. (That is one of the most important things in con-

Mr. Miles Walker.

trolling tbe temperature of the field coils of revolving field machines.) I would look through all my data of these machines, and I would find probably three or four machines which were alike almost in every particular, except in the fact that some of the coils were nearer together in one machine than in another. Then I would find out which of these points on this curve correspond; perhaps one is No. 4, another No. 5, and another No. 2. I would find, as a general rule, that if the other factors are about constant for this machine, then as the only other controlling factor left is the closeness of the coils together, that the distribution of these three points with regard to the dotted line gives me the law of the heating of the coil which depends upon the closeness of the coils together, and as a fact it will be found that No. 2 machine has been built with the coils very close together. By plotting points out in this way one can ascertain what the effect of any one of these factors is. Supposing, for instance, I want to find out whether it is better to put a coil close to a pole and cool it towards the pole, or whether it is better to put a ventilating duct in; all I have to do is to find out two machines, one of which has a ventilating duct between the pole and the coil, and the other has not. The other main factors here are just about the same; and if the curve of one machine comes here and that of another machine there, then it is known which is the best.

Mr. Berry.

Mr. A. F. BERRY: I am speaking to-night as a visitor, at the President's kind invitation, the subject of Mr. Rayner's paper being particularly interesting to me. Before making the remarks I have to make I should like to congratulate the authors upon their systematic presentation of such lists of figures. They are exceedingly difficult to make palatable, and I for one have derived very much pleasure and instruction from the paper. There is so much that is beautiful that one has to pick about a good deal to find that which is merely useful. There is one main point that occurs to me in connection with Part I. of Mr. Rayner's paper, that is, he mentions how he realises the limits of laboratory tests, especially as regards the first portion of the paper. That brings one straight on to the methods of tests. If you have to test insulation I always look upon it that you may treat it in two ways, either as a thing separate in itself and self-contained, or you may treat it as part and parcel of the conductor, or group of conductors, to which it belongs. For varnishes and varnished materials, in fact, for the bulk of those things that are considered good or have been considered good, one is perfectly justified in adopting the first method of test, viz., that which has been exclusively adopted in Mr. Rayner's tests. Then with regard to the second method of test, if one aims at high temperatures and at getting considerably more duty out of copper, the only way to test an insulation properly is as part and parcel of the conductor or coil that it isolates. I know no other way. This is pretty obvious when you consider anything in the nature of an enamel that will stand a high temperature. It is brought, first of all, by the vehicle of cotton, or any other fabric, in as close contact as possible with the conductor; then it is matured either artificially or in actual running. When you use the latter method you get the alternate heat-

ing and cooling, and you get the various chemical changes that are absolutely necessary. In time and under heat the enamel leaves its first vehicle, the cotton or fibrous substance, and it gradually adheres to those conductors round it, and the whole thing becomes a solid mass. No one endeavours to calculate the strength, etc., of a building from tests on a single brick that has been merely sun-dried. Coils that are designed to withstand particularly high temperatures must be tested as a whole and mature, otherwise the tests are useless. I only just touch on that point because time is very precious in this place. What Mr. Rayner particularly draws attention to is that one must be very careful how one reads all these schedules of tests. They are very beautiful, but they may be worse than misleading if not read with very great discretion. The question of cheap production is the one that interests those of us who are manufacturers. The only way that is left for us to produce cheaply, now that every designer has had plenty of time to develop his ingenuity, is to get more duty out of the material. To get more duty out of copper it must be run hotter, and I think it is only when we have got the same amount of duty out of a pound of copper that we are now getting out of 1½ lbs. that we who are manufacturers will be permitted to live instead of merely existing. Again, overloading is inseparable from the rough class of work to which every day we are getting more accustomed, and overloading means high temperatures. So that we have to face that difficulty. I have pursued this subject of high temperature working for a number of years, and, as far as I am concerned, I have my methods and materials. The attacking of this question of temperatures is a very serious one. It involves a repudiation of an article of our faith—the belief in low temperatures—to a great extent. Perhaps I speak somewhat strongly, but it is better rather to speak too soon and too strongly than to have to face what I fear may be the result of worshipping low temperatures, viz., that we may very soon find ourselves following a lead instead of giving one. For Part II. of the paper one can have nothing but admiration ; the curves, the figures, the detail are admirable. The only thing is it takes a considerable amount of picking out. There are some very useful points. For instance, there are two coils (1γ and 1δ) wound with the object of seeing if more duty could be got out of a properly-treated coil than if it had simply been wound with dry cotton. The results are astounding. One finds that the difference between coil 1γ and coil 1δ is something like 50 per cent. in their maximum rise. It will also be found what is more important, perhaps, that coil No. 1γ exhibited a difference between its maximum and its mean temperature 100 per cent. greater than that shown by coil No. 1δ. That is a most important point, and I think one cannot well lay too much stress upon it. In fact, had No. 1γ not been taped that difference would have been greater. That is clearly proved, in spite of anything that may be said, as will be seen by referring to coils 4A and 4B. There we have a remarkable taped coil, the covering consisting of three thicknesses of varnished fabric and four of boot webbing. All that thickness of insulation made the

Mr. Berry.

Mr. Berry. difference between the maximum and the mean about 3 per cent. or 4 per cent., as against 4 per cent. or 5 per cent. when it was untaped. Taping a coil brings the mean and the maximum temperature rise nearer together. I was rather surprised to find that with all that thickness of covering in coil 4 the maximum temperature is only increased about 14 per cent., which is a very small increment for such a huge amount of covering. The point that most appeals to me, however, in this paper is the curve on page 657 showing the attempted electrical destruction of the cotton. There is an authoritative statement from the National Physical Laboratory, that if the temperature is raised from 60° C. up to 120° C. a megohm resistance something like ten thousand times as great is obtained, and if that temperature is maintained for about six weeks, I believe we learn it is about fifty thousand times as great. The trouble is that mechanically the cotton has been partially destroyed; but that authoritative statement is of enormous assistance to us, because on the continent and in England I have found the very greatest difficulty in persuading my friends that if they will only destroy the cotton they will get better results from it provided they put some enamel or japan with it to keep it in its place. For that proof I think Dr. Glazebrook and Mr. Rayner deserve our most cordial thanks. With regard to the curve of progress in the direction of temperature—and that is the only direction in which there is progress for us—it will be as it has been, an up-and-down sort of curve, but so long as the trend is upwards that is all we need care about. Evolution does not come without a bit of struggle; but there is no doubt that efficiency and regulation, or rather the want of them, will ultimately decide what temperature is and is not too great for a machine.

Mr. H. S. Russell. Mr. H. S. RUSSELL: I think the most important point in the paper is the question of the temperature limit of machines. Dr. Glazebrook has told us that the object of the tests was to enable the Engineering Standards Committee to fix a figure for the temperature rise to be allowed in electrical apparatus. My own opinion is that the results given in the paper show how dangerous it would be to fix such a temperature rise. Mr. Berry has already mentioned that in these days of keen competition the chief direction in which improvement in electrical apparatus can be expected is to get a larger output out of the same amount of material, and it seems obvious that these larger outputs can only be obtained by running the materials hotter. If they are going to fix a temperature limit for these machines, it seems to me that they would have to forbid engineers to overload their machines, and I do not think that can be done. It would discourage the designers and makers of machines whose machines will stand more overloading, because they can be run at higher temperatures. It is not many years ago that motor-driven vehicles were limited to a speed of four miles an hour, and you had to have a man in front with a red flag. While those regulations were in force of course any progress in the manufacture of motor-cars was practically impossible, because it was not worth any one's time to work at them, and it seems to me a temperature limit would be likely to have

a similar effect in restricting improvement in electrical machines. Mr. Berry pointed out the fact that when a coil was made solid by impregnating the cotton-covered wires with an insulating material of an enamel-like nature, it apparently gave the whole coil a greater conductivity for heat, and the surface of the coil a greater emissivity. The maximum temperature rise on an ordinary cotton-covered coil (1γ) was 46 per cent. greater than that on a coil (1δ) made solid with berrite, which is of the nature of an enamel. In connection with this point there is a statement on page 626 of the paper that specimens of insulation, after being heated at a temperature of 180° C. to 190° C. for some days, were practically destroyed. This bears also on another point on which Mr. Berry spoke, about testing the insulation apart from the conductors. I have seen coils made solid with berrite insulation which have been run for a considerable time at temperatures from 200° C. to 240° C., and at the end of that time appeared to be quite as good electrically as they were before; in fact, the insulation resistance was higher, and the mechanical properties of the insulation were better. I am afraid the other points I wished to mention have already been anticipated, so I will not detain you any longer. Mr. H. S. Russell.

Mr. A. RUSSELL: I have been much interested in these two papers. Mr. Goldschmidt's paper will, I think, be more appreciated by engineers than by physicists. As Dr. Thompson pointed out, his theoretical curve is the ordinary expotential curve, and that curve is, of course, only approximately correct. It is very interesting to notice how closely his experimental curves agree with it, and the question arises whether we could not invent some other curve that would fit in perhaps even more closely with the experimental curves; but that is a small point. I do not quite understand what Mr. Goldschmidt means when he says that approximate methods enable us to keep in closer touch with the real physical phenomena. I should have thought that the more assumptions we make the further we get from the physical phenomena. Mr. Rayner's paper is a valuable contribution to the subject. It will be appreciated, not only by those who have to design machines, but also by those who have to test them. In discussing the earlier part of the paper there is one suggestion I should like to make, and that is in connection with the method of measuring the dielectric strengths of materials. Mr. Rayner gives the dielectric strength in terms of the average volts per millimetre at which the discharge ensues, and that is the ordinary way of giving it. As Mr. Hobart showed, Professor Arnold apparently gives it in the same way. I take it that the dielectric strength ought to be measured by the maximum potential gradient or, as the older electricians would call it, the maximum electric force to which the insulating material is subjected. I think if we designed a testing apparatus so that we could calculate these maximum forces, then we should find that the average maximum force obtained by experiments on six thick samples would agree with that obtained by experiments on six thin samples. The breaking-down strength expressed in terms of the maximum electric force would come out the same in each case. Perhaps I could make this clearer by considering the dielectric strength of air. Mr. Thury Mr. A. Russell

Mr. A. Russell.

has lately published a careful experimental research on the sparking distances between two metal balls in air. Take two brass balls, each one centimetre in diameter, and placed, say, one centimetre apart; then, when the alternating effective voltage is 23,000 a disruptive discharge will take place. If we put these brass balls eight centimetres apart, then 63,000 volts are required to break down the air-gap. That would give us an apparent stress of about 8,000 volts per centimetre, as compared with 23,000 volts per centimetre when they are one centimetre apart. If we put them further apart the apparent volts per centimetre would be less. As Lord Kelvin pointed out a long time ago, the average volts per centimetre get less and less the further apart we place them. The easiest way of seeing why this should be the case is to consider the electrostatic capacity between two equal spheres. The capacity between two spheres when they are at some distance apart is very nearly equal to half the radius of either. It diminishes very slightly as the distance between the spheres is increased, and when they are at an infinite distance apart it is equal to half the radius. The potential gradient, or the electric force, or, as it is more properly called, the electric intensity, is a maximum at the surface of the two spheres. By Coulomb's law it is proportional to the surface density. Thus, since at considerable distances, the capacity between the two spheres is constant, the surface density for a given potential difference between them is also constant; so that the disruptive voltage is independent of their distance apart. It is impossible to maintain two spheres, however far they are apart, at a higher difference of potential than a certain maximum which is determined by their radii and the dielectric strength of the medium in which they are immersed. In order to get standard conditions we must design the testing apparatus so that we can calculate the maximum potential gradient. The case of two spheres has been worked out by Lord Kelvin. The formulæ are very complicated, and they are very troublesome to apply. If they prove too troublesome for electricians, I would suggest that we ought to proportion the size of our electrodes to the samples we have to test. If we double the linear dimensions of the electrodes when we are testing a sample twice as thick, I think it will be found that the disruptive voltage per centimetre will come out the same in the two cases. Why thick samples break down, when tested in the ordinary manner, at a less average electric intensity than thin samples may be explained as follows. If the electrodes are taken twice as far apart, the capacity between them is greater than half its former value, so that if we double the voltage we get a greater quantity of electricity on the electrodes than in the first case; and thus the maximum potential gradient is generally greater, and the thicker sample will break down at a less proportionate voltage than that required by the thin sample. There is plenty of experimental evidence to bear out this fact, and Mr. Rayner's experiments show it very clearly.

Mr. Peck.

Mr. J. S. PECK: Mr. Rayner's paper has been of great interest to me, as for a number of years I have been interested in the use of insulating material as applied to electrical apparatus. In the first

place I note on the second page of Mr. Rayner's paper that at a high frequency a higher voltage is required to break down a certain dielectric than at a lower frequency. I do not understand why this should be so; in fact, I should have anticipated just the opposite. I would, therefore, like to ask Mr. Rayner whether he determined the wave-form at the different frequencies. I should also like to know how the variations in voltage were obtained, and I think that a diagram showing the connections of the testing outfit would be of interest and value. It is well known that when a resistance is used for controlling the voltage supplied to a transformer, the wave-form may be very seriously distorted. I consider this question of frequency one of great importance, as affecting the testing of commercial apparatus, and I believe that with the same wave-form an insulation test at high frequency will be more severe than one at low frequency. This is undoubtedly true as far as the heating of the dielectric is concerned. Mr. Peck.

With regard to the results of the aging tests on insulating material at high temperatures, I think these results show conclusively that in general there is a marked deterioration, both mechanically and electrically, when insulation is maintained at a high temperature for any great length of time. Practically all of the samples show a marked deterioration in mechanical strength, and in the great majority of cases there is also a deterioration in electrical strength. Where no electrical deterioration is shown, it is not at all certain that it would not have developed had the test been continued longer. I believe that in commercial apparatus which is operated at a high temperature much more trouble results from mechanical deterioration than from electrical deterioration. I have seen apparatus which has broken down after being in service for a number of years, where the insulation was apparently entirely carbonised, so that it would crumble to powder at the slightest disturbance, yet its dielectric strength was reasonably good, and its insulation resistance extremely high.

The moisture trouble, which Mr. Rayner had with some of his field coils, is one which all manufacturers of high-voltage apparatus have encountered repeatedly. I have seen water distilled from ordinary cotton-covered magnet wire which was taken directly from stock, and almost all fibrous insulating material will absorb moisture unless it is thoroughly impregnated with oil or varnish. In the manufacture of high-voltage apparatus, great difficulty has been found in removing and excluding moisture from the insulation. The best way of removing moisture is to place the apparatus in an oven in which a vacuum is maintained, and which is heated to a temperature sufficiently high to drive off the moisture rapidly.

For excluding moisture, two general methods are in use. One is to remove the moisture in a vacuum oven, then impregnate the coils with some waterproof compound. With certain classes of apparatus this compound is admitted to the vacuum oven and forced into the coils under pressure. This method is, however, limited in its application. The other method is to place the complete apparatus ready for shipment in a vacuum oven, remove all of the moisture, then seal the

Mr. Peck. apparatus hermetically in its shipping cases, so that no moist air can come in contact with it during shipment. The Westinghouse Company has adopted this latter method on all of its very high voltage transformers.

Mr. Moon. Mr. W. MOON (*communicated*): When paper is exposed to the atmosphere it absorbs or gives off water vapour until its moisture is proportional to the hygrometric state, or degree, of saturation of the atmosphere, not to the absolute moisture.

The rate at which the paper absorbs or gives off water-vapour is proportional to the difference of the hygrometric state of the atmosphere and that of the paper.

A hundred grammes of paper taken from a cable, dried at 300 F., and afterwards exposed to the atmosphere :—

Time. H.	M.	Weight.	Time. H.	M.	Weight.
0	0	94	1	0	99
0	5	95	1	40	100
0	15	96	3	0	101
0	25	97	5	40	102
0	38	98	14	0	103

A hundred grammes of paper wetted by steam until damp to the touch and then exposed to the atmosphere :—

Time. H.	M.	Weight.	Time. H.	M.	Weight.
0	0	132	1	30	108·5
0	5	129	2	0	107·25
0	10	126	3	0	106·5
0	15	124·5	4	0	106
0	30	120	7	0	105·5
0	45	116	14	0	104·5
1	0	112			

Weight.	
100	paper in cable on drum.
103	short lengths of cable with ends exposed several months.
105	paper taken out of cable and exposed in sitting-room four days.
108	paper exposed in cellar four days.
93	paper dried in an oven at 170 F.

From these figures it will be seen that the paper in the cable contains at least 7 per cent. moisture, and that it will absorb 8 per cent. more if exposed to the air in a damp situation.

Mr. Symons. Mr. H. D. SYMONS (*communicated*): Experiments I have carried out during the last few months have led me to differ in some particulars from Mr. Rayner. The mere effect of heat on such dielectrics as have been tested is not nearly so disastrous to them as the variation of temperature which they are required to withstand when used for the insulation of machines. The continued heating and cooling they undergo seems to render them much more brittle than continued heat, provided the temperature attained is not sufficiently high to char

the material. When testing for puncture resistance the most satisfactory results have been obtained when the material is tested between a large metal disc and a small one, the smaller one having rounded edges and about one inch in diameter. The actual pressure on the dielectric does not affect the puncturing voltage, but a standard frequency and wave form are essential. To enable all tests to be comparable, it has been found most satisfactory to take the voltage which produces instantaneous rupture as the "puncture resistance." In order to do this the circuit must be closed on the high-tension side of the transformer, which entails considerable labour; but if not done, the puncture resistance is governed by the heat conductivity of the material under test. A simple explanation of the fact noticed by Mr. Rayner that varnished substances are more damp-proof when cold than the unvarnished, but not so when hot, is that the moisture can more easily be driven out of the unvarnished material. The experiments on the variation of the insulation resistance with variation of temperature are undoubtedly an indication of the amount of moisture in the dielectric, but that some dielectrics have a very high temperature coefficient appears to have been overlooked. That no definite relation exists between ohmic resistance and dielectric strength is certainly true, and forms a strong argument against measuring dielectric strength in any way but that mentioned above. In conclusion, it seems a pity that at present some suggestion for standard methods of carrying out tests for specific resistance and dielectric strength have not been made. Mr. Symons.

Dr. R. T. Glazebrook, F.R.S., communicated the following notes on the Theoretical Forms of the Temperature Curves within the Field Coils of Motors and Generators :— Dr. Glazebrook.

In the discussion on Mr. Rayner's paper, Mr. Goldschmidt called attention to the fact that the curves given were all parabolas, and this valuable remark of his led me to investigate the theoretical distribution of temperature in the coils, making some simple assumptions as to the thermal conditions existing in the interior.

Let P be a point in the interior of one of the coils, and consider a section through P by a plane at right angles to the surfaces of the coil and parallel to the axis of the core.

Let $A_1 P O A_2$ be a line through P in this section and at right angles to the axis. The temperature gradient is very much steeper, according to Mr. Rayner's curves, along this line than in the direction at right angles to it, while the distribution of temperature in the two neighbouring sections, one on either side of the one chosen, is nearly the same as in the section considered.

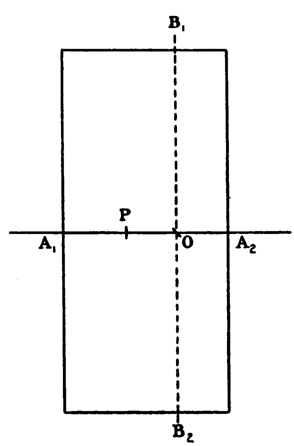

Fig. D.

Thus the greater part of the flow of heat in the coil takes place in

Dr. Glazebrook.

the direction of the line $A_1 O A_2$. We will suppose, as an approximation, that the whole flow goes on in this direction.

In the case of Mr. Rayner's curves to which we are going to apply the results, an additional justification is found for the assumption in the fact that the horizontal series of thermo-junctions was placed in a position for which the temperature gradient in a vertical direction was a minimum; thus along this line the flow of heat in a vertical direction was a minimum, and nearly the whole flow was horizontal.

Take the line O P as the axis of x, and take the point O in the line at which the temperature is a maximum as the origin. We shall determine the position of this point later.

We will treat the coil as of uniform thermal conductivity k throughout. The conditions at the two surfaces A_1, A_2 are different, the one being exposed to the air, the other to the action of the core. We can express this by supposing that the thermal emissivities h_1, h_2 of the two surfaces are different.

The quantity h_1 represents the heat leaving unit area of the surface per unit time for unit temperature difference between the surface and its surroundings.

Let v, V_0, V_1, V_2 be the temperatures at P, O, A_1, and A_2 respectively, each measured by its excess above that of the surrounding air.

Let $OP = x$, $OA_1 = a_1$, $OA_2 = a_2$, and let $A_1 A_2 = 2a$, the thickness of the coil.

Let H be the heat generated per unit of time per unit volume of the coil, we consider H as a constant.

Draw $B_1 O B_2$ through O at right angles to $A_1 O A_2$.

Then $B_1 O B_2$ is the line of maximum temperature, the heat generated to the left of this escapes through the left-hand surface of the coil, that generated to the right escapes through the right-hand surface through A_2.

Consider the heat travelling per unit of time across a small area S placed at P at right angles to $A_2 O$.

Its amount will be $-kS\frac{dv}{dx}$. But if the temperature is steady this must be the heat which is generated in a prismatic volume of the medium standing upon the area S and reaching a distance x from P to the line of maximum temperature. The volume of this prism is Sx, and the heat generated in it per second HSx. Thus equating these two we have

$$k\frac{dv}{dx} + Hx = 0,$$

whence integrating and remembering that the temperature at O is V_0

$$V_0 - v = \frac{1}{2}\frac{H}{k}x^2.$$

That is to say, the temperature curve is a parabola, as Mr. Goldschmidt pointed out.

Again, when $x = a_1$ $v = V_1$, the surface temperature, thus

$$V_0 - V_1 = \frac{1}{2}\frac{H}{k}a_1^2.$$

Dr. Glazebrook.

But the heat lost per unit time from unit area at $A_1 = h_1 V_1$, and this must be the heat generated in a prism on unit area reaching from A_1 to O. The amount of this heat is $H a_1$. Hence we have $H a_1 = h_1 V_1$,

$$\text{or} \qquad V_1 = \frac{H a_1}{h_1}.$$

Hence

$$V_0 = \frac{H a_1}{h_1} + \frac{1}{2}\frac{H a_1^2}{k}$$
$$= H a_1 \left\{ \frac{1}{h_1} + \frac{1}{2}\frac{a_1}{k} \right\}.$$

Similarly by considering the section of the coil to the right of O we have as the equation for the temperature as before

$$V_0 - v = \frac{1}{2}\frac{H}{k} x^2.$$

So that the temperature curve is the same parabola as before, while the boundary conditions give

$$V_0 - V_2 = \frac{1}{2}\frac{H}{k} a_2^2$$
$$V_2 = \frac{H a_2}{h_2}$$
$$V_0 = H a_2 \left\{ \frac{1}{h_2} + \frac{1}{2}\frac{a_2}{k} \right\}.$$

Equating the two values of V_0 we find after division by H

$$a_2 \left\{ \frac{1}{h_2} + \frac{1}{2}\frac{a_2}{k} \right\} = a_1 \left\{ \frac{1}{h_1} + \frac{1}{2}\frac{a_1}{k} \right\}.$$

$$\text{Also} \qquad a_1 + a_2 = 2a.$$

And these two equations give us a_1 and a_2, and thus determine the position of the vertex of the parabola, *i.e.*, the section of maximum temperature.

On solving these equations we find

$$a_1 \left[2 + \frac{k}{a} \left\{ \frac{1}{h_1} + \frac{1}{h_2} \right\} \right] = 2a \left\{ 1 + \frac{k}{a h_2} \right\}$$
$$a_2 \left[2 + \frac{k}{a} \left\{ \frac{1}{h_1} + \frac{1}{h_2} \right\} \right] = 2a \left\{ 1 + \frac{k}{a h_1} \right\}.$$

Thus

$$\frac{a_2}{a_1} = \frac{1 + \dfrac{k}{a h_1}}{1 + \dfrac{k}{a h_2}} \equiv \frac{K_1}{K_2}, \text{ say}$$

where

$$K_1 = 1 + \frac{k}{a h_1}$$
$$K_2 = 1 + \frac{k}{a h_2}.$$

Thus

$$a_1 = \frac{2a\ K_2}{K_1 + K_2}$$
$$a_2 = \frac{2a\ K_1}{K_1 + K_2}.$$

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Again, the latus rectum of the parabola is $\frac{1}{2}\,\frac{k}{\mathrm{H}}$.

By plotting Mr. Rayner's curves on a large scale the latus rectum of any of his parabolas can be measured, and thus since the watts dissipated per unit of volume of the coil are known we can find k.

Moreover, since $\mathrm{V}_1 = \frac{\mathrm{H}\,a_1}{h_1}$ we have $h_1 = \frac{\mathrm{H}\,a_1}{\mathrm{V}_1}$, but a_1 and V_1 are known from the curves, thus h_1 and similarly h_2 can be found.

If we assume the theory and also the values of h_1, h_2, and k, we can calculate in terms of H what the values of V_1, V_2, V_0, and V, the mean temperature, should be.

For putting as before

$$\mathrm{K}_1 = 1 + \frac{k}{a\,h_1} \qquad \mathrm{K}_2 = 1 + \frac{k}{a\,h_2},$$

we have

$$\mathrm{V}_1 = \frac{\mathrm{H}\,a_1}{h_1} = \frac{2\,a\,\mathrm{H}}{h_1}\;\frac{\mathrm{K}_2}{\mathrm{K}_1+\mathrm{K}_2} = \frac{2\,a^2\,\mathrm{H}}{k}\;\frac{\mathrm{K}_2(\mathrm{K}_1-1)}{\mathrm{K}_1+\mathrm{K}_2}$$

$$\mathrm{V}_2 = \frac{\mathrm{H}\,a_2}{h_2} = \frac{2\,a\,\mathrm{H}}{h_2}\;\frac{\mathrm{K}_1}{\mathrm{K}_1+\mathrm{K}_2} = \frac{2\,a^2\,\mathrm{H}}{\mathrm{K}}\;\frac{\mathrm{K}_1(\mathrm{K}_2-1)}{\mathrm{K}_1+\mathrm{K}_2}$$

$$\mathrm{V}_0 = \frac{\mathrm{V}_1+\mathrm{V}_2}{2} + \frac{1}{4}\,\frac{\mathrm{H}}{k}\,(a_1^2 + a_2^2)$$

$$= \frac{2\,a^2\,\mathrm{H}}{k}\;\frac{\mathrm{K}_1\mathrm{K}_2(\mathrm{K}_1+\mathrm{K}_2-1)}{(\mathrm{K}_1+\mathrm{K}_2)^2},$$

while

$$\mathrm{V} = \mathrm{V}_0 - \frac{\mathrm{H}}{6\,k}\;\frac{a_1^3 + a_2^3}{a_1 + a_2}$$

$$= \mathrm{V}_0 - \frac{2}{3}\;\frac{a^2\,\mathrm{H}}{k}\;\frac{\mathrm{K}_1^3 + \mathrm{K}_2^3}{(\mathrm{K}_1+\mathrm{K}_2)^3}.$$

Thus all the temperatures can be found in terms of known quantities.

As an example let us consider a coil such as the coil I of Mr. Rayner's series for which, as will appear later, we may take the following values for the interior and exterior conductivities :—

$$k = 0{\cdot}0155$$
$$h_1 = 0{\cdot}0095$$
$$h_2 = 0{\cdot}0045$$

the units being watts and inches. Suppose the coil to be 3 in. in thickness and let it carry 0·375 watts per cubic in., so that

$$a = 1{\cdot}5 \text{ in.} \qquad \mathrm{H} = 0{\cdot}375.$$

Then we find

$$a_1 = 1{\cdot}84 \text{ in.} \qquad a_2 = 1{\cdot}16 \text{ in.}$$
$$\mathrm{V}_1 = \mathrm{H}a_1/h_1 = 72^\circ \text{ C.}$$
$$\mathrm{V}_2 = \mathrm{H}a_2/h_2 = 97^\circ \text{ C.}$$

$$\mathrm{V}_0 = \mathrm{V}_1 + \frac{1}{2}\,\frac{\mathrm{H}a_1^2}{k} = \mathrm{V}_1 + 40^\circ = 112^\circ \text{ C.}$$

While

$$\mathrm{V} = \mathrm{V}_0 - \frac{\mathrm{H}}{6\,k}\,\frac{a_1^3 + a_2^3}{a_1 + a_2} = 112^\circ - 10^\circ = 102^\circ \text{ C.}$$

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Thus on this coil the position of maximum temperature will be 1·84 in. from the outer sides; the surfaces of the coil will reach temperatures of 72° C. and 97° C. respectively above the air; the maximum temperature will be 112° C. and the mean 102° C.

Again, it has been assumed in the above theory that the coil is of uniform conductivity for heat, but this is clearly not the case; it is made up of two materials: copper whose conductivity in c.g.s. units is about 1, and cotton of conductivity about 0·0001.*

If we measure the rate of production of heat in watts and take an inch as our unit of length, then each of these quantities requires to be multiplied approximately by 10 (actually by 4·2 × 2·54) so that the conductivity of copper, measured by the watts crossing each sq. in. of area where the temperature gradient is 1° C. per in., is 10, and that of cotton about 0·001.

Let us suppose each layer of the material of unit thickness to contain a thickness a of material of conductivity k_1, and a thickness $1 - a$ of conductivity k_2. Let T_1, T', and T_2 be the temperature of the first surface; the common interface and the second surface respectively Q, the quantity of heat measured in watts crossing per sq. in. in each second

$$Q = \frac{k_1}{a}(T_1 - T') = \frac{k_2}{1 - a}(T' - T_2) = k(T_1 - T_2)$$

if k be the resultant conductivity.

Thus

$$Q\left\{\frac{a}{k_1} + \frac{1 - a}{k_2}\right\} = (T_1 - T_2) = \frac{Q}{k};$$

$$\therefore \quad \frac{1}{k} = \frac{a}{k_1} + \frac{1 - a}{k_2}.$$

Hence

$$k_2 = \frac{k\,k_1\,(1 - a)}{(k_1 - k\,a)} = k\,(1 - a)$$

approximately; for, as will be shown shortly, $k\,a$ is small compared with k_1.

Thus k_2 can be calculated from a knowledge of k, the average conductivity, and a, the fraction of the thickness of the coil occupied by copper.

Mr. Rayner's observations enable us to check the above theory. From the curves the values of V_1, V_2, V_0, a_1, a_2 can be found, and the values of k, h_1, h_2 calculated. Each curve gives two values for k, which ought to agree if the theory be sound, while by taking further points on the curves additional values can be found. The equality of these various values forms one test.

Moreover, assuming the insulating materials on the various coils all to have approximately the same conductivity, there ought to be a connection between the values of k and the proportion of the coil occupied by copper, or, using the data somewhat differently, it should

* This is the figure given for cotton wool in the tables.

Dr. Glazebrook. be possible to find from the mean conductivity and the proportion of copper the conductivity of the insulating material.

Again, the values of h_1, h_2 depend on the surface conditions, and for different coils under similar conditions ought not to differ greatly. On referring to Mr. Rayner's table, it will be seen that a number of observations were made on coil No. 1 under various conditions.

The following Table I. gives the results of the application of the theory. In the Table the values of k, the average conductivity for the two parts of the coil, are given, as well as the mean conductivity; also the values of h_1, h_2, the exterior conductivities, and k_2, the conductivity of the insulating material. H represents the watts per cubic in., and a the proportion of copper per in. thickness of coil.

The table also gives the actual loss of heat per unit area over the two surfaces as given by theory, and the average loss over the entire surface as calculated from the total heat generated and the area of the surface.

The results are of considerable interest. The values of the interior conductivity for the two halves of the coil are in close agreement. The conductivity for 1_β, in which the thickness of the wire was 0·044 in., is distinctly less than for 1_γ, where it was 0·072 in., while that of $1\,\delta$, which was "berrited," is higher still, being double that of the original coil. The exterior conductivity for 1_β, which was running, is greater than that 1_γ for which was at rest, though this difference is not so marked as in the case of some of the other coils.*

The exterior conductivity h_1 of the outer half of 1_γ is throughout the same; until we reach 1_γ F the exterior conductivity h_2 of the inner half is strikingly the same; that of 1_γ F is larger because there was no core. For $1\,\delta$, the berrited coil, both the exterior conductivities are increased above their values for 1_γ.

Except for 1_γ F and $1\,\delta$ the actual average loss of heat lies between the two observed losses, and though this is not the case for 1_γ F and $1\,\delta$, it must be remembered that no allowance has been made for variations of temperature in the vertical direction.

The values found for k_2, the conductivity of the insulating material, are of the same order as that for cotton, though in view of the great uncertainty of the ratio of the copper to the whole thickness no great weight can be attached to this.

Thus we might conclude that the behaviour of a coil made of materials such as those in 1_γ, used under similar circumstances to those described in Mr. Rayner's paper, could be predicted by assuming an average interior conductivity of 0·0155 and exterior conductivities of 0·0095 and 0·0045. The values of the temperatures for such a coil have already been given.

The results can be tabulated similarly for the other coils, and are given in Table II. In view of the near equality for the interior conductivities within the two halves of each coil it has seemed sufficient to tabulate only the mean.

An examination of the table shows that with the exception of 4, B,

* See Table II.

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TABLE I.

Coil.	H.	k			a	k_2	h_1	Actual Loss.	h_2	Actual Loss.	Observed Loss.
		Outer side.	Inner side.	Mean.							
I β											
A_R	0·508	·0110	·0107	·0108	·88	·0013	·0110	·62	·0059	·45	·49
I γ											
B_{SC}	·460	·0162	·0158	·0160	·85		·0106	·69	·0047	·41	·44
C_{SC}	·380	·0152	·0160	·0156	·85		·0104	·57	·0044	·33	·37
D_{SC}	·310	·0145	·0145	·0145	·85	·0024	·0092	·41	·0044	·27	·30
E_{SC}	·250	·0155	·0157	·0156	·85		·0089	·32	·0044	·22	·24
F_{SNC}	·410	·0162	·0156	·0159	·85		·0097	·55	·0064	·42	·40
I δ											
F_{SNC}	·380	·0228	·0228	·0228	·85	·0035	·0118	·50	·0103	·46	·37

R = Running. S = Standing. C = Core in place. NC = No Core.

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TABLE II.

Coil.		H	k	a	k_2	h_1	Actual Loss.	h_2	Actual Loss.	Observed Loss.
2	A_R	·473	·0199	...	...	·0138	·89	·0060	·53	·68
	B_R	·465	·0212	·83	·0034	·0153	·87	·0065	·53	·67
	C_{SC}	·284	·0195	...	...	·0066	·54	·0032	·32	·40
4*	A_R	·240	·0285	·68	·0085	·0049	·36	·0014	·12	·35
	B_R	·240	·0141	...	...	·0080	·36	·0018	·12	·35
	$C_{S\,NC}$	·300	·0207	...	...	·0055	·42	·0021	·19	·44
5	A_R	·182	·0112	·89	·0009	·0138	·32	·0091	·27	·31
6	A_R	·070	·0272	...	...	·0350	·25	·0067	·12	·12
	B_S	·072	·0299	...	...	·0102	·22	·0063	·16	·12
	$C_{S\,NC}$	·203	·0271	...	...	·0152	·64	·0071	·44	·36
7*	A_R	·162	·0059	...	...	·0037	·20	·0026	·16	·19
	B_{SC}	·152	·0048	·81	·0009	·0058	·20	·0025	·13	·18
	C_{SC}	·282	·0043	...	...	·0060	·54	·0017	·18	·33
8	A_R	·260	·0112	·58	·005	·0084	·37	·0031	·21	·38
	B_R	·562	·0126	...	...	·0077	·80	·0033	·47	·83
	C_{SC}	·275	·0122	...	...	·0039	·39	·0019	·23	·41
	D_{SC}	·237	·0109	...	...	·0037	·34	·0018	·20	·35

R = Running. S = Standing. C = Core in position. NC = No Core.

* See p. 719.

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and perhaps 7, the values of k found in the various experiments for the same coil are in fair agreement.

Coil 4 (Table II.) was, according to Mr. Rayner's paper, covered with "three layers of varnished cambric and four thicknesses of boot webbing." These were removed before the test B. Moreover, a series winding was enclosed in the same taping. When the coil was standing there was no current in this winding. Thus the condition of the coil was entirely altered between the two tests. A brass plate under the taping also produced an anomalous effect.

Coil No. 7 was covered outside by the series winding; thus the conditions assumed in the theory did not hold, and the results are anomalous.

Coil No. 3 is omitted; the section of the coil is irregular and the curves anomalous.

The values of h_1 show clearly the effect of the fanning action caused by running the machine. In coils 2, 6, and 8 the values of h_1 for the running tests are from two to three times as great as those for the tests when the coil was standing. In coils 4 and 7 this is not the case, but the fact that the series windings were used in the running and not in the standing tests sufficiently accounts for this.

The differences in the value of h_1 and h_2 for the various coils are considerable. Coil 8, in which the figures are low, was covered with $\frac{1}{8}$ in. canvas, paper and leatheroid, and $\frac{1}{8}$ string, all varnished. This accounts for the low value of a and of the average conductivity.

Coil 6 was only varnished on the outside. The average conductivity is about 2·5 times as great as for 8, and the emissivity about four times as great.

Coil 5, which has the same exterior conductivity on the outside as 2, was also varnished, but the "formers were arranged to secure considerable cooling effect on the core side," and this is evidenced by the high value of h_2 0·0091, as against 0·0060 for 2 and 0·0031 for 8, the thickly-covered coil.

Attention should be called to the fact that in a number of cases the watts per cubic inch vary over a large range, *e.g.*, from 0·46 to 0·25 in 1_γ, from 0·20 to 0·07 in 6, and from 0·56 to 0·24 in 8, without seriously affecting the value found for k.

The values of k_2, the conductivity of the insulating materials, vary greatly, as might be expected from the uncertainty of the data, but are all comparable with the conductivity of cotton.

Probably enough has been written to draw attention to the importance of Mr. Goldschmidt's remark to which this note is due, and to justify the hope that by a series of experiments on one coil, makers can determine with ample accuracy the constants k, h_1, h_2, and thus deduce the thermal conditions which will prevail in other coils not altogether dissimilar in pattern.

Mr. Goldschmidt.

Mr. R. GOLDSCHMIDT (*in reply*): I should like to make a few remarks on the second part of Mr. Rayner's paper—the distribution of the temperature in the interior of field coils. One can very easily follow up this problem by a calculation, and so find that the curves which represent the distribution of the temperature have quite a definite

Mr. Goldschmidt.

shape; in fact they are parabolas. The conclusion which can be drawn from this fact is that the highest and the mean temperature have a definite relation one to another. I find that the difference between the maximum and the outside temperature is just 50 per cent. higher than the difference between the mean temperature and the outside temperature. If, for instance, the temperature measured by resistance is, say, 40 deg. higher than that measured by the thermometer, then the maximum temperature is $1{\cdot}5 \times 40 = 60$ deg. higher than the temperature measured by the thermometer. This rule, if applied to Mr. Rayner's examples, checks within 5 deg. with twenty-two of the thirty cases. It offers a very easy means of calculating approximately the highest temperature inside when the temperature by the thermometer and by resistance measurement are given. Errors are caused chiefly by the inequality of the cooling effect of the inner and outer surface; to allow for this one does well not to calculate with the theoretical 50 per cent., but with 67 per cent. Mr. Rayner's tests all refer to single field coils. When measuring the resistance rise it is usual, in actual practice, to measure the resistance of the field system as a whole. There are sometimes not inconsiderable differences between the different field coils, and an inaccuracy is caused through the measuring of the whole of the field coils in series. With slow-speed machines which are half closed or very broad, or generally with all machines where the hot air coming from the armature and the bottom field coils can accumulate round the top field coils, the latter are considerably hotter than the bottom coils. I observed as an outside limit 30 deg. F. difference between the top and the bottom coils. With very thin or well inside ventilated field coils the average measured by resistance rise can be smaller than the maximum temperature which could be obtained by thermometer. I observed in a special case that the temperature measured by resistance rise was smaller than that which could be registered by the thermometer. This was on a big transformer, but practically it is the same as with field coils. One might conclude from this that in a case where particular value is set upon a low temperature or a guaranteed figure to be verified, the temperature ought to be measured by resistance rise on a single field coil only. At any rate, I think that in some cases the distribution of the temperature over different coils can become as important as the distribution inside.

I have to thank Professor Thompson for following up the problem of temperature curves in a more mathematical way. As mentioned in the last paragraph of my paper, I prefer approximate methods, or rather "point-to-point methods," chiefly because it is so easy to apply them when the load, resistances, and other conditions vary. General results naturally can be obtained quicker mathematically, but for the application in actual practice I think the approximate method handier than the mathematical one. This applies also to Mr. A. Russell's remarks on the same subject.

Professor Thompson raises the question of heating constants, and compares the figure which I used in my examples with constants of other authors. As mentioned in my paper, this question is too complex to be fully dealt with in a study on temperature curves as far as they

influence the rating of machines, and I thought of giving a separate paper on this subject. With semi-enclosed medium-size machines the heating constant $\frac{1}{1500}$ is a good average, the temperature rise being measured by resistance increase, and the *whole* surface of the coil counted as cooling surface. If other authors have observed less temperature rise for a given number of watts per square inch surface, the reason must be that they did not measure the temperature by resistance rise (Esson), that they considered only the outer cylinder as cooling surface, or studied well-ventilated, open-type machines. The ratio of the temperature rises of stationary field coils for best and worst ventilation (totally enclosed machines) for the same number of watts per square inch is about 1 : 3. Mr. Goldschmidt.

The specific heat of cotton is stated by Professor Thompson to be about 4, instead of 6, so that the specific heat of the whole coil is found to be about 1·15 instead of 1·30 times that of pure copper. This calculation has only been carried out for obtaining an idea of the theoretical effect of the cotton insulation. The examples show that the coefficient in question actually is about 1·25. Very likely the air enclosed in the spaces between the wires and the influence of heat conduction increase this factor.

Professor Thompson further comments on my statement that the initial temperature increase per second is lower the higher the amount of cotton in the coil. The assumption on which this sentence is based is "constant current density," regardless of the actual section of the coil. It simply means that the initial temperature rise per minute taken from my Table I. is to be reduced by a greater amount the higher the ratio cotton : copper.

Mr. RAYNER (*in reply*): I would first of all draw the attention of members to the diagram (Fig. E) which Dr. Harker has kindly prepared for me, as there has not been time for Dr. Harker to do it himself. It shows the correction which must be applied to a voltmeter to read temperature centigrade correctly when the "cold" junction is at zero centigrade, and the voltmeter reads 100 at 100° C. The two curves have been drawn for iron-eureka and copper-eureka couples. The former has a much straighter curve than the latter, the error at 50° C. being about 0·3° C., while the latter has an error of about 4°. At 400° the errors are of the order of 12° and 63°. Mr. Rayner.

Dr. Harker is at present considering the question of pairs of metals for commercial measurement of temperature, to supplement and perhaps take the place of the costly metals and alloys of the platinum groups. An incidental advantage of iron-eureka is that its thermoelectric voltage per degree is some four times that of the usual platinum and platinum alloy thermojunctions.

I am very much obliged for the very kind and appreciative remarks of Professor Silvanus Thompson. There is one point which I do not quite understand: that other experimenters have found that more heat is abstracted from the core side of the coil than from the side exposed to the air. This I have not found to be the case. In general, if the coil be a tight fit on the pole, I have found that the pole becomes hot, and when a steady condition is

Mr. Rayner.

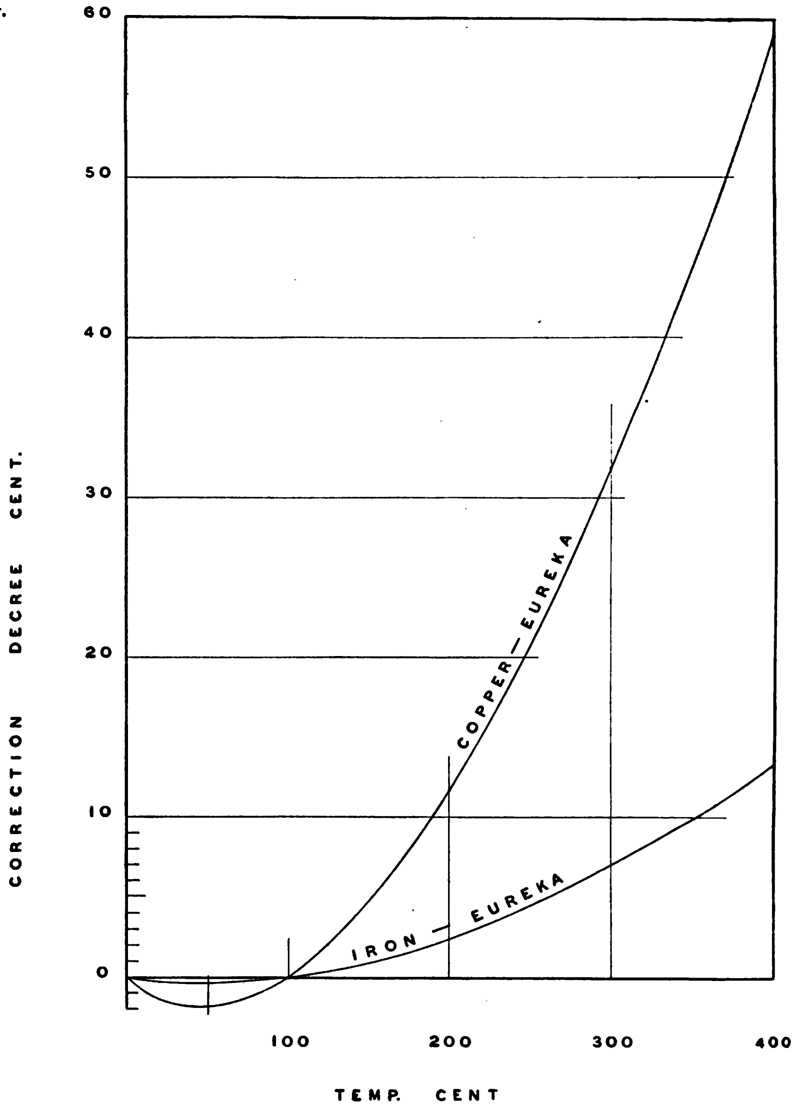


FIG. E.

arrived at, little relief is obtained by heat abstraction on the core side. This is illustrated by curve B, transverse section, coil No. 4. If by the use of a metal former or otherwise an air space is left between coil and pole, then the cooling on the core side is much more considerable (see coil 5), which shows that the use of a metal former is perhaps of more advantage than is sometimes considered to be the case.

On referring to the paper by Messrs. Neu and Levine, mentioned by Professor Thompson, after discussing results obtained under artificial conditions in which they refer to the abstraction of heat by the iron core, the following paragraph will be found :— Mr. Rayner.

"When the machine is running, the iron does not cool the coils as much as when the machine is not running. The hottest part of the winding is halfway up, close to the core, showing that very little heat is conducted away by the core, as it is itself warmed by radiation from the armature, by hysteresis and foucault currents in the pole-pieces, etc."

Naturally, circumstances alter results considerably, and though not specifically stated, it appears from the diagrams that the coil (on a former) had considerable external covering, which would naturally cause less heat to be radiated from the outside than would otherwise be the case. Especially would this be so when, as in some experiments, the coil was tested suspended without a core, when more heat was dissipated from the inside than from the outside.

I am very greatly obliged to Mr. Hobart for his criticism and advice. The committee under whose direction this work has been done had the figures quoted in the book by him and Mr. Parshall before them as to the effect of duration of electric stress on such substances ; and the interesting figures there quoted, the experiments for which would take a long time, were considered by the committee to be an important point to be dealt with in any recommendations that they might make.

At the same time a repetition of these experiments would probably have given results not materially differing from those quoted, and, for the present at any rate, it was not considered necessary to perform these tests. Personally, I was always on the look out for substances on which some such tests could, with satisfaction, be applied, but the differences in disruptive voltages in adjacent parts of the same specimen is so great, as to make such tests very deceptive unless averages of large numbers are taken, and even then it is the lowest value obtained which should be taken. These tests have naturally been considered more or less comparative. An examination of the list will show that they are such substances as would be used for low voltage continuous-current machinery and transformers, and not for high voltage plant. I mean by high voltage, 2,500 to 10,000; on such mica is, at present, the only substance used, and that has not been dealt with. I would point out that the disruptive voltage appears, for low voltage machinery, to be a matter of very little moment. In ordinary machinery the insulation would be as effective if it were pierced with small holes. In other words, the same thickness of air is amply sufficient to prevent disruptive discharge. Even in transformers, adjacent metal parts have usually very small voltage differences ; and when this is not the case, the required insulation is obtained, not so much by the dielectric strength of the material, as by its thickness.

With regard to the point that it would have been more useful to have had experiments done on substances which had been warmed sufficiently to dry them thoroughly, it happened in many cases that we had not sufficient quantity to make such a test and also a test on the

Mr. Rayner. specimens absolutely untreated; and as in the latter case the disruptive voltage was considerably lower, the experiments were done with the substances in this condition, in order to give results of the various specimens in their weakest state. If other similar work is done the point will not be lost sight of. In fact, from the appearance of the surface of substances which have been subjected for a short time to, say, 3,000 volts and upwards, one would say that any organic material must fail, in time, if subjected to a voltage sufficient to spark through the same thickness of air. The surface appears to be gradually eaten away. It may be possible to run oil transformers under these conditions, in which case one would be relying on the oil. I consider that the limiting condition of all these fabrics is their brittleness. I have, in visiting the makers' works, examined what machines I could find, which had failed, to see if any such failures could be ascribed to any other cause than mechanical brittleness. Makers of course know the usual point is where the armature bars leave the core slots, and the cracking of the insulation is due to the rocking forces set up between the magnetic field and current in the armature bar. It, therefore, seems that the most important point to be considered is an insulator which will remain reasonably pliable after being raised many times to as high a temperature as other circumstances permit.

I am much obliged for the interesting remarks of Mr. Miles Walker, and hope the figures in the paper will be of some use to him.

Messrs. Berry and H. S. Russell have dwelt on the possibility of the satisfactory running of electrical plant at high temperatures. The tests have been, as far as possible, carried out under running conditions. As I have remarked, the tests have been practically confined to substances subjected to low voltages only.

With regard to Mr. A. Russell's remarks, the voltage is the ordinary root mean square. The electrodes used had flat circular surfaces 1 inch in diameter. Mr. Russell's remarks concerning potential gradient and disruptive voltage are very interesting; but if the method used really gives a lower result than a more theoretically perfect one, by which various tests would be comparable, it would appear best to use the one which gives the lower value, as such conditions must obtain in practice. As regards the actual testing apparatus, we had to use what plant was available. A rotary converter gave an alternating E.M.F. of 60 volts. The testing transformer of 10,000 volts required a primary current of 200 volts. An intermediate transformer, 1 : 5, was used with resistances in the primary, and a choking coil in the secondary circuit to give a fine adjustment. The curve of E.M.F. of the rotary converter was a smooth one. I cannot say what the actual curve of the high voltage was, as the choking coil which was used to give the fine adjustment may have slightly affected it. As we found that there was a difference in the disruptive voltages according as the rotary converter was run fast or slow, I tested the sparking difference in air between two brass rods of ¼-inch diameter with hemispherical ends. These showed the same property as regards sparking distance, which may roughly be represented by :—

Mr. Rayner

$$\gamma = 1{,}200 + 1{,}700x \quad \text{at } 56 \sim \text{ per sec.}$$
$$\gamma = 1{,}800 + 1{,}700x \quad \text{at } 36 \sim \text{ per sec.,}$$

x being the distance apart in millimetres, between the limits of 1·5 and 3·0 mm., and γ the disruptive voltage. This effect may be a genuine one, or due to alteration of wave form with alteration of speed; I cannot be certain. But, as I said before, the results are purely comparative, and have been obtained under the same conditions. The frequency used was 50.

Mr. Peck has mentioned the fact that insulating substances may be quite good electrically when useless from a mechanical point of view, and I strongly agree that brittleness is the first danger to beware of, if high temperatures are to be aimed at. At the last meeting members will recollect that we measured the actual insulation resistance between two layers of cotton-covered wire, and that the resistance changed according to the o curves already described. Finally, we took it up to a temperature of 400° C. or even more. That is not very far short of visible redness, so that cotton will stand a great deal. If it had been in the open-air it might not have lasted so long without partial burning. It was quite black, but still a good insulator compared with a wire which had not been treated in any way. As regards the drying-out of electrical plant, I have had my opinion confirmed that the proper way to treat machinery is to put it in a heated vacuum tank and pump out the water vapour.

I am greatly obliged to Mr. Goldschmidt for pointing out results which can be deduced from my paper. I confess that I had not expected that they would be easily amenable to mathematical analysis on account of the many independent variables, and the comparatively small number of coils experimented upon. In fact, I preferred to give all possible data in the tables, and let designers and others tabulate any further data deducible from them in their own manner. If it will be of any interest to Mr. Goldschmidt, I shall be only too glad to give him the full figures from which all the curves have been calculated.

The figures given by Mr. Moon for the hygroscopic property of cellulose in the form of paper appear to agree materially with the figures given for cotton, which were confirmed by results obtained and recently published by the Testing-house of the Manchester Chamber of Commerce.

In reply to Mr. Symons, the fact that alternate heating and cooling is more trying to insulating materials than continual high temperature, would naturally be suspected; but it is of importance to hear that the effect is possibly much greater than one would expect. It would appear that the "instantaneous puncture voltage" test would give considerably higher results than the method described; and, as long as discrepancies of 20 per cent. may be found between adjacent parts of the same specimen, I would be inclined to use my easier method, which gives more conservative figures. I have not found, except in the case of unheated substances, especially treated fibre and the like, any very appreciable temperature rise due to the application of the voltage for the short time this method of testing requires. Further,

Mr. Rayner. the puncturing voltage does not appear to be a matter of real importance, as has been previously explained, for the class of plant in which these insulating substances are used. It would appear probably that the damp-proofness of varnished substances is due not so much to the locking up of the moisture as to the fact that the actual varnishing of fabrics, probably previously well dried, prevents moisture getting in at all in anything like the same quantity. Though, perhaps, not explained in the paper, various experiments have been made on the temperature coefficient of ohmic resistance of varnished substances. The results are qualitatively very similar to those indicated in Diagram E, showing an enormous temperature coefficient. As to standard methods of testing dielectrics, my opinion is that the simplest of all, the bending test round a cylinder, is the most useful. The punching test has confirmed the results of the bending test. As to electrical tests, full details have been given of the apparatus used, but before attempting anything in the way of standard apparatus, it would be very desirable to have an interchange of views between such persons as have worked at the subject.

The President. The PRESIDENT : Gentlemen, it is my pleasant duty to ask you to give a cordial vote of thanks to the authors of these papers. I think you will all agree with me that they have very great practical value. Without wishing to go very deeply into the subject, I have been struck with one fact which Mr. Rayner mentioned—that the insulation resistance of the cotton-covered field-coil remains constant for a certain time, until the temperature of the copper wire begins to rise, and then it comes down in the most extraordinary way when the moisture is being driven out. That accounts for a lot of failures in field-coils which formerly gave us a good deal of trouble until we found the remedy ; when the machines were completed we knew we had to warm them up very carefully so as to drive all moisture out before any tension at all could be used on the windings. It is very interesting to have it confirmed in Mr. Goldschmidt's paper that you can get a much greater duty out of a dynamo machine, or out of a motor, if you only run it for a short time before the permanent conditions have really been attained. I will now ask you to give a hearty vote of thanks to the authors for their interesting papers.

The resolution was put and carried by acclamation.

Proceedings of the Four Hundred and Twenty-Second Ordinary General Meeting of the Institution of Electrical Engineers, held in the Rooms of the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 23, 1905—Mr. ALEXANDER SIEMENS, President, in the chair.

The minutes of the Ordinary General Meeting held on March 16, 1905, were taken as read and confirmed.

The list of candidates for election into the Institution was taken as read, and it was ordered that it should be suspended in the Library.

The following list of transfers was published as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members—

Chas. Wm. Speirs.

From the class of Associates to that of Associate Members—

Arthur Frederick Russell Curteis.
John Edgar Edmundson.
Percy Godfrey Pettifor.
Clifford Copland Paterson.

Messrs. H. Howard and W. Henderson were appointed scrutineers of the ballot for the election of new members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

Member.

John McLorinan Robb.

Associate Members.

Robert Geraint Cooper.
Edward Ayerst Davies.
Albert Davis.
Bertram Glenny.
Robert D. Bagot Glenny.
Lancelot Ovid Heyes.
William Arthur Heyes.
Edmund Charles Hopkins.
Ernest Wellesley King.
Edwin H. Rayner, M.A.
Joseph William Wyatt.

Students.

John Angus Allan. | Ernest L. Pretraud.
Claude Francis D. Suggate.

Donations were announced as having been received since the last meeting to the *Library* from Mr. E. S. A. Robson ; to the *Building Fund* from Messrs. G. B. Byng, M. B. Byng, H. Hirst, L. Wood ; and to the *Benevolent Fund* from Messrs. G. B. Byng, M. B. Byng, J. J. Chapman, H. Hirst, J. P. Lawrence, E. G. Love, H. L. Riseley, C. H. Shanan, to whom the thanks of the meeting were duly accorded.

The Discussion on Mr. E. H. Rayner's and Mr. R. Goldschmidt's papers was concluded (see page 694).

The meeting adjourned at 9.45 p.m.

GLASGOW LOCAL SECTION.

STREET LIGHTING BY ELECTRIC ARC LAMPS.

By H. B. MAXWELL, Associate Member.

(Paper read January 10, 1905.*)*

The lighting of streets is a most important subject to any municipality, but it is one to which, it is to be feared, very little attention is given in many towns, to judge by the results. It is by no means rare for ratepayers to have the privilege of paying a rate, by no means negligible, for the faint glimmer obtained from occasional flat-flame gas-burners, when the lighting could be increased a hundred-fold by the substitution of electric arc lamps with but a small increase in the rate.

The author fully realises that there is a number of small districts where arc lighting is unnecessary, and perhaps even undesirable, but in such places half-ampere "Nernst" lamps can effectively displace incandescent gas lamps both as regards economy and lighting. This however, is not within the scope of this paper. There are, on the other hand, numbers of towns most atrociously lighted which could largely increase their lighting at the same, or at a small increase in cost by the substitution of arc lamps.

The objects of this paper are to emphasise the fact that street arc lighting is not the expensive luxury it is generally deemed, even by many station engineers, and also to form the basis of a discussion which cannot but be of benefit.

If the requirements of effective street lighting are carefully studied, it will be found that what is wanted is, not an illuminant that most affects the eye, causing the pupil to contract so that other objects are indistinct, but one that illuminates the street and surroundings without this dazzling effect, and also one that has a maximum candle-power at an angle below the horizontal. For this reason arc lamps properly placed are infinitely superior to incandescent gas lamps which, owing to the concentration and intensity of their light, the necessity for placing them comparatively low down, and owing to their maximum candle-power being at an angle above the horizontal, are one of the worst forms of lighting in this respect. It can undoubtedly be overcome to a large extent by placing the lamps much higher, but it is then necessary to increase the candle-power by grouping some three or four mantles together at a great increase in cost, in order to avoid the

shadow cast by the pole in this form of lighting, and to provide sufficient candle-power to efficiently light the street at these heights.

An example of this form of lighting may be seen in Victoria Street, London, in the city of Westminster, and the reports of tests of the various forms of lighting in that city, carried out by Mr. Bradley, the city engineer, will doubtless be well known to the members of this local section. A copy of one of these reports, taken from the *Electrical Times* for March 10, 1904, is appended to this paper through the courtesy of Mr. Bradley, and from this it will be seen that the average cost per candle-power per annum is the lowest in the case of the arc lamps supplied by the Westminster Company even at the high rate of £22 per lamp per annum.

In certain streets in Partick as many as six 3-feet incandescent gas burners have been displaced by each 12½-ampere arc lamps, with the arc 25 feet from the road level. With the gas lamps there was undoubtedly plenty of light, but objects on the road between lamps at a distance of 100 or 150 yards from the observer were indistinct, and in some cases quite invisible, whereas with the arc lamps similar objects were quite clear when 300 yards distant from the observer in straight streets. Surely this is the form of illuminant required for street lighting for both police and traffic purposes.

The chief reason that arc lighting has not been more extensively used for street lighting is that station engineers do not charge low enough rates to make it commercially practicable, probably because they do not realise that street lighting has a load-factor of over 42 per cent. Another reason is that, when a station engineer gets a street or two to light, his main object seems to be to cram as many lamps as possible into the space available, so as to increase the revenue, with no considerations of economy or of the most suitable positions of the lamps with a view to further extensions.

If arc lighting is adopted the following points should first be considered :—

(1) Size and type of lamp to be used.
(2) Height of lamp from ground.
(3) Distance between lamps.
(4) Position of lamps.
(5) Whether trimming shall be with lowering gear or ladders.
(6) Whether circuits should be connected to distributors or not.
(7) Switchgear, etc., in the poles.
(8) Charge to be made per lamp per annum.

1. *Size and type of lamp.*—The first point to be considered is, whether open or enclosed arc lamps should be used. The open type is much superior for street lighting, as it is much cheaper to maintain and the colour of the light is very much better. Only five enclosed lamps can be run in series on 480 volts, as against ten open, if allowance is made for the resistance of cables, etc., and a 6-ampere enclosed lamp does not give as much light as a 10-ampere open. Enclosed lamps also do not run well in series.

A number of towns are now adopting smaller arc lamps for lighting their side streets, but the side streets require nearly as much light as the main streets in the majority of towns, and the decreased illumination can be sufficiently effected by keeping the lamps a little further apart. The advantages of having all lamps the same size are that all lamps are interchangeable, and only one stock of carbons and stores need be kept. The author is of opinion that the 10-ampere open type arc lamp, pendant from a swan neck or bracket, in all streets will be found most satisfactory.

Globes should undoubtedly be plain opalescent of such a density that the carbons are quite visible during the daytime. If the density is greater, too much light is cut off, and, if less, the light is not sufficiently diffused. As an example of other types of globes, reference need only be made to the Muranese globes on the arc lamps supplied by the St. James' and Pall Mall Co., mentioned in Mr. Bradley's report, from which it will be seen that these globes cut off about 25 per cent. more light than the opalescent globes. Most of these globes have been put in Pall Mall and Waterloo Place, and they have a very dingy appearance.

In choosing the make of lamp to be used it should be remembered that the cost of the lamp is only a small proportion of the total capital expenditure, and that, with a good lamp, not only will the cost of maintenance be less, but failures will be reduced to a minimum, a point of first importance.

2. *Height.*—The height of a 10-ampere arc from the ground level should be 20 feet, and it will be found that at this height the street is effectively lit, and the light is projected to considerable distances.

3. *Distance.*—Except in streets with very fast and heavy traffic the distance apart is not of such importance as that lamps should invariably be placed at street corners so as to get the maximum efficiency from the lamps. In main streets, where there are tramways, the lamps may be placed from 60 to 80 yards apart, to suit the corners; in less frequented streets this may be increased to 90 yards; and in side streets to 100 yards, or even more in special cases.

4. *Position.*—In addition to placing lamps at the street corners, they should be placed on alternate sides of the road, except on curves, when they should be placed on the outside of the curve. By placing lamps at the corners one lamp will shine in several directions, and also it is at the street corners that light is most important for traffic purposes. A street lit with lamps staggered on alternate sides of the road is better lit than with centre poles, and with less obstruction to traffic. The use of tramway poles is not desirable, as the maintenance will be higher owing to vibration and the higher ladders required, and failures will be more frequent.

5. *Lowering gear or ladders.*—The makers of lowering gear claim that this apparatus among other advantages is economical, but in the author's opinion there is no economy in using this apparatus, except, perhaps, in crowded cities, where it may be necessary to send a second man out with the ladder in order to prevent accidents. The capital

cost of the lowering gears will be infinitely greater than the cost of ladders, and the maintenance of these gears must be exceedingly costly, as so much depends on the apparatus being kept in the most reliable and safe condition. A trimmer will be able to attend to as many lamps, or even more, with a ladder as with a gear, especially if he has to examine and keep clean the safety mechanism and spring contacts. The swinging lamp offers just as much obstruction to traffic as a small ladder, and in windy weather the ladder is probably the safer, and certainly most people would prefer to have a trimmer fall on them than an arc lamp and suspension gear.

6. *Whether connected to distributors or not.*—The author is of opinion that it is most necessary to control the street arc circuits direct from the station by separate feeders. By this means the whole is under the observation and control of the station staff, who can immediately tell from the feeder ammeters whether all the circuits are burning correctly, and, if not, have it remedied immediately. Also if a section of distributor fails, it is of great benefit not to have the street lamps affected, and *vice versa.* The arrangement with separate feeders also obviates the necessity for men switching on and off at all hours of the night, which is an extremely wasteful and clumsy arrangement even when the men do not oversleep themselves. The extra capital charges, etc., in the one case are very much the same as the extra cost of wages and current in the other. The only other alternative is time switches and remote control switches, which are both unsatisfactory. In the former case a man is usually required to put the switches right when they operate at the wrong time, and frequently to break the arc formed by the opening of a switch, unless this has already been done by some enterprising fireman.

There are other advantages of the separate feeder arrangement, which will be apparent from the description of the arrangement at Partick given later.

7. *Switchgear, etc., in the poles.*—It is essential to have in the base of every pole at least an isolating switch and a substitutional resistance. The latter are very necessary, as without them the circuits are liable to be seriously overrun in the event of lamps failing owing to shortness of carbon or other causes. An automatic cut-in must, of course, be provided either in the lamp itself or in the base of the pole, in order to substitute the resistance when the lamp fails. The author prefers the latter position, as the cut-in is then more accessible, and a third wire up the pole is unnecessary.

8. *Price.*—There are various methods of charging for street lighting; in some cases the lighting department borrows capital for the lamps, etc., and even maintains them; but for the purposes of this paper it is supposed that the electricity department borrows the necessary capital and maintains the lamps, the lighting department being charged on the estimated cost. All charges, both capital and revenue, should be kept entirely separate from the electricity account except the item for current only. The author thinks that the street lighting capital account should include all cables, road-work, switchboards, etc.

The capital cost of installing 400 10-ampere arc lamps, with poles, resistances, cables, road-work, switchgear, etc., will be about £24,000, so that the annual cost per annum, including all charges, will be—

	Total.			Per Lamp.		
7 per cent. interest, sinking fund and depreciation, on £24,000	£1,680	0	0	£4	4	0
Carbons—75,000 pairs at £4 10s. ...	337	5	0	0	16	10
Wages—Five trimmers, repair hand, and mate	455	0	0	1	2	9
Repairs, painting, etc.	100	0	0	0	5	0
672,000 units at 1d. per unit	2,800	0	0	7	0	0
	£5,372	5	0	£13	8	7
			say	13	10	0

It will be observed that the charges on capital and for current form over 80 per cent. of the total cost of the lamp if 1d. per unit is charged for current. It is therefore to these two items we must look for any large reduction in price, although it is essential that the other items should be kept down if cheap arc lighting is required.

The 7 per cent. allowed for interest, sinking fund, and depreciation will be found more than sufficient, as the greater part of the capital outlay is for mains, feeders, poles, road-work, and labour, which depreciate very little. If the money is borrowed at $3\frac{1}{4}$ or $3\frac{1}{2}$ per cent., the remaining $3\frac{1}{2}$ or $3\frac{3}{4}$ per cent. for sinking fund and depreciation is sufficient to repay the loan in less than twenty years.

The figure given for carbons can easily be obtained if short ends are saved and used in the spring and autumn. It is advisable to trim full during the four summer months, as then half the trimmers can be put on other work. In Partick a bonus system is used with the trimmers, which will be explained later. The figure given in the estimate can only be attained by using the very best carbons, as, if an inferior carbon is used, the carbon will not last so long, and will leave a lot of ash, which causes dirty globes; the light also will be defective.

The amount allowed for wages will be found sufficient in the majority of towns. Five trimmers have been allowed for, being eighty lamps per man; but in a number of towns, where the height of the lamps does not exceed twenty feet, each trimmer can attend to a hundred lamps, making fifty per day in winter. If this were done, this item could be still further reduced. If lamps are placed higher, as when the tramway poles are employed, it is usually necessary to have a second man to help with the ladder, and stand by it when it is in use. In Partick it has been found that one trimmer can attend to ninety lamps, even with extra difficulty of trimming incurred by the use of Hyde Park carriers and telescope ladders, these latter being necessary owing to the extra height of the lamps. Every £50 increase in the total wages bill will add 2s. 6d. to the cost per lamp per annum.

The repair man's mate should be trained for a trimmer, so that he may take any vacancy caused by a man leaving or being away ill.

The question of current is the most important. The number of

units allowed in the estimate is based on 3,500 lighting hours per year, which is slightly in excess of the actual lighting hours when the circuits are switched on and off from the station.

The only items that should be included in the charge for current for street lighting are the works costs *exclusive* of wages (which should not be increased at all by the addition of a street lighting load), and interest, sinking fund, and depreciation on the capital cost for boiler, generator, and condenser plant. The cost of all cables, feeders, and switchboards has already been debited against the capital cost of the street lighting, so that £40 per killowatt demanded, even after providing for a small amount of spare plant, will be more than sufficient to cover the increased cost of station plant due to the street lighting. Eight per cent. interest, etc., on this sum will be a charge of £3 4s. per kilowatt per annum, which, at 3,500 units per kilowatt per annum, works out at 0·22d. per unit. Any other items, such as insurance or office costs that may be increased by the addition of street lighting, will be amply provided for by the 0·03d., so that a station can supply for street lighting at the following rate: Works costs — wages + 0·25d.

If considered necessary a small addition to this price may be added for profit (every 0·1d. adding £280 to the revenue from four hundred lamps), but in the author's opinion no such increase is necessary, as the decreased cost per unit of coal, stores, and repairs (especially the last item), owing to the much better load-factor, will in itself cause a considerable profit. Thus, if the increased load-factor reduces these costs by only a farthing, a profit of £700 will accrue from the 672,000 street lighting units, in addition to that realised from the whole units generated from the station. It must be remembered that the above method of making up the charge is based on the works costs previous to the adoption of an extensive street lighting scheme, and that the reduction in the works costs will not only affect the units for street lighting, but the whole supply from the station.

There are fifty-four stations in the *Electrical Times* records, in which the cost per unit for street lighting, if worked out on the basis given above, would be from 0·57d. to 1d. per unit, and which could therefore supply four hundred 10-ampere street lamps at prices ranging from £10 18s. to £13 10s. per lamp per annum (everything included), and yet the town with the lowest rate has no street lighting at all.

In nearly all these fifty-four cases the charge actually made per unit is much in excess of this figure (in some cases being four times as great), and consequently only a small amount of street lighting can be obtained. Of those that also have a traction supply the charge for this is invariably much lower than that for street lighting, although the latter has a better load-factor, requires no special plant, and, being a constant steady load, is not so severe on the plant. Moreover, in the price for traction supply is usually included capital and maintenance charges on feeders and switchboards.

Annexed to this paper (Appendix I.) is a list of a few of these towns which amply demonstrate this point. This list has been compiled from the *Electrical Times* records for August 18, 1904.

Quite a number of the fifty-four towns are large cities, where arc lamps can be employed to the best advantage, and where a powerful light is essential, and some of them have nothing but ordinary flat-flame gas-burners which do nothing more than make the darkness visible.

From information received from about fifty towns the average cost per lamp per annum of incandescent gas lamps, including gas, repairs and maintenance, wages, uniforms, etc., but providing nothing for capital charges or for buying new lamps out of revenue, is—

Single 3½ to 4 foot burners	... ...	£2	10	0
Two ,, ,,	... ...	4	10	0
Three ,, ,,	... ...	6	15	0

The candle-power of these on the street after fifty hours' use will respectively be 40, 70, and 100 candle-power.

There is not the slightest doubt that a very large number of towns can light their streets with arc lamps as cheaply as with incandescent gas, and with a large increase in light, provided the arc lamps are judiciously placed and the whole scheme has been carefully designed.

Again, referring to Mr. Bradley's report (Appendix II.), the average candle-power of a 10-ampere arc lamp with opalescent globe is 637, which, at £13 10s. per lamp per annum, works out at 5d. per candle-power per annum, a figure which could not be equalled by any other form of lighting, the average obtained from ordinary incandescent gas lighting being 1s. even when taking the nominal candle-power of the mantle.

The formula given for obtaining the price for street lighting, or any supply of as good a load-factor, leaves ample margin, and the author would be only too pleased to take on as many 10-ampere street lamps as he could get at £13 10s. per lamp per annum. There is not the slightest doubt that in a year's time, when condensing plant and efficient machinery have been in operation for a year with the present street lighting load, the cost figures at Partick will speak for themselves, and will compare favourably with stations with a traction supply.

Station engineers will, of course, take better prices if they can get them, but if street lighting cannot be obtained at a better rate, it will undoubtedly pay to supply at the rate given rather than lose a load of such an excellent load-factor. In towns where the electricity supply is in the hands of a company the municipality should borrow the necessary capital and maintain the lamps themselves, buying only the current, as a company is sure to protect itself by making a liberal estimate of the other items, bringing up the total cost of the lamp, while nominally selling current at a low rate.

A description of the arrangements made in Partick may be of interest.

The lamps adopted are all of the 12½-ampere open type, run ten in series on the outers of the three-wire system at 480 volts, and number in all 290, which number is shortly to be increased to 300. The 12½-ampere size of lamp was found necessary owing to the excessive height

of the tramway poles, which in the original scheme were utilised to a large extent, the lamps being placed in Hyde Park carriers, with the arc some 26 feet from the roadway. The author was not responsible for this arrangement, but is of opinion that it has some advantages with the disadvantage that they are more costly to trim and maintain.

In extending it has been thought of greater importance to keep the whole scheme uniform instead of changing to 10-ampere lamps at a lower level and at shorter distance. The distance at which the lamps at present in use can be placed, while at the same time maintaining a good light, is surprising. In several streets the lamps are placed as far apart as 180 yards, and the result is excellent; and the author will be pleased at any time to show examples of this to any one who may be interested.

If in any circuit the lamps are particularly far apart, the circuit is overrun at 13 amperes, which gives a considerable increase in candle-power, or if in others they are close together, the circuit is run at 12 amperes. This method of regulating the circuits is not generally made use of, but it is very effective, and it has been found that the extra current in no way hurts the lamps, that no special adjustment is necessary for only ½-ampere variation from the normal, and that even at 13 amperes the carbons last for more than two days even in the middle of winter.

The arc circuits are entirely run with 7-16 twin lead-covered wires laid solid, the two conductors being insulated with different coloured papers, so that they may be distinguished. The cables are looped in and out of each pole, cut and all the ends sealed in one sealing chamber. Two wires of one colour are connected into the isolating switch, the two of the other colour being joined together by a detachable connector. By this means every length of cable between poles can be tested separately.

Contiguous lamps are connected alternately on the red and white conductors, and where the ends of two circuits of ten lamps are close to each other the two circuits are connected in series, thus forming two separate single-wire circuits, with contiguous lamps on different circuits. The circuits have, as far as possible, been run in this manner, so that when a fuse blows only every alternate lamp is out till it has been replaced.

The circuits are supplied from feeder pillars which each control from four to six circuits, in which the fuses and line resistances are placed, these being supplied from the station by independent feeders and from separate bus-bars. The street lighting is started on separate machines from the ordinary supply, and afterwards paralleled on to the main bars, thus obviating any irregularity in the ordinary lighting due to switching on several circuits at once. As the machines are separated out again at dawn and shut down on the load, the feeder switches are only operated in the case of emergency, which is a great saving to the switches, as the arc formed when opening arc circuits at 500 volts is excessive owing to induction. Also if at any time an arc circuit or feeder is earthed so that a fuse will not hold, all that is necessary is to open the paralleling switch and run with one pole

earthed till the fault is remedied. If the earth is on a circuit it can then be easily isolated by testing at each lamp to earth with a voltmeter. This arrangement is very useful, as a negative carbon dropped through is sufficient to cause an earth on arc circuits in Hyde Park carriers.

With regard to maintenance, a trimmer, however expert and economical he may be, is useless unless he takes the trouble to keep his lamps and globes scrupulously clean. This is the whole secret of successful arc lighting as regards illumination. In Partick the usual method of working, namely, to give a trimmer so many lamps to attend to and leave the rest to him, has been found quite impracticable, and a special method has been adopted. Each trimmer's lamps are divided into four equal sections, say A, B, C, and D. On Saturday, Sunday, and Monday he only trims and does no cleaning. Saturday and Sunday are short days during the months Sunday trimming is necessary, and on Monday the spare time is occupied in sorting short ends into the different lengths, cleaning and oiling ladders, etc.

On Tuesday he trims A and B and cleans A.
On Wednesday he trims C and D and cleans C.
On Thursday he trims A and B and cleans B.
On Friday he trims C and D and cleans D.

By this means each lamp is thoroughly cleaned once a week, and a glance at the week's orders and key plan tells at once where the trimmers are working should it be necessary to put current on a circuit for testing purposes.

A bonus system has been adopted with the trimmers in the following manner. The carbons, wages, and repairs on each trimmer's lamps are booked against him, and the man with lowest costs per lamp per annum is awarded a substantial bonus, but a trimmer's costs per lamp are penalised by 6d. every time one of his lamps is reported out, unless such extinction is proved to be beyond his control, and 6d. for every dirty globe reported. Each trimmer is also allowed a certain percentage of new globes per half-year. If he exceeds this number a proportion of the cost is deducted from his wages for each extra globe; if he uses less, the same proportion is given him as a bonus. It is perhaps needless to add that all the trimmers so far have kept well inside their percentages.

The ordinary routine repairs include the taking down and overhauling of every lamp once each year. If the lamps have been kept in good condition by the trimmers, it usually is only necessary to clean the internal mechanism. The repair hand and his mate can easily take down forty lamps in three weeks during the summer months, replacing them with overhauled and tested lamps, clean and overhaul them and connect them up for test, a volt and ampere chart being taken off each lamp. The final adjustments are made by an assistant, if required, so that the repair hand requires no technical knowledge. In addition, any lamps reported out or burning badly have to be attended to. It has been found that the one repair hand and mate can easily attend to three hundred lamps, and also find time for a considerable amount of other work

APPENDIX I.

LIST OF A FEW TOWNS, WITH PRICES CHARGED FOR STREET LIGHTING AND TRACTION.

Number.	Price received for Traction Supply.	Price received for Public Lighting.	Correct Price for Public Lighting Works costs – Wages + 0·25d.
1	...	2·30d.	0·85d.
2	...	3·00	0·93
3	1·40	2·50	0·98
4	1·50	2·54	0·94
5	1·50	2·17	0·83
6	1·82	4·16	1·56
7	1·25	3·32	0·71
8	1·19	2·50	0·82
9	1·75	3·00	0·86
10	1·50	2·18	0·90
11	1·67	2·98	1·52
12	...	1·58	0·75
13	...	2·62	0·91
14	...	3·00	0·85
15	...	3·12	0·97
16	1·73	2·92	0·95
17	1·86	2·44	0·95
18	1·50	3·96	0·95
19	1·40	2·18	0·83
20	0·50	2·38	0·76
21	1·53	3·14	0·97
22	1·69	3·00	1·00
23	...	1·98	0·71
24	...	1.50	0·78
25	1·10	2·00	0·78
26	1·65	2·00	1·12
27	1·39	2·70	0·69
28	0·90	2·85	0·67
29	1·25	2·26	0·82
30	1·47	None	0·89
31	1·50	None	0·57
32	...	None	0·92
33	...	4·04	0·98
34	...	4·95	0·93
35	...	3·03	0·85
36	1·84	2·97	1·14
37	1·92	3·24	1·60
38	...	3·07	0·96
39	1·96	3·03	1·05
40	...	3·56	1·35
41	1·91	3·07	1·08
Average.	1·50	2·82	0·92

APPENDIX II.

Reprint from the "Electrical Times," March 10, 1904.

MR. BRADLEY'S REPORT ON THE WESTMINSTER STREET LIGHTING.

CITY OF WESTMINSTER.—CITY ENGINEER'S DEPARTMENT: TESTING BRANCH.

Tests of Street Lamps (Gas and Electric), with Statement of Comparative Costs extending over a period of 18 months.

Description and Position of Lamps.	No. of Lamps in City, of Class specified.	Average Candle Power.	Total Cost per Lamp per annum. £	s.	d.	Total Cost per Candle Power per hour. d.	Test No. 6. Total Cost per Candle Power per annum. d.	Average Total Cost per Candle Power per annum, including all tests up to date (6 Series). d.	Total No. of Tests up to date.
(1) Electric arcs (Charing Cross and Strand Electric Light Co.). Opalescent globes	100	670	30	0	0	·00273	10·75	**11·49**	40
(2) Electric arcs (St. James' and Pall Mall Electric Light Co.). Muranese globes	60	474	34	0	0	·00445	17·23	**15·1**	43
(3) Electric arcs (Westminster Electric Supply Corporation). Opalescent globes	945	605	22	0	0	·00222	8·73	**8·7**	35
(4) Refuge lanterns, four mantles, Victoria Street	12	113	13	6	6	·0072	28·3	**30·7***	24
(5) Sugg's high-pressure lamps, Parliament Street	29	573	18	5	0	·00194	7·65	**9·85**	35
(6) Incandescent mantles, Victoria Street type	1,241	42	3	10	0	·00508	20·0	**18·42**	40
(7) Triple flat-flame, footway, Whitehall, now in process of removal, Scott-Snell lamps being substituted	35	51	6	1	2	·0076	29·9	**25·65**	33
(8) Incandescent mantles, Carlton House Terrace type	251	38	2	17	11	·00465	18·3	**18·18***	27
(9) Triple flat-flame, Strand type...	508	47	9	8	10	·0122	48·2	**47·44***	30

* Average of five series only.

DISCUSSION.

Mr. Newington.

Mr. FRANK NEWINGTON: At the commencement of the paper the author compares arc lamps with incandescent gas lamps, proving that the latter are totally unfitted for street lighting. I am not prepared to go as far as that myself. For main streets the arc lamp is, without doubt, the only thing to use, but for smaller streets we have at present no electric lamp that will compete favourably with the incandescent gas lamp. It is to be hoped, however, that soon an electric lamp will be got to beat the incandescent gas lamp. The author states that in Partick he has replaced as many as six incandescent gas burners by one arc lamp. The incandescent gas lights must have been placed very close together, or the arc lamps very far apart. In Edinburgh, one arc lamp has displaced three and a half gas lamps. Mr. Maxwell talks of 150 and 300 yards as the distances for arc lamps, but that seems to me an enormous distance. Sixty to seventy yards is a fair average. With regard to the size and type of lamp to be used, I certainly think that the open arc lamp is much better than the enclosed. With the enclosed lamp the colour is not satisfactory, and they do not burn steadily. A great deal depends on the density of the globe. In making some tests I have found that while the opalescent globes do good at certain angles, on the whole there is a loss of about 50 per cent. In Edinburgh an experiment has been tried by shading off the opalescence from two or three inches of the top, gradually merging into clear glass, and I think it may do good, but we have not gone far enough to know exactly. It certainly gives more light at a distance of from forty to fifty yards, but I do not yet know the price of a globe of that sort. I am in agreement with Mr. Maxwell about the lowering gear. Ladders are certainly clumsy and awkward, and when used in Edinburgh, which is very hilly, two men are required, which, of course, adds very much to the expense. The lowering gear would not answer in a windy place, as it would, no doubt, mean a very great breakage of globes against the post in lowering or raising the lamp. As to whether circuits should be connected to distributors or not, I cannot see the use of separate feeders, distributors, or switchboards, as that adds enormously to the cost. Apparently the capital cost of the lamps in Partick is £60 per lamp, and the average should not be more than £30 per lamp. The author says: "By this means the whole is under the observation of the station staff, who can immediately tell from the ammeters whether all the circuits are burning correctly, and if not have it remedied immediately." Does he mean that if one arc went out he would open a feeder switch at the station and switch off fifty others? If that is so, it does not seem to me to be quite worth while. There is no necessity to have a switch on each lamp. As to the cost per lamp, I should like to know if the figures given are actual or estimated. If they are the actual figures for wages and repairs, they are very low, and I congratulate the author upon them.

Mr. Maxwell has allowed 7 per cent. for interest, sinking fund, and depreciation on £24,000, amounting to £4 4s. per lamp. I do not

Mr. Newington.

know what is there meant by depreciation. If the lamps are kept and properly maintained out of revenue, and the capital outlay is paid off after the usual term of twenty-five to thirty years, I do not see the need of a depreciation fund. I think £4 4s. is too high. Taking the interest on the money borrowed at 3½ per cent., the amount of interest gradually decreases each year as the principle decreases, and for a period of thirty years the average interest works out at 1·8 per cent. instead of 3½, so that £3 or £3 4s. is quite enough to allow for the three items interest, sinking fund, and depreciation. The price of carbons mentioned in the paper is higher than that paid in Edinburgh, where we can get satisfactory carbons for £3 10s. per thousand pairs. In Edinburgh a considerable saving has been effected in carbons by using different lengths at different times of the year, according to the length of time the lamps burn. As to wages, it is said that in Partick only one man was necessary with a ladder. I should like to know the kind of ladder used, as in Edinburgh we have been unable to get one that one man can handle. The figure of 5s. per lamp for repairs may be sufficient for the first year or so, but I am certain that after five or six years, the lamps would cost very much more to maintain. In Edinburgh the figure is 18s. per lamp with one thousand lamps in use. As to the cost of electricity, the author thinks that wages should not be included. On this point I disagree with the author, so far as wages in the station are concerned; the arc lamps are switched on just about the time of maximum load, and therefore more boilers and engines are required, and so more men. It is incorrect to say that nothing is needed for repairs of machinery at the generating station. These must be in proportion to the output and the number of hours each machine or boiler is in use. The costs for arc lighting in Edinburgh are as follows:

	£	s.	d.
Generating costs, including proportion of salary, full allowance for wages, and maintenance of plant	2	19	6
Trimmers' wages	2	11	6
Carbons	0	13	8
Maintenance of lamps	0	18	0
Maintenance of mains and switches (estimated figure)	0	10	6
Total working costs	7	13	2
Interest on sinking fund on lamps, lamp posts, mains, and necessary generating plant and buildings ...	2	11	3
Rents, rates, taxes, and proportion of management expenses	0	11	8
Making a total per lamp per annum of ...	£10	16	3

Arc lighting at, say £11 or £12 per lamp, is by no means a gold mine; and there is nothing to be made out of it. The price at Partick is not stated in the paper, but I gather it is £12 to £13, which is an exceedingly low figure for the first year or two of any undertaking.

Professor Jamieson.

Professor ANDREW JAMIESON: When the author of this paper arrives at the practical details, he gives us useful information from his own experience, with which, for the most part, I have pleasure in agreeing. But I do think that 100 yards is the maximum distance apart which 12½-ampere arc lamps should be placed from each other even in straight open wide streets; and I do not think such lamps suitable for narrow back streets or slums.

Regarding the author's allowance of only 7 per cent. for interest, sinking fund, and depreciation—and taking into account the particularly expensive system which he has adopted of using a large number of underground feeder cables from the Central Generating Station to his lamp centres—I am distinctly of opinion that 7 per cent. per annum on the borrowed capital will simply pay the 3½ per cent. interest and 3½ sinking fund to pay off the capital in 20 years. Consequently, at least another 3 per cent. will be required for a general reserve, renewals, and depreciation fund. Or, a total allowance of 10 per cent. per annum on the original sum should be allowed for in the first estimate to cover all these items, and place the ratepayers in a comfortable and secure position. At the very commencement of the Glasgow Electric Lighting Scheme, I carefully worked out these particulars with the late Mr. William Foulis, M.Inst.C.E., and we found that the total annual cost to the city per 10-ampere arc lamp came to £15 15s. Of course, now that such large installations of arc lamps are in operation in big cities like Edinburgh and Glasgow, the annual total cost per lamp is considerably reduced. But I am certain, that if Mr. Maxwell remains in the smaller borough of Partick for the next twenty years, he will then feel a great deal happier and manage his committee much more easily, if he had got them in the first instance to charge £15 15s. per lamp per annum and put aside 10 per cent. for paying off the original borrowed capital, etc. No kind of arc lamp and no kind of underground cable will—as far as my experience goes—last twenty years, even with the greatest care and repair. I have now had fully thirty years' experience of underground cables, and their average existence does not exceed fifteen years; whilst I cannot point to a single arc lamp which has been working at any of our central or railway stations for over that time—since fashions change and well recognised improvements have sent many otherwise fairly good arc lamps to the scrap-heap.

Mr. Maxwell states that "The only items that should be included in the charge for current for street lighting are the works costs *exclusive* of wages" . . . and further on, "so that a station can supply street lighting at the following rate: Works cost — wages + 0·25d." What was that minus for? It will never do to subtract the due proportion of wages spent for arc lighting. The algebraical sign should have been a plus +.

I observe that Mr. Maxwell has reckoned the candle-power value of a 10-ampere arc lamp with an opalescent globe as 637 candles. This value agrees very closely with the mean result of many tests I made upon a number of 10-ampere lamps, which came to 625 candles with similar globes. The 12½-ampere lamps shown to me the other day by Mr. Maxwell are excellent both in mechanism and construction, and as

Professor Jamieson.

an old locomotive man I would advise him to adhere to them throughout his district, for there is a great comfort and saving of time and labour in having one set of lamps with interchangeable parts and carbons. In conclusion, I have pleasure in congratulating the author upon his present results, and the careful, systematic manner in which he has hitherto managed the street lighting of Partick.

Mr. Hanson.

Mr. A. C. HANSON: In the first place, I do not agree with Mr. Maxwell as to the method in smaller streets of placing the lamps at a much greater distance. That might be very well in some cases, but in other cases, where there are cross streets, it is necessary to have a lamp at the corner of the street, and to enable that to be done, it is often a good thing to have the smaller lamp. I am, myself, running several smaller lamps with great success. The light from these is exceedingly good, and they give no trouble. As to globes, at present undoubtedly the opalescent globes are the best, and as to having the best lamp, I am in hearty agreement with the author. It is a great trouble to have a lamp that is cheap in the first cost, but soon goes wrong and requires repair. For a 10-ampere lamp about 20 feet is the proper height. No doubt the 12½-ampere lamp at 25 feet is quite as satisfactory, and there is a tendency nowadays to go in for lamps of large candle-power. Mr. Maxwell says: "A street lit with lamps staggered on alternate sides of the road is better lit than with centre-poles." Generally speaking, that is true, but in certain wide streets I think it is an exceedingly good thing to have centre poles. In Edinburgh centre-poles are used with very good effect. Regarding the lowering gear or ladders, the author may possibly be right as to ladders as opposed to lowering gear, but at the same time there is a great deal to be said for this gear in a hilly town. In Stirling two men have to be employed to look after the ladder, as it is quite impossible for one man to take it up the hills, and that adds very considerably to the cost. In very windy weather it has been blown over two or three times. As to whether circuits should be connected to distributors or not, it is a very good system in many ways to have separate feeders and distributors for the arc lamps, but it is a very costly one. In Partick I suppose all lamps are run throughout the night, but in a good many towns half the lamps are switched off about eleven or twelve o'clock. The author must bear in mind that the *Electrical Times* figures certainly in some, and I believe in very nearly all cases, included the capital charges. On some of the figures this would mean a decrease of about 0·6d. per unit. As to including the wages in the price, I do not agree with that at all. If he does this, there seems to be no reason for putting the wages on any new customers. Arc lamps have to be provided for in the same way as other consumers that overlap the peak. The author seems to be treating the public lighting rather better than he should, possibly for the purpose of bringing out a figure like 1d. per unit. There would be a good deal less gas used in towns if it could be done for 1d. per unit, but there are very few towns that could do it at that.

Mr. Burbidge.

Mr. A. H. BURBIDGE: It seems to me that if we take the author's figures the costs are improved very considerably if double carbon lamps are adopted. I have seen lamps that were in use for a year and never

Mr. Burbidge. had to be taken down. If that is the case with other lamps, there seems to be no objection to the double carbon lamps now. The immediate gain is that the extra cost consequent is more than saved by the decrease in the number of trimmers required. Regarding lowering gear, the gear we have in Kilmarnock is working very satisfactorily indeed. During the last three months, in spite of a good deal of windy weather, we have had no globes broken. The only trouble is that the gear sometimes sticks, which necessitates the ladder being taken out. One man can trim 50 lamps comfortably in five hours, and I do not think that one man with a ladder can trim the same number much under a day. In addition to trimming the lamps every morning, each globe is cleaned when it comes down. I agree with the last speaker about the difficulty of having separate feeders and distributors for the arc lamps. In many towns it would necessitate carrying new feeders back to the station, which is quite out of the question. I have tried lamps 100 yards apart, and can see absolutely no difference at that distance as compared with 70 or 80 yards; the general effect was just as good at either. Big lamps are just as necessary in side streets, but the existing gas lights in side streets at present are generally on a very much smaller scale. The result is that as soon as an estimate is placed before a committee it is thrown out owing to the exceedingly high cost of arc lighting.

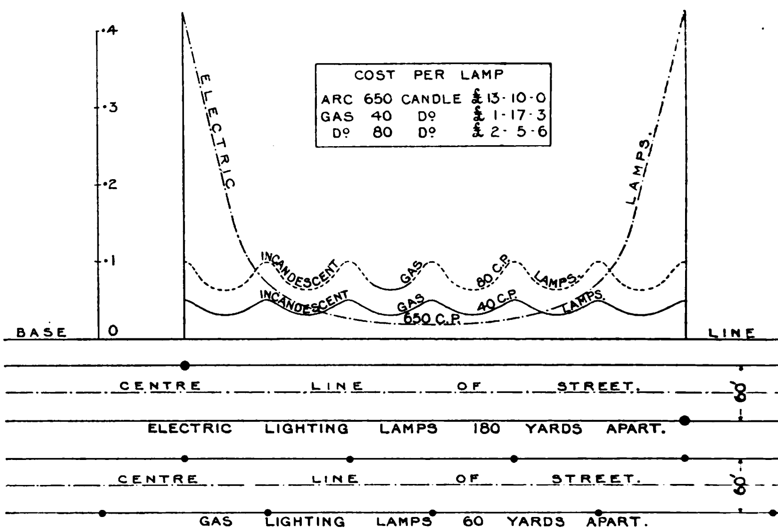
Mr. Robertson. Mr. J. A. ROBERTSON: I do not think that the author's comparison is quite fair to the enclosed arc lamp. Four or five years ago, several attempts were made at street lighting by enclosed arc lamps which were not altogether successful, but since that time they have been much improved. I would not go so far as to follow the American practice of erecting enclosed lamps in the main streets of a town, but for side-street lighting the enclosed lamp is often to be preferred, and, owing to the better grouping, may sometimes work out cheaper. What we are all waiting for to light our side streets is a small lamp using about 100 to 150 watts which will be as efficient as the 10-ampere open type.

With regard to incandescent gas lighting, I do not agree with the author that in all cases this type of lamp cannot be substituted by arc lamps. I was asked a short time ago to get out a scheme for the electric lighting of some side streets at Greenock which had previously been lighted with incandescent gas at a cost of £2 7s. 6d. per lamp per annum, the burners used being No. 3 Kern type. From experiments made on the street, I concluded that for efficient lighting, Nernst lamps using 125 watts would be required to substitute each incandescent gas burner. Making allowance for all charges, I could not see that it was possible to erect the lamps, supply them with current, and maintain burners, for less than £3 per lamp per annum; the result is that the incandescent gas lamps are still used, and we have turned our attention to a more remunerative class of business. I do not think the author's figure of 150 yards between lamps is practicable in side streets—at least it would not be practicable in the side streets we have in Greenock. If the streets are straight and wide, it might be possible in some instances to space them this distance; but where the side streets are crooked and narrow, it would be quite out of the question to put lamps at more than 70 or 80 yards apart. The most contentious point in Mr. Maxwell's

paper is that relating to the charge to be made for current. I do not agree with him at all in omitting the works wages when calculating the cost of current for the lamps, and can see no reason why he should debit his other consumers with this item. He may tell us that the addition of a street lighting load does not increase his wages bill, but the same thing might equally well be said of every new consumer who comes on the mains after the station has been opened. As a matter of fact, I question very much if the street lighting load does not add materially to the wages bill in a station like Partick. Mr. Maxwell has, I understand, a storage battery at Partick, and works his men in eight-hour shifts. Now I think he will agree that if there were no public lighting load, it would be quite possible to dispense with one of the shifts of men and allow the battery to take the load from, say, midnight to 6 a.m., so that his public lighting is directly responsible for one-third of his works wages. The author states that 0·03d. per unit is sufficient to cover all charges for management, insurance, etc.; but there is one item which he omits entirely, namely, rent, rates, and taxes. At Partick he is paying, I notice, 0·07d. per unit sold under this heading, and in fairness this should be added, as there will be no question, I think, that the public lighting load should pay its proportion of taxes, along with other consumers. My opinion is that street lighting should be treated exactly as we would treat any other customer requiring the energy under similar conditions. Interest and sinking fund charges should be calculated on the capital cost of the street lamps and cables, as well as on the plant which has to be reserved for supplying the maximum demand for street lighting at any time during the year. Provided that the station is purely a lighting station, the whole of the works costs should be included. The proportion of the management charges incurred by the meter and consumers department should be subtracted, rents, rates, and taxes added in proportion to the capital employed, and the sum of these various items, when divided by the number of units supplied, will give the cost per unit to the undertaking. Mr. Robertson.

Mr. A. WILSON (Glasgow): The low estimation in which the electric arc lamp, for street lighting, is now held by almost all public authorities is a sufficient reason for dealing with the subject, but I am afraid better arguments than any Mr. Maxwell has stated will be required to bring arc lighting into favour again. At the beginning of the paper, Mr. Maxwell tilts at the old wind-mill of flat-flame gas burners. This was a favourite pastime of all young electrical engineers, but, like the flat-flame burners themselves, this comparison was now quite out of date. In the next paragraph, it is stated that half-ampere Nernst lamps can effectively displace incandescent gas lamps, both as regards economy and lighting—a wild statement. It is admitted, even by electrical engineers, that Nernst lamps are, from their construction, quite unsuited for street lighting, without taking into consideration at all the fact that, light for light, the cost is quite three to four times that of the most ordinary form of incandescent gas lighting. Mr. Maxwell then states the requirements for effective street lighting, which are fairly right, but one most important condition has been entirely omitted, no doubt because with arc lamps it is impossible to fulfil it. I refer to Mr. Wilson.

Mr. Wilson. the necessity of having the illumination as uniform as possible over the whole street surface. The true criterion of effective street lighting is the amount of light supplied to the darkest part of the street; alternate zones of light and darkness are most objectionable, and only serve to intensify the want of light in the dark places. Mr. Maxwell then condemned the concentration and intensity of incandescent gas lamps. This, from an electrical engineer, recommending arc lamps, is surely a case of the pot calling the kettle names. The conclusions given in the paper as to the relative values of the two systems with regard to the direction of the rays of light are not correct, but do not count for much, as, without a diffusing globe, arc lamps are quite useless for street lighting, and a good reflector with the incandescent gas lamps easily directs any stray rays in the direction most required. It was said



that in Partick as many as six three feet incandescent gas burners have been displaced by each electric arc lamp. Here I would thank Mr. Maxwell for his testimonial to the incandescent gas lamps. Mr. Maxwell says that with them there was undoubtedly plenty of light, but it was stated that objects at a distance between 100 or 150 yards were indistinct. What kind of lighting could be expected from lamps two to three hundred yards apart? When the author deals with the financial side of the matter, I must express dissent. When these one-sided and altogether wrong views as to costs were put before councillors and others without having the chance of weighing the true facts of the case, one can quite understand why there is such an amount of arc lighting in Partick. The author states that in several streets the arc lamps are placed as far apart as 180 yards, and the result is excellent. The result referred to must be the cost, and not the effective lighting of the streets. I have examined many examples of what

are claimed to be good systems of street lighting, as well as this at Partick, and I must confess that I have only seen one place more inefficiently lighted, and that is in the side streets of Brighton, a town, by the way, where the corporation also owns the electricity undertaking. The author takes the cost of the arc lamps at the low, and in my opinion, unfair figure given for Partick, while he is content to take the average of about 50 towns for the cost of the incandescent gas lamp instead of the local figures. The well-known law of inverse squares for calculating lighting values could scarcely be ignored in a comparison of lights such as this. Taking the figures given in the paper for the arc lamp, say 650 candles, and for the incandescent gas say 40 candles, also that one arc displaces six gas lamps, it will be seen from the diagram submitted that the illumination supplied by the arc lamps is concentrated near the lamps, leaving the middle distance in comparative darkness; while with the incandescent gas the illumination is fairly equal throughout. Also, it shows that for this middle distance, which is the true test of good lighting, when Mr. Maxwell displaced the six incandescent gas lamps and put in one arc lamp in their place, he reduced the effective illumination by more than one half. The cost of the incandescent gas lamps using three feet per hour for, say, 3,600 hours, with Glasgow gas is— Mr Wilson

		s.	d.	
Gas		22	6	
Renewals		1	9	
Wages		13	0	
Total		37	3	per lamp
			6	
or	£11	3	6	for the six lamps.

For 80-candle incandescent gas the cost per year in Glasgow is £2 5s. 6d. for each lamp. The price of the gas taken is its true commercial value, and the wages include cost of uniform, holidays, etc.

Mr. W. W. LACKIE: I entirely disagree with Mr. Maxwell's method of arriving at the cost of street lighting per unit. It is true that the load factor of street lighting is 42 per cent. if all the lamps are left on throughout the whole hours of darkness, but there are very few towns or cities where this is done. The figure of 42 per cent. is reduced to 30 per cent. if half the lights are turned out at midnight, and it is slightly more than 30 per cent. if incandescent lamps take the place of the arc lamps switched out at that time. If all the arc lamps are switched out at midnight and incandescent lamps take their place the load factor is reduced as low as 20 per cent. In many towns of about 20,000 inhabitants the hours of public street lighting with arc lamps are below 2,000 per annum, and in such towns the arcs are generally switched off at midnight and incandescents lighted. The height of an arc lamp to give uniform effect has theoretically nothing whatever to do with the candle power. The shape of the photometric curves of a 5-ampere arc and of a 15-ampere arc are practically the Mr. Lackie.

Mr. Lackie.

same. The distance apart should regulate the height. The maximum light in all open arcs is thrown out at 45°, and lamps spaced 40 yards apart should be approximately 20 ft. high, while those spaced 60 yards apart should be nearly 30 ft. high. Mr. Maxwell refers to arc lamps on tramway poles. In Glasgow we have some hundreds of lamps on tramway poles, and I am assured by our Superintendent of public arcs that there is no more wear and tear on arc lamps placed on tramway poles than on those placed on ordinary poles. The vibration caused to lamps on tramway poles by the car traffic is not at all necessarily a bad thing for the lamps. In passing I may mention that all tramway poles and supports are placed 40 yards apart, and it is therefore a little difficult to see how Mr. Maxwell is to get lamps spaced 60 yards apart and at the same time to place them on tramway poles. Lowering gear, of course, cannot be used on crutch lamps, and the best arrangement for public lamps is to fix them and climb to them. On the other hand, if lowering gear is to be used its use should be confined to outlying districts. Lowering gear costs, in the way of interest, depreciation, and sinking fund approximately 12s. per lamp per annum, and an extra man costs 12s. per lamp per annum, so that no saving is effected by its use. The capital cost is one of the chief items in connection with street lighting by arc lamps, and I think the capital cost with a high-pressure system would be very much lower than with a 480-volt scheme. The possibility of dispensing with feeders, line resistances, substitutional resistances, having all the lamps controlled from the station, and the likelihood of reducing the current on the circuits after midnight and increasing it again in the early morning, are considerations which render such a scheme well worth the attention of engineers. I am afraid that the ammeters in the Partick station cannot indicate the respective times when a lamp is using the current and when its substitutional resistance is using it, so that the station staff can learn nothing of the real state of affairs even when the ammeter is giving a correct reading of the current. The figures given as to the cost of 400 10-ampere arc lamps are of course for lamps burning 36 hours with one trimming. If lamps burning 72 hours could be had, the cost per lamp per annum might be reduced by almost £2. Coming to the cost per unit charged for street lighting energy, I cannot agree with Mr. Maxwell that wages should be omitted. In order to give street lighting and all other long-hour consumers the benefit of their high-load factor, wages should be taken as a standing charge in the same way as interest, sinking fund, and depreciation, and dividing up per kilowatt of maximum demand. Generating station labour is required no matter whether one or a million units are being sent out, and it should be taken as a standing charge. If that were done wages would come out as a very small item indeed on a 42 per cent. load factor, and probably would not add to the cost per lamp by more then a few shillings. Just one other point. Mr. Maxwell says that a negative carbon dropping through will cause an earth on a circuit. That should not be the case if the lamps were as a whole insulated. Nor is it necessary to test the drop of pressure at each lamp if one lamp goes to earth. If a test is made at the section pillar the

reading should exactly locate the lamp in which the earth has occurred. Mr. Maxwell's revenue of £3,000 for 300 lamps at 1d. per unit is not consistent with the figure of £7 per annum for 10-ampere lamp. The whole paper is a capital exposition of the Maximum Demand System of charge. That system was tried in Partick and abandoned, but I believe that ere this time the abandonment is matter for regret. Mr. Lackie.

Mr. W. A. CHAMEN: In practice, I agree that it is often most convenient to fix lamps alternately along the sides of the road, but I do not agree that this is really the best way of lighting a street in all cases. Some of the most beautiful street electric lighting which I have seen is in Berlin where the lamps are suspended along the middle of the road. This, however, is difficult, if not impossible, to accomplish in the City of Glasgow. In Berlin there are tramway poles along each side of the road, and these poles are carried up to a considerable height above what is necessary for tramway purposes. Additional span wires with various complications of contact plugs, travelling carriages and raising and lowering gear are fixed right above the tramway trolley wires and are so arranged that the lamp is first brought to one side of the street and then lowered for trimming. The accessories which, as Mr. Maxwell insists, are so necessary (item 7 of the paper) are fixed in the bases of the tramway poles. It will be seen at once that if tramway poles are not regularly used, it will become necessary to fix these accessories, including the winch and other tackle, upon the walls of houses, and this in Glasgow is almost an impossibility; for it will be found that for long stretches there is no wall space between shop fronts which can be occupied by such apparatus. I am of opinion, however, that for lighting the by-streets of a city laid out in chess-board fashion, as so much of Glasgow is, lamps either erected on pillars standing in the middle of the road at the points of intersection and at a considerable height above the ground or on long overhanging brackets from one of the corners will give a most excellent result. No doubt a good deal will be said by gas engineers about the iniquity of placing lamps at long distances apart, but I can say from experience that I prefer to live in a street well lighted in this way than in one with incandescent gas lamps fixed lower down and at more frequent intervals. I do not think that the question of lowering gear for arc lamps as a means of getting at them for trimming without the use of ladders should be put aside as being useless. There are various objections in detail, no doubt, against arrangements for lowering arc lamps, but if, as has been done at Brighton, amongst other places for many years past, lamps can be trimmed by one man walking round with his carbon satchel and a winch handle, some gain has been made at any rate by reducing the cost of wages. We are experimenting at the present time with various kinds of lowering gear, and we hope that something satisfactory and not expensive either in first cost or in maintenance can be arrived at yet. Mr. Chamen.

No doubt the arrangement of separate feeders which Mr. Maxwell describes is very convenient, and in some cases may be quite feasible, but it is scarcely possible within reasonable bounds of cost in

Mr. Chamen. a large city. I also disagree with Mr. Maxwell when he says that time-switches or remote-control switches, are unsatisfactory. We are experimenting with both time- and remote-control-switches, and we have good grounds for hoping for success in one direction or the other, if not in both. With regard to the price charged for current, I quite agree with Mr. Maxwell that it is often charged at too high a figure, but in the city of Glasgow it is no secret that the current consumed for arc-lighting is charged at the low figure of 1d., in arriving at which, however, we do not exclude all wages, interest, sinking fund, and depreciation. We consider that 1d. per unit is a fair price in Glasgow's case for a supply with nearly 50 per cent. load factor, particularly when one keeps in mind that the total quantity consumed per annum is 1,460,780 units, that there are no meters required, and that there are no bad debts and no cost of collection of this revenue. It is only to be expected that electrical engineers come into sharp conflict with their friends the gas-engineers over this matter of street lighting. Mr. Wilson has taken up the usual argument used against arc lamps for street illumination, namely, that they give intense illumination within a certain area, while, on account of the distance between lamps, patches of comparative darkness appear between them. It is worth noting that though the "patchy" nature of arc lighting is complained of by Mr. Wilson, we find gas engineers striving hard to introduce lamps of high candle power themselves. In conclusion, I wish to note a very interesting little detail where Mr. Maxwell states that a negative carbon dropped through the holder is sufficient to cause an earth. This doubtless arises from the fact that the globe rod is attached directly to the pillar in the form of carrier used in most cases where the lamp is fixed directly above the top of the pillar. In hanging lamps, however, this particular little mishap can never occur.

Mr. Calderwood. Mr. W. T. CALDERWOOD (*communicated*): Mr. Maxwell's arrangement for switching in arc lamps from the station from independent 'bus-bars and feeders, using separate machines from the ordinary supply till the lamps are lit up, then parallelling in, is rather impracticable for large towns with extensive street-lighting systems. Time switches controlled from the station are the ideal arrangement. The time switches must, of course, be thoroughly reliable, not too expensive, and they must give a sufficient time interval between the switching in of the various arc-lamp circuits to minimise flickering at the general consumers' lamps. A time switch for this work is being tried in Glasgow arc-lamp street lighting, and it is operated as follows: One time-switch is placed into the base of lamp post of the first of each circuit of ten arc lamps. In the station there is an arc lighting board containing two small tumbler switches, one open reading ammeter, and two incandescent lamps. By closing one of these tumbler switches the first-time switch in the street is closed, thus lighting up the first ten lamps. The closing of this time-switch operates a time element, which, after a predetermined interval, closes the next time-switch, thus lighting the second circuit of ten lamps, and so on. On the station board the ammeter is graduated 1, 2, 3, etc., to show the number of time-switches that have been closed, and on the last switch

being closed, one of the incandescent lamps lights up, thus showing that all the circuits have been completed. In switching out the arc lamps all the switches must be opened before the other incandescent lamp lights up, so that the station engineer can tell how matters stand outside, both for switching in and out. The difficulty about arcing at the switches has been got over. The time-switches, or arc-starters as they are called, are operated from the station through a three-core $\frac{3}{20}$ cable, and this cable is dead except at the time of switching in or out of the lamps. By using arc starters there is a large saving in men's time and also in current. Mr. Calderwood.

Mr. WILLIAM McWHIRTER: Although we may not agree with Mr. Maxwell, I think we must admire the pluck he displays in putting forward certain views and opinions. In many cases these views are not likely to be accepted, but at the same time there are many points in the paper and many suggestions which will be found worth close and careful study. It has been difficult at times for me to follow Mr. Maxwell's reasoning, and there is one thing that I would very much like Mr. Maxwell to refer to in his reply. That is with regard to the part of the paper where he speaks about the incandescent lamps being very awkward to look at owing to the concentration and intensity of their light. I think the older school of electricians always understood that the intensity of a light is really the candle-power divided by the area of the light-giving surface. If I am correct, and we take the area of a 12-ampere arc as one-tenth of a square inch, and the area of a 30 c.p. gas mantle as about 9 square inches, that is, when they are in good order, and that is only occasionally—going upon this assumption, I am sure Mr. Maxwell must admit that he has made a great mistake somewhere. Mr. Maxwell must find some other word than "intensity." It is interesting to note that the author gives the incandescent gas mantle credit for such excellent work. In fact, he admits that a 12½ ampere arc on some occasions replaces as many as six incandescent lamps. I can assure you that I have never paid so much attention to gas lighting for many years, and I am certain that Mr. Maxwell's 12½ ampere lamps can easily replace 16 even at 300 yards. On leaving here last meeting night, I examined about 50 incandescent lamps which I passed on the way. Of these, thirty had mantles badly damaged and globes broken, and at least ten more were burning so badly that one wanted the proverbial match to find them. I have to confess that the result of the observations was very disappointing. The gas mantle as now made is much too fragile for street lighting. It is fragile even indoors, and we may therefore expect to find it more fragile out of doors. The question about giving gas for the value of the residual products has been proposed to my knowledge for the last twenty-five years, and I thought it was dead and buried. I thought the fluctuations which had occurred in these bye-products had practically put it out of court. I regret that Mr. Maxwell has given no figures bearing upon enclosed arcs. In fact, very little has been said about them, and I do not know why the various speakers have fought shy of them, because it is a well-known fact that for years past we have had no improvements at all in the open type of arc lamp; the Mr. McWhirter.

Mr. McWhirter.

improvements have all been confined to enclosed lamps. It is also well known that in the United States open lamps are going out of use entirely, being replaced all over by enclosed lamps with circuits running up to 100 lamps. I think that is an ideal arrangement, and, as there are no steadying coils required in these lamps, the efficiency must be very considerable. Further, I understand that nearly all these lamps are arranged for lowering to the ground, so that there is no running about with ladders or tower waggons, and this altogether tends to a very marked improvement. As a consumer of electrical energy, I object to the suggestion of the author to relieve street lighting at the expense of the private consumers. I think it really too bad that we should be taxed twice. We pay for the energy we use at a ridiculously high rate, and we have to pay rates for the lighting of the streets as well. I think that such a suggestion should not have been made. I do not think Mr. Maxwell is at all wrong when he put aside 7½ for depreciation and sinking fund. And I am surprised to hear Mr. Newington talk about the life of cables. Not one of the lighting engineers here or anywhere else can give the remotest idea of the life of cables. The time has been far too short to talk about reducing depreciation, and I certainly think that when the electric lighting departments reduce this to the figure mentioned by Mr. Newington, they will make a very great mistake.

Mr. Robertson.

Mr. R. Robertson: There is just one point about the paper which I would like to emphasise. Station engineers seem to think that if their current is used for anything that has the name of "power" they can supply it more cheaply than if it is to be used for any purpose which they call "lighting." Mr. Maxwell has clearly shown that to be a fallacy—and that it does not matter to the station engineer what the current is used for, so long as it keeps the plant in use for a longer number of hours for one purpose than for the other, and the question of load factor is the only one that ought to regulate the cost at which the current is supplied for different purposes. I quite agree with Mr. Maxwell that in most cases where public lighting is done by electricity, the price charged, particularly in cases of all-night lighting, is quite excessive in comparison with the cost of supplying such lighting. I have great pleasure in asking you to accord to Mr. Maxwell a hearty vote of thanks for his interesting and useful paper.

Mr. Maxwell.

Mr. Maxwell (*in reply*): I would like to say that most of the station engineers who have spoken on the paper have applied my remarks to some particular case, and several of them have applied them particularly to Glasgow. Glasgow is an exceptional case; it is the second city in the Empire, and it is not likely that a paper written for general purposes could apply specially to it. I think that my remarks would apply to the majority of towns throughout the United Kingdom. Mr. Newington, Mr. Wilson, and several others have referred to arc lamps at a distance of 300 yards apart. The maximum distance in Partick is 180, and that is in extreme cases and with 12½ ampere lamps. Mr. Newington also disapproves of keeping to one particular type of lamp, and says that other types of lamp should be tried. In Partick several types have been tried, and in our opinion we have got the

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best type and have stuck to it. Regarding the same gentleman's remarks about the intensity of the globes, while an ordinary opalescent globe cuts off part of the light, it diffuses the light; consequently it is tolerated. As to the point about separate feeders, in towns like Glasgow and Edinburgh it may be impracticable, but in the majority of towns it is practicable. Mr. Newington says that in Edinburgh the capital cost per lamp is £30, but that surely does not include cables. I expect the figure is for the pole, lamp, and accessories. Mr. Newington also wishes to know if the figures given in the paper are actual figures, and I may say that they are based on actual figures, but the type of lamp advocated in the paper is the 10-ampere size, whereas in Partick we have 12½-ampere lamps. The same speaker thinks there is no need for depreciation as well as sinking fund, and consequently reduced the figure in the paper by £1. Regarding ladders, it is only in the flat parts of Partick that one man can go round with the ladder. There are some very hilly streets in Partick—some of them, with a gradient of about 1 in 5, being absolutely useless for traffic—and it is impossible to expect one man to work a telescope ladder on these. For the flat parts a specially-designed ladder is in use. Mr. Newington also thinks the figure given in the paper for repairs—namely 5s. per lamp—is much too small, but I have found that the actual cost of repairs during the past year has been just about 1s. per lamp, while painting has cost an additional 6d. per pole when each pole was painted every second year. At the commencement of this period 230 of the lamps were practically new, and seventy of them were three years old, so that the figure given in the paper should be ample even when the lamps are old. The same speaker also thinks the figure given for carbons is too high. All the station engineers have objected, as I expected they would, to the station wages being struck out of the cost. When a street lighting extension is carried out you do not buy new machinery especially for the purpose, but you take this extra load into consideration when carrying out extensions, and I fail to see that it costs more in station wages to run, say, four 550 k.w. sets than to run four 500 k.w. sets. The same applies to the boiler-house if mechanical stokers are used. The formula given is meant only as a rough approximation, as each town must of necessity take its own particular conditions into consideration. I am prepared to admit that to a certain extent something should be allowed for wages. The load factor, however, must be taken into consideration; if wages come out at ¼d. per unit, the wages applicable to street lighting must not be taken at ¼d. per unit, but should be proportional to the load factor. It will be noticed also that I have included the whole of the repairs in my price. That is another item which is to a certain extent dependent on load factor. Mr. Newington has said most emphatically that the repairs depend on the number of hours during which the plant is run. I do not think that is so. Supposing the repairs are £2,000 and the plant is being run for four hours a day on the average; then, if the plant was run for eight hours a day would the repairs be £4,000? I do not think so, and the repairs will probably be less in the boiler-house, because a better load factor in the boiler-

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house reduces the repairs. Professor Jamieson said that lamps at a distance of 100 feet from each other are too far apart, but I have some 180 yards apart, and they are very satisfactory. Professor Jamieson is very much mistaken in presuming that Partick has no slums. Partick has slums. Taking it for granted that the life of cables is sixteen years, it is not quite consistent to say that 10 per cent. should be allowed for depreciation on cables, as that would repay the whole loan in about eight years. With regard to Mr. Hanson's remarks that it would be very expensive to put a lamp at each corner of a street where these were close together, there is no necessity for doing so. Mr. Hanson prefers centre poles in wide streets. I do not approve of centre poles even in wide streets, as more lamps are required for efficient lighting. They are used in some cases where there are tramway poles, and there one can only get 40 or 80 yards, which is not satisfactory. Mr. Hanson also prefers lowering gears in hilly towns. There might be some advantage in lowering gears, but I maintain that they have not been sufficiently tried to be put into general use. All the lowering gears in use now are new, and we do not expect new wires to give way. The same speaker has also referred to separate feeders as being impracticable, but in most towns I do not think this is so. Mr. Hanson and Mr. Lackie have referred to the prices given in the *Electrical Times* including capital charges, etc. The figures given in the paper are specially selected. I have the figures of the cost per lamp per annum, and I have worked them backwards from that, and if anything is included in the price of current for capital charges it must be a very minute amount in the cases quoted. The main point is that there are over forty towns in the United Kingdom that charge between £20 and £30 per arc lamp per annum, and some of these do not include capital charge on arc lamps, etc., in that figure. Among these are many large towns. Is it to be wondered, then, that incandescent gas is making such strides in its application to street lighting. Mr. Burbidge advocates the use of twin arc lamps, in which I disagree with him. I believe, however, Mr. Burbidge refers to what is commonly called the double carbon lamp, and in that I agree with him. The same gentleman refers to a lowering gear by which he found one man could attend to fifty lamps in five hours. That means six minutes per lamp, and in that six minutes the man has to get from one lamp to another, lower and raise as well as attend to his lamp. I do not believe that a trimmer can keep his lamps clean if only allowed this time. With forty lamps to attend to in an eight-hour day a trimmer has an average of twelve minutes per lamp, which is not too much even when the lamps are close together. The average time taken by a trimmer with lowering gear or telescope ladder to walk from lamp to lamp and lower and raise the lamp in the one case or to raise and lower the ladder and climb up in the other case is about four minutes when the trimmers are taking their own time and do not know they are being observed. I do not think that less than six minutes should be devoted to cleaning and trimming if satisfactory results are to be obtained. Mr. Robertson advocates single or semi-enclosed lamps, but he says afterwards that he would not

Mr. Maxwell.

advocate them for main streets. If they are not good enough for main streets they are not good enough for side streets. Mr. Robertson also referred to rents, rates, and taxes. The addition of a street lighting scheme makes very little difference to the rates and taxes, and when divided by the large number of units consumed by such a load this item is almost negligible.

I can assure Mr. Wilson that there are thousands of flat flame burners still in use. I have taken the trouble to find this out, and I have ascertained that in Birmingham there are 8,000, in Salford 5,000, in St. Pancras 3,000, in Halifax 2,700, in Hackney 2,500, in Dundee 3,000, and in Aberdeen 2,500; so that we can hardly call the flat-flame burner a thing of the past. Mr. Wilson pointed out that the lighting with arc lamps was "blotchy." Mr. Chamen has answered this point in his communication, and I need say nothing further. As to the point that was made about arc lamps being worse for concentration and the intensity of light, we get out of that by cutting off half the light in order to diffuse the light, and even after this loss we can beat gas. Mr. Wilson also referred to remarks made in the paper about the distance of 150 yards away from the lamps. That, of course, refers to objects between lamps at 150 yards from the observer, and likewise 300 yards away from the observer with arc lamps. In that light, I think Mr. Wilson would see that the remarks made in the paper are quite reasonable. Mr. Wilson understands the supply of gas, but he does not understand this point, that, whereas in gas supply he is running practically with a load-factor of 100 per cent., in electricity supply that is not the case. In the remarks that have been made, my net figure for current has, I think, been entirely borne out by the other station engineers, so that Mr. Wilson's opinion stands practically alone. If Mr. Wilson thinks my figure too small, I can assure him that I can put down a station for street-lighting purposes solely, and supply at a profit for the same figure, and I can even cut off half the light in opalescent globes, and still beat incandescent gas. Any of the members who have recently had the opportunity of looking through the gas journals will have seen that, at any rate, any paper about electric street lighting is worth devoting considerable space to. Mr. Wilson also said that he only knew one town worse lit than Partick, and that was Brighton. I maintain that the lighting of the back streets of Glasgow is a disgrace to any city, and any one who goes through the back streets of Glasgow at night, as I have done—solely for the purpose of looking at the gas lamps—will bear out this statement. I would draw attention to Mr. Wilson's statement, that renewals only cost 1s. 9d. per lamp per annum. If that is a fact, then it explains everything. That is why Glasgow gas lighting is so bad. There is no other town in the United Kingdom that can get anywhere near such a figure. Even in Birmingham, which has the cheapest gas street lighting in the kingdom, the figure is very much higher, being about 4s. dearer on wages and renewals together. Further, in his gas costs, Mr. Wilson includes nothing for interest and repayment on the £23,000 now being spent on the gas lighting of the city.

Mr. Wilson cited certain towns where arcs are practically not used

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at all. That bears out my own remarks. Why are they not used? Solely because the prices are too exorbitant. Perhaps it is advisable not to particularise, but nearly all the towns quoted by Mr. Wilson charge enormous prices for street arc lighting, and I have a list of about fifty towns where it is £18 10s. per lamp per annum and over, and in one as high as £30. Regarding the curve drawn out by Mr. Wilson, 10-ampere arc lamps have been taken at 180 yards apart, and in the paper I have dealt with 12½-ampere lamps. Then, the candle powers shown on the curve cannot have been taken from actual tests. It may not be irrelevant to state that one journal—*The Street*—in a comparative table gave the most economical street gas lighting as 1·103 pence per 1,000 candle-power per annum as compared with 2·048 pence for the best arc lighting. The cost of gas was taken at 2s. 8d. per 1,000 cub. ft., *and of electricity at* 4·096 *pence per unit*. Comment is needless. The same paper also gave the cost of this form of lamp, called the "Milennium," as £15 10s. 5d. per lamp per annum exclusive of capital charges. The Milennium gas light is quite new, and it will probably be found after further experience has been obtained that the figure given will be considerably exceeded. Mr. Lackie has stated that if they put arc lamps sixty yards apart, they ought to be thirty feet high. That may theoretically be correct, but if we go on at that rate, what should be the height of lamps which are 180 yards apart? This speaker also approved of the use of steel tramway poles, saying that the maintenance is no greater when these poles are employed than with separate poles. If side poles with span wires attached are used, it has been found that the screws get loose and drop out, and in Partick we are doing away with these as quickly as possible. Mr. Lackie gave a figure of 12s. per pole as the cost of maintenance, etc., of lowering gears. I did not think it was so much, but it bears out what I have said. With regard to Mr. Lackie's final remarks, I think that gentleman has forgotten that in Partick there are 12½-ampere lamps, not 10-ampere lamps. Mr. Chamen has given some very interesting particulars about the arrangements at Berlin, and has also referred to lighting side streets with a pole in the centre of the crossing. At long distances one does not get the same effect from the centre pole as with the lamps at the side of the street. Mr. Calderwood has given particulars of a very interesting arrangement for switching on the lamps. Of course I had no knowledge of this when I put down such an expensive feeder arrangement in Partick, but although this method appears satisfactory, I do not like the arrangement unless the switches are operated from the feeding points and not from the distributors. Mr. McWhirter apparently cannot understand the remarks made in the paper about intensity or concentration. He said the area of the arc was about half a square inch, but the arc was not seen. One should not see the arc actually, but must take the area of the globe. Half of the light is cut off in order to diffuse it properly. Another point is that the arc lamp is high up, being placed beyond one's line of vision, and there is no necessity to look up at it at all. The same gentleman referred to one hundred enclosed arc lamps on one circuit. That meant supplying arc lamps in series at 10,000 volts, and I should not care to do that.

MANCHESTER LOCAL SECTION.

THE ELECTRICAL OPERATION OF TEXTILE FACTORIES.

By H. W. WILSON, Associate Member.

(*Paper read January* 17, 1905.)

The author is aware that during the last two or three years a considerable amount has been written concerning the subject dealt with by the present paper; but, in spite of this, a certain amount of misapprehension exists as to the exact conditions to be met.

It is, of course, obvious that the adoption of the electrical transmission of power for the operation of textile factories of all kinds, and more particularly of cotton mills, is a subject of great importance to the electrical industry of this country, for if once seriously adopted it would mean the opening of a very large market for the manufacturers of electrical apparatus. It is proposed in this paper to consider very briefly several features of the question, but so far as possible to pay special attention to the question of present costs of mill-driving, concerning which there is very little information before the public.

Having regard to the extensive adoption of electrical driving of textile factories, both in the United States and on the Continent, the first striking point is the very small amount of similar work which has been done in this country, considering the efforts which have been made by various firms to induce mill-owners to give the systems which they have advocated a trial. In the author's opinion there are a number of reasons for this, the first and most important being the fact that the depression which has existed in the textile industry during the last three years, and which now seems to be passing away, has made the mill-owners very chary of taking up any schemes involving a considerable capital expenditure; and in the second place, the natural conservatism of this country, which apparently causes all the manufacturers to wait until some one else has tried a new thing before experimenting themselves; and thirdly, to a certain extent, the number of contradictory statements which the advocates of electrical driving have put forward. Various advocates of electrical driving have, in their enthusiasm, put forward statements as regards increase of production, efficiency of transmission, and particularly economy of coal, which it was perfectly impossible to guarantee; and as the average mill owner and mill manager is well up in all details of his own costs, he is sometimes

apt to condemn electrical driving altogether on account of such mis-statements. It is exceedingly doubtful whether, in the case of a new mill, any considerable economy of fuel could be shown by adopting electrical driving, and the case for its adoption should be urged on quite other grounds.

As is well known, a very large amount of work has been carried out in the United States in the electrical driving of both spinning and weaving mills, and in the beginning this development was due to the necessity of taking advantage of water powers in the neighbourhood of the mills. All the mills first fitted up in the States had water turbines as their prime movers, and it was not until some years after when the advantage of electrical driving had become apparent that the idea of electrically operating a mill, the prime mover of which must be steam-driven, was seriously considered. Experiments were then made in this direction with very satisfactory results; and, speaking generally, it may now be said that the bulk of the new mills in course of erection are to be electrically driven. A large amount of similar work has been carried out on the Continent, particularly in Russia, Italy, and Spain, with satisfactory results, but the bulk of the installations about which accurate details are available have been laid down in the United States. Experiments, however, have and are being made in this country, with some of which the author has personally been concerned, and the results which have been obtained have convinced some of the leading cotton manufacturers that the subject is very well worthy of their consideration.

One or two large textile factories in this country have recently been arranged for electrical driving, one being a linen thread spinning works in Scotland, and by the courtesy of The British Thomson-Houston Company the author has been allowed to give some diagrams showing the results obtained here. Another instance is that of a weaving shed in East Lancashire, which has just been started, and the results have, it appears, been so far satisfactory. The advantages which the advocates of electrical driving urge for the operation of large textile factories are stated briefly as follows:—

(1) The mill and the engine-house can be placed each in its most convenient situation without any regard to their relative positions.

(2) The internal arrangements of the mill as regards shafting, gearing, belt, and rope drives, etc., are greatly simplified and their costs reduced. The flexibility as regards extensions is, of course, obvious.

(3) The grouping of the machines is much less arbitrary than in a mechanically driven mill, as the motors and the comparatively light shafting required by them can be placed where most convenient.

(4) The reduction of the chance of a breakdown, which would stop the whole mill, to a minimum.

(5) The ease of running one section for overtime or on special work.

(6) The reduction of the maintenance and depreciation charges.
(7) The greater steadiness of drive which can be obtained under suitable conditions, with a subsequent permissible higher speed and increased output.
(8) The reduction in the total capital cost of the mill per spindle or per loom with a factory of above a given size.
(9) The possibility of keeping a constant check upon the results obtained in each department of the factory.

The most important one, and the one which has been most keenly discussed, is No. 7. A great many mill engineers and managers in this country decline to admit its probability, and it requires very considerable argument to make them admit the possibility of the suggestion.

Electric driving permits of greater production, because the machinery can be run on the average nearer the maximum possible speed than when operated mechanically. It cannot be run at a higher maximum speed, as the manufacturing conditions do not permit of this, but the average speed is higher. Exactly accurate figures are, of course, difficult to obtain, and one is again, unfortunately, compelled to use data obtained from American sources. These show that an increase of production of some 4 per cent. in weaving and up to 8 per cent. in spinning may be expected and obtained. It is obvious that an increase in production of this order would more than offset a considerable increase in the cost of power, should this be required.

As regards the statement that a considerable increase in the cost of power is of little importance if accompanied by an increase in output, it may, perhaps, be advisable to point out that the cotton spinners and weavers state their costs at so much per pound of yarn or cloth produced. In the case of a spinning mill, on most classes of counts, it may be taken that the margin over the price of raw cotton, out of which the manufacturers have to pay all their costs and make their profit, is something between 3d. and 3¾d. per lb. Assuming a price of 4d. per lb. therefore, the selling price of the yarn would be 7½d. A considerable proportion of the 3½d. manufacturing cost is made up of the piece-work price per pound of yarn produced which is paid to the operatives, and it would not be easy to get any modification of this piece-work price. What may be regarded as the standing charges of the mill—that is, the amount required to cover rent, rates, taxes, insurances, cost of power, salaries, and day labour—is but a comparatively small percentage of the price of the finished article, and it therefore takes a considerable increase in output to make any considerable difference in the total costs per pound. Assuming, however, that these fixed charges represent about 12 per cent. of the price of the product, which appears to be about the correct figure, an increase in output of 5 per cent. would mean a reduction in the total cost of the goods equivalent to 0·6 per cent. In a modern spinning mill the cost of power does not usually represent more than 2½ per cent. to 3 per cent. of the production costs, and an increase of 10 per cent. in the

cost of power would only represent about 0·25 per cent. increase in the total costs. The percentage margin of increased profit may, therefore, seem very small, but on a turnover of the amount which the business of a large cotton-mill represents it would be quite perceptible in the annual balance-sheet. Various private experiments have been made in this country which show similar, and in some cases even better, results to those mentioned above, but the author is not at the present time at liberty to give details.

Various objections are urged against the electrical drive, which it will be advantageous to mention at once, as they will be dealt with more or less in various portions of this paper.

The first objection is that the reliability of electrical driving has not been sufficiently proved.

Secondly, that the capital expenditure involved in the adoption of the electrical system is so great in comparison with mechanical driving as to put it out of court.

Thirdly, that it is only advantageous in special cases, or where the average load factor is poor.

Fourthly, that the efficiency of a mechanical drive is considerably higher than that of an electrical one.

As regards the first objection, it is one with which all electrical engineers have become familiar by its constant repetition. It is a companion saying to that venerable friend of ours, that "electricity is still in its infancy," and just about as correct. As regards the question of the reliability of a properly installed electrical plant, there can be no two opinions amongst those who know anything of the subject.

The second objection is answered by the eighth item on the list of advantages claimed, and is a point which will be dealt with later in the paper.

As regards the third objection, one frequently meets persons who are prepared to admit that it may be advisable to adopt electrical driving for awkwardly arranged old mills which are being partly refitted, or for a number of small mills close together all belonging to one owner, but that in the case of a new mill the advantages of the electrical drive are mythical. This point will also be dealt with later.

The fourth objection, that the efficiency of an electrically driven mill is not so great as that of a mechanically driven mill, is a much debated point. The author would point out that the bases for argument are not sufficiently good to enable one to say exactly which view is the correct one. It is, of course, perfectly easy to measure exactly the efficiency of an electrical plant from the prime mover to the driven machine, and to state with positive accuracy what the efficiency is. In the case of a mechanically driven mill no one can state definitely what the actual efficiency at full load really is. The time-honoured method of taking a friction diagram with all straps on the loose pulleys, and then taking a full-load diagram, and considering that the difference between the two represents the percentage efficiency, can only be a very rough approximation to the truth, and the actual efficiency must be considerably lower than the results of such an experiment would show. The

author was once in charge of some experiments which involved, amongst others, the driving of a number of mule spinning frames by a single large induction motor. To begin with, for a period of three weeks the motor was driving the frames from a line shaft through a rope drive, and records of the power taken were obtained from an integrating wattmeter. After this the motor was directly coupled to the line shaft through a flexible coupling and run for another period of three weeks, the mules running the same number of hours per week and spinning exactly the same counts. With the rope drive eliminated the production results were somewhat increased, and the average power taken for the whole period reduced by between 12 and 13 per cent. It would seem, therefore, fairly obvious that the more belt or rope drives or other transmission gears introduced the lower must be the efficiency, and persons who argue that with the most modern forms of rope drive it is possible to obtain efficiencies of the order of 98 per cent. are exceedingly sanguine.

Upon this question of efficiency accurate figures are difficult to obtain, and it must be admitted that in the most modern mills the results are probably very good; but it is very much to be questioned if a better result than 80 per cent. is ever obtained, and certain instances of even modern mills, where the result is nearer 60 per cent., are known to the author. In one mill, which recently had a new engine put in, the full-load diagram was 738 I.H.P., and the light-load friction diagram 300 I.H.P., or an apparent efficiency of 59 per cent.

The next point that arises is the consideration of the conditions to be fulfilled, and the various types of machinery to be driven.

In a spinning mill there are the various preparation machines, including beaters or openers, scutchers, carding engines, drawers and slubbers, intermediate and roving frames, which, speaking generally, are a very steady load; ring spinning and ring doubling frames, which, running at a given speed and upon one class of goods, represent an almost perfectly steady load; and spinning mules, individual machines of which class, from the conditions of their working, represent one of the most variable loads it is possible to obtain. In a weaving shed, manufacturing a given class of cloth, the load should be very steady; and in a drying, bleaching, and finishing factory the load on the different machines is exceedingly variable, though the load on the prime mover may be moderately constant. For spinning and weaving absolute constancy of speed is desired; for bleaching, dyeing, and finishing speed variation on the machines is essential.

Having regard to the conditions of the case, it will be seen that for spinning and weaving induction motors offer great advantages, and the bulk of the electrical machinery now used in this class of factory is three-phase, 40-∿ or 50-∿ 500-volt plant. For bleaching, dyeing, and finishing, with the large speed variation called for, direct current machinery must be employed.

As the limitations of this paper will not permit of dealing at all fully with the question of dyeing and finishing machinery, it may be said at once that the most up-to-date manufacturers, who have tried electrical driving for this class of work, admit that the results obtained are very

greatly superior to anything that can be done by mechanical driving, largely on account of the speed variations required. The average load factor for work of this description is generally low, and the generator capacity is sometimes not more than 50 or 60 per cent. of the motor capacity. Some very ingenious arrangements for speed control have been brought out for this class of work by several firms, including Messrs. Mather & Platt, in this country, and the Ward Leonard Company, in America.

The author has collected various figures as to the powers taken by different machines used in dyeing and finishing, and gives them in Table I. herewith, as some of them may be of interest, and it may be mentioned that in nearly all cases each machine has its own motor.

In discussing the best arrangement for the electrical driving of a large spinning mill a number of points have to be considered. To maintain the average speed at the highest possible it is necessary to reduce all belt or rope transmission between the prime mover and the driven machine to a minimum. The question therefore immediately arises: Is it better to provide for a motor directly connected to each machine, or for the machines to be grouped and driven by larger motors? It is obvious, of course, in saying this that the case of the driving of mules must be considered separately.

The single mule is such an extremely variable load that, so far as any results up to the present have shown, it is impossible to drive them singly by motors. It is necessary to drive a number of them from a single machine, so as to damp out the fluctuations as much as possible, and get something approximately resembling an even load. With four mules on a single motor the author has seen variations in demand from 15 to 90 H.P. almost instantly, and this manifestly represents a condition of affairs under which no motor can maintain an absolutely constant speed. Broadly speaking, it cannot be said to be good practice to drive less than twelve mules from a single motor of, say, 150 H.P. Under these conditions good results are being obtained. However, with the other machines, which normally give a fairly constant load on their driving machinery, these conditions do not apply. It is possible to either use a number of comparatively small motors, or in the case of looms very small motors directly connected, or driven through a single strap on to the various machines, or to group the machines and drive through larger motors. With the possible exception of ring spinning and doubling frames, there is very little doubt in the author's mind that the correct policy is to group the machines. The larger motors have a considerably higher full-load efficiency, and the total cost of the installation is much lower than if small motors were installed. With ring spinning frames constantly on one class of counts the larger number of small motors may possibly be worth considering on account of the more even speed obtained.

In order to give some idea of the powers taken by the various classes of machines, a few extracts from a paper read by Mr. E. W. Thomas are given. The tests detailed were made at the Olympia Cotton Mills, Columbia. The machines driven and the various motors were as follows:—

Machine.	Conditions of Test.
6-Bowl Calender, bowl 4′ 8″	Piece 3′ 8″ wide. Only ¼ weight on bowl.
4-Bowl Dry Mangle, 4′ 0″ ×	No weight on / Full weight on } Piece 3′ 0″ wide.
3-Bowl Water Mangle, 4′ 4″	
2-Bowl Starch Mangle, 4′ 1	Piece 3′ 0″ wide. Doing very thin stuff. Doing light stuff. At lowest speed.
23-ton Drying Range ..	Motor at 575. 2 pieces 2′ 10″ wide going through.
Damper Bowl, 5′ 0″ × 18″ ..	Shafting only. Machinery running light.
2 Folders, 5′ 0″ wide ..	Maximum load. Both on.
Brush Washing Machine ..	
Rubber Polishing Machine..	
20-ft. Stenter	
Beamer	
Small Calender	
Friction Calender	
White Squeezer	8-h.p. at normal speed.
White Washer	Top speed.
Dyer Becks	

"Five openers on the first floor of the mill are driven by a 20-H.P. motor, belted (No. 20A in Table II.).

"Five breakers, ten intermediate and ten finisher scutchers on the third floor, 45 in. wide, are driven by a belted motor of 150 H.P. (No. 21, Table II.).

"Eighty-eight 45-in. revolving top cards are driven by a 150-H.P. belted motor (No. 14).

"Sixty-two 45-in. cards are driven by a 150-H.P. motor (No. 15).

"Two hundred and forty drawing deliveries, 1,140 slubber spindles (12 by 6), 3,432 intermediate frame spindles (10 by 5), and 608 fine frame spindles (7 by 3½) are all driven by a belted 150-H.P. motor (No. 9).

"Thirteen thousand nine hundred and eighty-four fine frame spindles are driven by two belted 150-H.P. motors (Nos. 3 and 4).

"One hundred thousand three hundred and twenty ring spindles are driven by ten 150-H.P. motors, each motor being directly connected to the shafting it operates, thus obviating the use of counter belts, and between the engines generating the power and the spinning frame there is only a 2¾-in. belt, that being the belt from the line shaft to the frame direct. The motors operating the frames are numbered 1, 2, 7, 8, 10, 11, 16, 17, 18, 19. Six slashers and an exhaust fan on the second floor of the mill are driven by a 30-H.P. belted motor (No. 20C).

"The 2,250 40-in. Draper looms, as well as the necessary spooling and warping, are driven by four belted 150-H.P. motors and one 75-H.P. belted motor, the numbers of the motors being 5, 6A, 6B, 12, and 13. The looms are located on the first and second floors, and the spoolers and warpers on the second floor. The cloth room, containing six sets of cloth-room machinery, is driven by a 50-H.P. belted motor (No. 20B) located on the first floor.

"Table II. shows the various motors in this mill, with the H.P. developed by each under full load, and also without the load on the machinery. The numbers of the various motors correspond with the numbers given in the description of their location and distribution above."

Summing up, it is found that the combined rating of the motors in the mill was 3,175 H.P., and that the total load was 2,530·45 H.P., to which should be added 66·88 H.P., the equivalent of the machinery not in operation when the tests were made. This makes the total power required at the switchboard 2,579·33 H.P. Again, in considering the power required to drive each motor without machinery load, it must be borne in mind that the figures given not only cover the friction of the rotating shafts, couplings, and pulleys, but also all losses, great or small, between the motor and switchboard, the instrument making the records being located in the engine-room, and its connections being made at the switchboard.

Again, consideration should be given to the fact that in some cases the larger size motor drives but a small amount of shafting, while in other cases a small motor may drive a very much larger amount of shafting than its larger neighbour. The amount of power for the motor and shafting, including all losses, was 552·1 H.P. for the

TABLE II.

Horse-Powers, Full Load and No Load.

Motor No.	Size Motor H.P.	H.P. Full Load.	H.P. No Load.	Per cent. Machinery on Full Load.	Department.
1	150	119·6	26	89·76	Weft Spinning
2	150	123·5	32·5	100	" "
3	150	61·75	13	89·13	Fine Roving
4	150	58·5	13	82·63	" "
5	150	156	29·25	96·68	Draper Looms
6A	150	136·5	29·9	100	" "
6B	75	91	19·5	100	" "
7	150	135·20	28·60	100	Weft Spinning
8	150	126·10	26	85·15	" "
9	150	97·50	24·7	100	Roving and Drawing
10	150	133·90	26	100	Warp & Weft Spinning
11	150	130	24·7	100	" " "
12	150	120·9	36·4	100	Spooling, Warping, and Draper Looms
13	150	132·6	39	95·51	Spooling, Warping, and Draper Looms
14	150	84·5	15·6	100	88 Cards, 45 in.
15	150	52	13	100	62 " "
16	150	139·1	26	100	Warp Spinning
17	150	143	23·4	100	" "
18	150	148·2	26	100	" "
19	150	165·1	31·2	100	" "
20A	20	19·5	8·45	100	Openers
20B	50	19·5	10·40	100	Cloth
20C	30	26	10	100	Slashers
21	150	110·5	19·5	100	Scutchers
	3175	2530·45 *66·88 2597·33	552·1	21·81	

* This item, 66·88 H.P., is the estimate of power required for the machinery stopped during the test on motors 1, 3, 4, 5, 8, 13.

entire mill; but in some cases, as noted before, some machinery on motors numbered 1, 3, 4, 5, 8, and 13 was unavoidably in operation. Taking, then, the actual power that would have been required to operate all the machinery at the same time—namely, 2,597·33 H.P.—then the percentage of the total power required to drive the motor, shafting, and line losses was 21·25 per cent.

TABLE III.

Spindles per Horse-Power.

Warp Spinning :—

Motor No.	Spindles per H.P. full-load, including friction, motor, and shafting.	Spindles per H.P. full-load and deducting friction of motor and shaft.
16	68·3	84
17	66·4	79·4
18	68·3	82·2
19	60·7	75·9
Average ...	65·92	80·12

Weft Spinning :—

Motor No.	Spindles per H.P. full-load, including friction, motor, and shafting.	Spindles per H.P full-load and deducting friction of motor and shaft.
1	75	96
2	81·2	110·2
7	78·1	99
8	71·3	90
Average ...	76·4	98·8

Table IV. gives the looms per H.P. under conditions as in Table III.

TABLE IV.

Looms per Horse-power.

Motor No.	Looms per H.P. full-load, including friction, load of motor, and shafting.	Looms per H.P. full-load after deducting H.P. of motor and shafting.
5	3·0	3·68
6A	3·55	4·56
6B	3·56	4·53
12	3·68	5·26
13	3·70	5·23
Average of last four tests	3·62	4·87

Table V. gives the weights of the shafting, couplings, and pulleys, as also a list of the number of the bearings. This Table is interesting from the fact that this mill has undoubtedly less weight of rotating material per spindle for its shaftings, couplings, and pulleys than any mill that has ever been put in operation, that is complete in itself, and is a cloth-producing mill.

The weight of shafting per spindle is $3\frac{3}{8}$ lb. The diameter of the shafting in the Scutching Room is $2\frac{11}{16}$ and $2\frac{15}{16}$ in. In the Card Room it is $2\frac{11}{16}$ and $2\frac{15}{16}$ in. In the Spinning Room, $2\frac{11}{16}$ in., and in the Weaving Room, $2\frac{7}{16}$ and $2\frac{15}{16}$ in. The $2\frac{15}{16}$ in. shafts are the receiving shafts

for the belts from the motors, and also those where one shaft is belted from another, as in the case of the Carding and Weaving Rooms. All the pulleys in the mill above 20 in. diameter have wood rims.

The bays of the mill are 10 ft. 8 in. from centre to centre.

TABLE V.

Weights of Shaftings, Couplings, and Pulleys.

Motor No.	Shafting.	Coupling.	Pulleys.	Totals.	Number Bearings and Diameter.
1	1,912	354	2,660	4,926	$11 \times 2\frac{11}{16}$
2	1,912	354	2,660	4,926	$11 \times 2\frac{11}{16}$
3	6,018	920	5,970	12,908	$38 \times 2\frac{15}{16}$
4	6,018	920	5,970	12,908	$38 \times 2\frac{15}{16}$
5	18,080	3,750	14,467	36,297	$100 \times 2\frac{7}{16} - 15 \times 2\frac{15}{16}$
6A	18,080	3,750	14,617	36,447	$100 \times 2\frac{7}{16} - 15 \times 2\frac{15}{16}$
6B	11,219	2,250	8,928	22,397	$60 \times 2\frac{7}{16} - 9 \times 2\frac{15}{16}$
7	2,105	442	2,800	5,347	$12 \times 2\frac{11}{16}$
8	2,105	442	2,800	5,347	$12 \times 2\frac{11}{16}$
9	10,228	1,380	9,424	21,032	$78 \times 2\frac{15}{16}$
10	1,893	354	2,840	5,087	$11 \times 2\frac{11}{16}$
11	1,893	454	2,840	5,087	$11 \times 2\frac{11}{16}$
12	23,613	4,631	14,725	42,969	$129 \times 2\frac{7}{16} - 18 \times 2\frac{15}{16}$
13	23,408	4,742	17,147	45,296	$136 \times 2\frac{7}{16} - 21 \times 2\frac{15}{16}$
14	17,758	2,328	5,784	25,870	$37 \times 2\frac{11}{16} - 8 \times 2\frac{14}{15}$
15	6,882	1,746	4,562	13,190	$30 \times 2\frac{11}{16} - 8 \times 2\frac{15}{16}$
16	1,918	354	2,880	5,152	$11 \times 2\frac{11}{16}$
17	1,918	354	2,880	5,152	$11 \times 2\frac{11}{16}$
18	1,898	354	3,040	5,292	$11 \times 2\frac{11}{16}$
19	1,898	354	3,040	5,292	$11 \times 2\frac{11}{16} - 5 \times 5\frac{5}{8}$
20A	2,001	88	2,406	4,495	$7 \times 2\frac{11}{16} - 29 \times 1\frac{15}{16}$
20B	2,899	230	1,273	4,402	$6 \times 2\frac{15}{16} - 12 \times 2\frac{11}{16}$
20C	1,999	370	774	3,143	$3 \times 2\frac{15}{16} - 8 \times 2\frac{11}{16}$
21	2,890	115	1,695	4,700	$12 \times 2\frac{11}{16} - 6 \times 2\frac{15}{16}$
	179,545	30,936	136,182	337,643	
lb. per spindle	1·70	3·09	1·361	3·376	

The following shows some tests made on motor No. 17 operating warp spinning frames. There were 18 frames of 272 spindles, and 18 of 256 spindles, making a total of 9,400 spindles.

Per cent.	Frames.	Spindles.	H.P.	
Full Load	36	9,504	143	66·4 spindles per H.P.
$83\frac{1}{3}$	30	7,920	124·8	63·4 " "
$66\frac{2}{3}$	24	6,336	104	60·9 " "
50	18	4,752	84·5	56·2 " "
$33\frac{1}{3}$	12	3,168	61·75	51·3 " "
$16\frac{2}{3}$	6	1,584	42·25	37·5 " "
No.	0	—	23·40	or $16\frac{3}{10}$ per cent. of full load

Deducting 23·40 H.P. in each case above, the nett power for the frames is as follows :—

143	H.P.	= 23·40 + 119·6	H.P.	or 79·4	spindles per H.P.
124·8	„	= 23·40 + 101·4	„	or 78·1	„ „
104	„	= 23·40 + 80·6	„	or 78·6	„ „
84·5	„	= 23·40 + 61·1	„	or 77·7	„ „
61·75	„	= 23·40 + 38·35	„	or 82·6	„ „
42·25	„	= 23·40 + 18·85	„	or 84	„ „

The following shows some tests made on motor No. 1 and represent the power required for driving 38 ring weft spindles, 19 of 272 spindles, and 19 of 256 spindles, equalling 10,032 spindles. In this test a portion of the spindles were not run during any part of the time.

	Spindles.			
Full Load	8,976	119·6 H.P.	or 75	spindles per H.P.
83⅓ per cent.	7,392	104 „	or 71·8	„ „
70·6 „	6,336	93·6 „	or 67·6	„ „
52·9 „	4,752	75·4 „	or 63	„ „
35·3 „	3,168	58·5 „	or 51·1	„ „
17·6 „	1,584	41·6 „	or 38·1	„ „
0 „	—	26 „	or 21·7	per cent. of full load.

This percentage would have been materially reduced had the test for full load included the full number of spindles, viz., 10,032. Taking out the 26 H.P. from each of the above amounts, then :—

119	H.P.	= 26 + 93·6	H.P.	or 96	spindles per H.P.
104	„	= 26 + 78	„	or 94·8	„ „
93·6	„	= 26 + 67·6	„	or 93·7	„ „
75·4	„	= 26 + 49·4	„	or 96·1	„ „
58·5	„	= 26 + 32·5	„	or 99·5	„ „
41·6	„	= 26 + 15·6	„	or 101·5	„ „

The frames are 2¾-in. gauge, 1⅜-in. ring, 9,058 revolutions per minute spindles, running on No. 40½ weft. The motor is 150 H.P. directly connected.

The following shows some tests made on motor No. 14 driving 88 45-in. revolving top cards.

With the shafting, motor, etc., the H.P. was shown as follows :—

Full load, 88 cards, 84·5 H.P. or 0·96 H.P. per card.
Half „ 44 „ 48·75 „ or 1·10 „ „
No machinery 15·6 „ or 18·4 per cent. of the full load for all cards.

Deducting the friction load of motor and shafting, etc., from the total loads, this gives :—

84·5 H.P. = 15·6 + 68·9 H.P.......... 0·783 H.P. per card.
48·75 „ = 15·6 + 33·15 „ .... 0·783 „ „

The following shows some tests on motor No. 5 driving 482 40-in. Draper looms on two-harness work, 16 of these looms being unavoidably stopped throughout the entire test. Practically one-half of these looms were on the first floor of the mill, and one-half on the second floor, but all were driven from the lower floor. As has been noted, the 2,250 looms in the mill are driven by 5 motors, but only one motor is given below :—

With 466 looms	H.P.	required	156	or	3	looms per	H.P.
345 „	„	„	106·6	or	3·23	„	„
223 „	„	„	78	or	2·86	„	„
120 „	„	„	78	or	2·20	„	„
No. „	„	„	29·25	or	18·8	per cent. of full load.	

Deducting the load of the motor and shafting from the load in each case above, we have :—

156 H.P.	= 29·5 +	126·5 H.P.	or 3·68	looms per	H.P.
106.6 „	= 29·5 +	77·1 „	or 4·47	„	„
78 „	= 29·5 +	48·5 „	or 4·60	„	„
54·6 „	= 29·5 +	25·1 „	or 4·78	„	„

This motor was a belted one of 150 H.P. driving five lines of shafting, each line being 214 ft. 4 in. long, and $2\frac{7}{16}$ in. diameter, except the lengths having the counter pulleys upon them. These are $2\frac{15}{16}$ in. diameter. The total weight of shafting, couplings, and pulleys is 36,297 lbs. There are 100 bearings, $2\frac{7}{16}$ in. diameter, and 15 of $2\frac{15}{16}$ in. diameter. This test was made on July 12, 1904. The temperature in the room was 88 deg. and outside the mill 82·6 deg. The speed of the shafting was 300 revs. per minute, and the speed of the looms 163 revs. per minute.

On the tests of the other motors operating looms, after deducting the powers required for the motors and shafting :—

Motor	6A	ran	4·71	looms per H.P.
„	6B	„	4·93	„
„	12	„	4·92	„
„	13	„	5·16	„

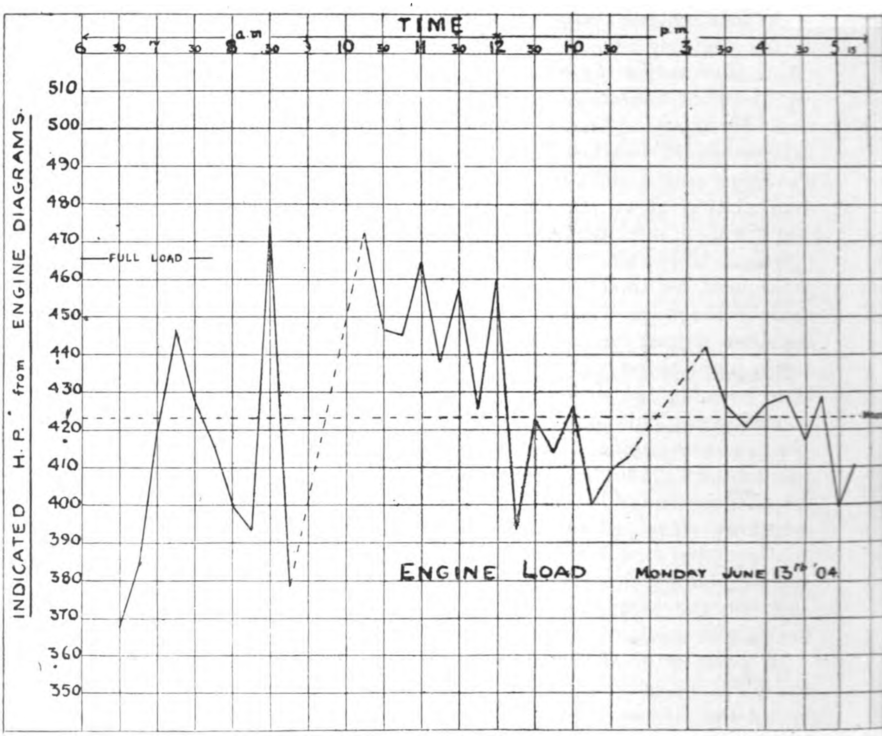
Power Costs in Mechanically Operated Mills.

A point which, to the author, seems of the greatest importance, and upon which very few figures are available, is the actual cost for power at the present time in modern mechanically driven mills. The author has collected a considerable amount of information on this subject, and though the figures may at first seem somewhat startling, there is no doubt as to their approximate accuracy. Cotton manufacturers nearly always state their power costs in terms of indicated horse-power, which, naturally, is the most convenient unit for them to use, and sometimes reckon in terms of the indicated H.P. hour, and sometimes the indicated H.P. year. The average of the results which the author

has come across show that in a modern spinning mill of good size it may generally be taken that the generation costs per I.H.P. hour upon the basis of calculation mentioned below, in the case of a mill running full time, amounts to about one-twelfth of a penny, or, say, approximately, about £1 per I.H.P. year. Allowing for repairs, interest, and depreciation (interest and depreciation being taken at about 10 per cent.), the total cost per I.H.P. year comes out to something of the order of £2 5s. to £2 10s. In the case of a weaving shed, which is normally a smaller undertaking with less engine power, and in which, of course, the proportion of the standing charges per I.H.P. comes higher, the cost may be taken as between £2 10s. and £3 5s. per I.H.P. year. The figures represent, of course, remarkably good results, and will take a great deal of beating; in fact, it is obvious that upon the question of economy only it would be very difficult to improve upon the figures which a modern cotton mill can show. The amount allowed for interest and depreciation of 10 per cent. on the steam plant may be criticised, and it may at once be said that great difficulty exists in obtaining exact figures as to the allowances made by the different mills, and it appears to depend, in some cases at all events, on the requirements of the balance-sheet for a given year. Many of the mill owners, however, consider 10 per cent. interest and depreciation allowance on their steam plant as too high, and point out that in some cases engines have been running for periods of thirty or forty years without any serious breakdown. This may be the case, of course, with old, slow-running, low-pressure engines, but with the much higher piston speeds now adopted and the higher pressures of working, the man who anticipates the effective life of his plant to be anything of this order is exceedingly sanguine. It is customary with a privately-owned electrical installation to allow 5 per cent. interest, and 7½ per cent. or 10 per cent. depreciation on the plant, and the author considers that in order to bring out the costs on the same basis, the same allowance ought to be made in the case of mechanically driven plant.

A point of great importance in arriving at the power costs is the consideration of what may be termed the working load factor—that is to say, the ratio of the average power demanded to the maximum. It is customary among some mill managers to assume that the stated indicated H.P. of the engine represents their actual average load, and to calculate their estimated I.H.P. hours from this figure. As a matter of fact, owing to a variety of causes, including the percentage of machinery standing idle at a given time, the class of work being put through, and the state of the weather, the actual load both in a spinning mill and in a weaving shed varies considerably from time to time, and the working load factor is generally something considerably below 100 per cent. There are very few figures available for determining this quantity, but the author has been able, through the courtesy of the British Thomson-Houston Company, to publish some diagrams which show the variations in load in a linen thread spinning mill, both on the engine as shown by the indicator diagrams, and also on the generator as shown by a recording wattmeter, and from these it is apparent that

the average load factor in this case is something of the order of about 90 per cent. In this case, of course, the mill, being electrically driven, the transmission losses are small; but, as pointed out by Mr. Woodhouse in a paper about a month ago, the greater the steady transmission losses the higher the apparent working load factor. The conditions in a linen thread spinning mill are not quite the same as in the case of a cotton mill, but the author would anticipate that the working load factor would be no better in the case of a cotton mill, and when more



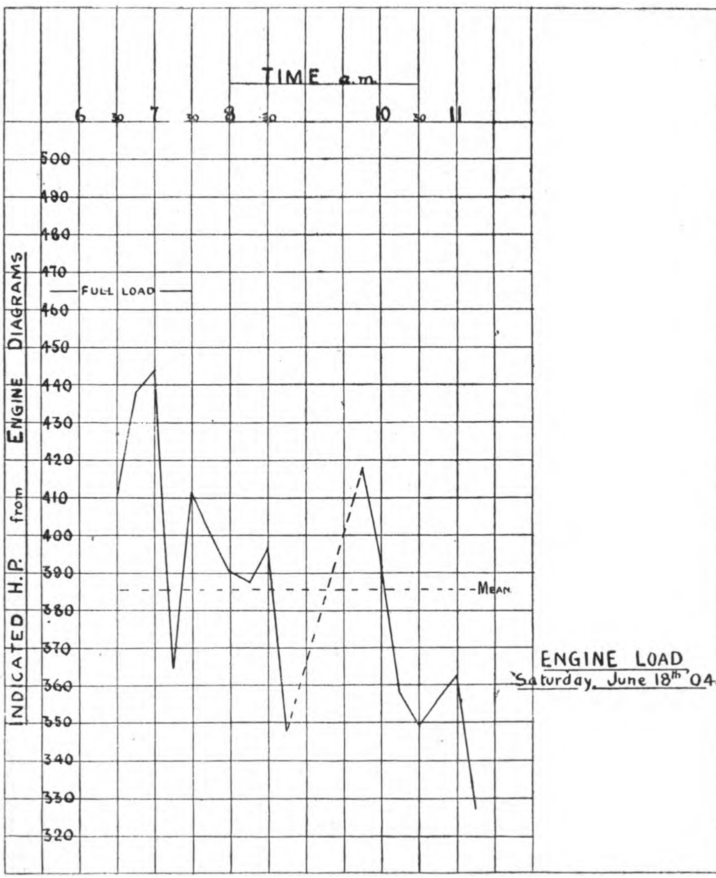
accurate data is available, this fact would have to be taken into account in calculating the cost per unit. It is improbable that the load factor represents anything better than 90 per cent., and 85 per cent. is probably more usual.

A considerable number of calculations which have been gone into at various times show that a properly arranged mill generating plant of fair size in this country should be able to produce energy, after allowing 5 per cent. interest and 10 per cent. depreciation in the capital cost of the plant, at about 0·3d. to 0·35d. per unit.

Centralisation of Power.

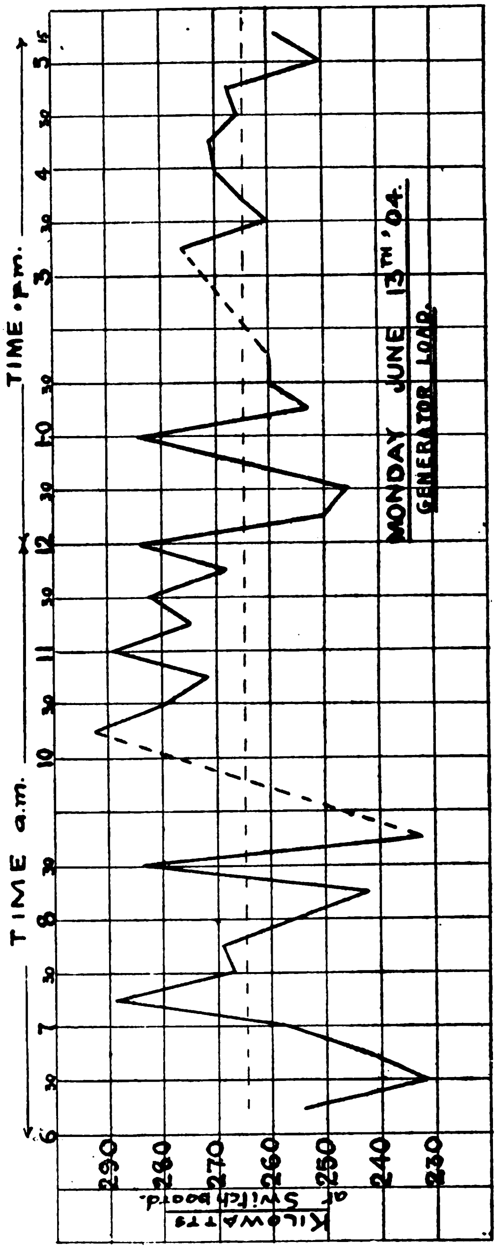
A matter upon which the author considers there should be room for profitable discussion is that of the advisability or otherwise of mills

taking power from a large power station. It is a subject which is of very considerable importance and worthy of the most careful consideration. In America the tendency, where mills are fairly closely grouped, is certainly towards the supply being given from one large station, which, from the condition of things, can be operated at a lower total cost per unit than can several isolated plants. This, of course, is

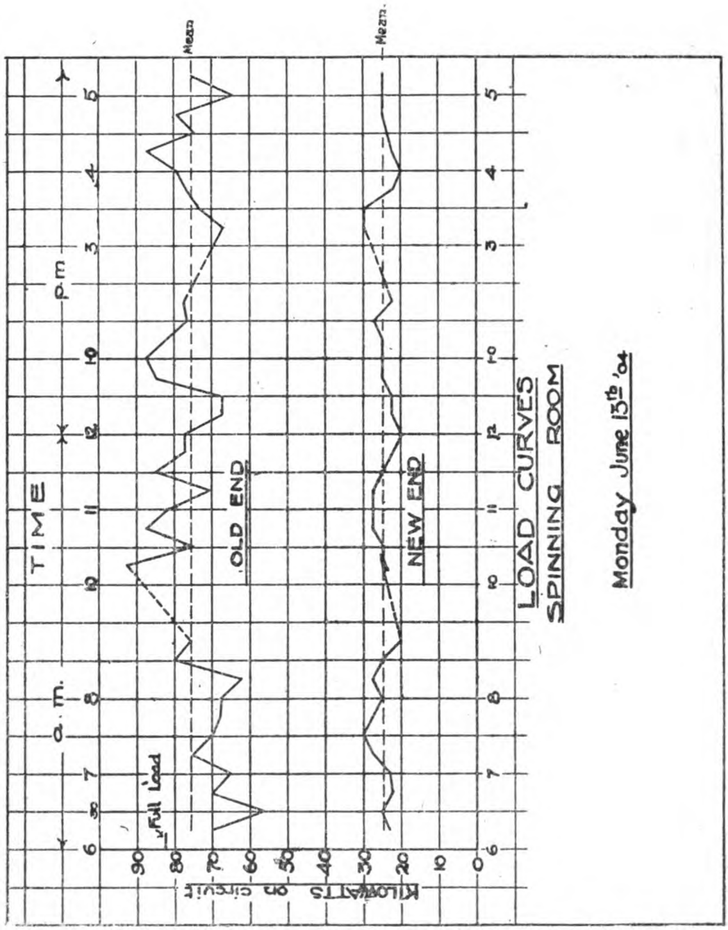


largely due to the reduced capital charges per kilowatt installed with a very large plant.

It is obvious from the figures given before that a cotton mill in this country is producing its own power very cheaply, and the question which becomes of importance is, can a large power station, allowing for the capital charges on its mains, supply energy at sufficiently low rates to make it worth the while of a mill company to take power from it? This, of course, is a subject which is outside the limits of the

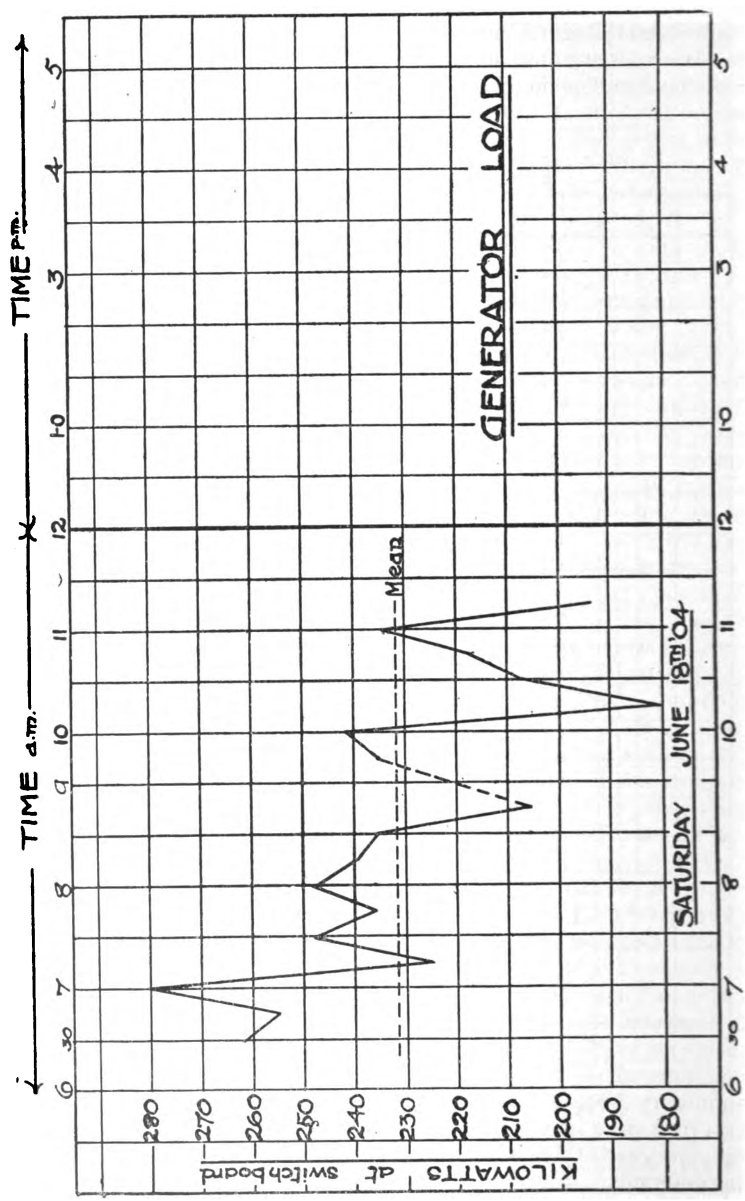

TIME a.m.
TIME p.m.
MONDAY JUNE 13TH '04.
GENERATOR LOAD.
Kilowatts at Switchboard.
290
280
270
260
250
240
230
6
7
8
10
11
12
1.0
3
4
5

present paper, but it is one upon which a great deal might be said, and the author trusts that some of those present may feel inclined to discuss the subject. It certainly would appear as if, in a district, say, like Bolton and the surrounding townships, or Oldham, it should be possible for a very large generating station to be able to work more cheaply than individual mill plants can, and in consequence to be able to supply the



mills with energy. As, of course, the mills are only running during the day-time the load factor on the station from the mills alone would not be very high ; but if, by the offer of special terms, other manufacturers could be induced to take power when the mills were stopped, such a station should be able to produce at extremely low rates per unit. It might be mentioned in this connection that arrangements have been made with a number of the jute factories in Dundee whereby they

undertake to take energy from a power company, whose station it is proposed to build, and which station, it is anticipated, will eventually



supply the whole of the power for all the jute factories throughout the district.

The advantages of the centralisation of power, such as suggested, are, of course, obvious, particularly in the reduction of the capital cost per spindle of the mill, which would enable the manufacturer to make the mill itself larger; the reduction of the chance of stoppage through breakdown to an absolute minimum; the doing away with the need of the manager looking after the power plant, and freeing him from anxiety about fluctuations in the price of fuel, strikes, etc. If the mill were able to purchase current at a price as low as it could generate it, the question would have the very serious consideration of all persons building new mills or installing new plant.

If it be granted that the electrical driving of a mill has the advantages which its advocates claim, there is a good deal in favour of taking power from a large power company, rather than generating it by an independent plant, provided that the purchase price per unit is not much higher than the generation costs of a private plant. Assuming that in a mill of 100,000 mixed mule and ring spindles the total cost per spindle, including the power plant and motors, works out at 28s. per spindle, the total cost of the mill would therefore be about £140,000. If, by eliminating the main generating plant, an amount of £10,000 could be saved, which would be something like correct, the cost would be reduced to 26s. per spindle. The £10,000 saved on the main generating plant would enable the company to put in a little over 7,500 extra spindles, representing an increase of 7½ per cent. in the production capacity of the mill on the same capital. Assuming, again, that the standing charges of the mill represent 12 per cent. of the production costs, the increase in the number of spindles would reduce the cost per lb. by 0·9 per cent., as the fixed charges would remain practically unaltered. The result would be, therefore, that the mill would have, on the same nominal capital, an increase of 7½ per cent. in production, and about 0·9 per cent. reduction in its manufacturing costs. This, it is obvious, would mean a very considerable increase of the profits of the mill, and would permit of the payment of a higher price for current purchased than at what it would have been possible to generate it by an independent plant.

Before closing it will be advisable to make a few remarks concerning the ninth item on the list of advantages claimed by advocates of electrical driving, namely, the possibility of checking the production of each department of the mill from day to day.

In the United States, Mr. Sydney B. Pain, of the General Electric Company, has made a number of experiments in various mills in order to ascertain how much the power consumption of the different departments varied during the course of the day. The recording wattmeter diagrams which he obtained exhibited some very interesting results, and he has been able to point out to various mill owners in some cases that they were short of machines of one class and had too many of a different class, these facts being deduced from the results shown on the diagrams. It was also found that, even in the best managed mills, there was a great tendency for energy demand to fall off as meal hours approached. It is obvious that if all the machines in a room are running at their full capacity up to the time of stopping, the power

demand should remain practically constant, and if the load diagram indicates that the demand commences to fall off somewhere about half an hour before the time of stopping, it is apparent that some of the machines must be running below their full capacity. The mill managers to whom these results were pointed out were at first quite certain that there must be some mistake in the wattmeter records, as such results as these were not possible in mills under their control. Further observation, however, and alterations on the lines suggested by the diagrams have shown that the records were not only correct, but that causes for them did exist. Alterations in the respective proportions of the manufacturing machinery and better discipline in the mill have, in many cases, almost done away with the variations in demand which were at first indicated, and the mill production has accordingly gone up.

LEEDS LOCAL SECTION.

TRAMWAYS PERMANENT WAY CONSTRUCTION AND MAINTENANCE.

By JAMES LORD, Associate Member.

(*Abstract of paper read February* 16, 1905.)

I have had an experience of twenty-one years in the construction and repairing of municipal tramways for horse, steam and electric cars at Bolton, Accrington, Blackburn, and Halifax. Since electric traction came to the front it has caused many alterations in the construction of the permanent way, owing to the cars being heavier and travelling at a greater speed. In the days of the horse cars the track was not of so great moment as at present. The concrete was generally laid first, the rails then laid with fish-plates only at the joints and packed afterwards with a mixture of sand and cement, and these have been known to stand without any repairs for many years. Steam traction was next introduced, and this was much more destructive to the permanent way than horse cars, but not, in my opinion, to such a serious extent as the present electric cars, owing to the above reasons, and the weight being distributed over a long area of track, as, in most cases, there were two bogies to each car and one to the engine.

During the time I was working as assistant under other engineers, four in number, each had a particular fancy for the lines being laid by one of the following methods, viz. :—

1. That the concrete be laid first, the metals afterwards, and packed up.
2. That the concrete be laid first, the metals to be put on whilst the same was wet. (This I consider a dirty and very unsatisfactory way.)
3. That the rails be packed up on blocks, crowed, and the concrete be put in afterwards.

In Halifax tramways were first laid in the year 1897 for the purpose of running electric cars, and, up to the commencement of the year 1900 (the time I was appointed engineer), there had been some twelve miles laid. On these the concrete was laid first, then the metals afterwards (without being anchored down) packed up with chippings and cement.

Since the year 1900 I have used girder rails as per section Figure 1,

weighing 96 lbs. per yard, with fish-plates 25 ins. long weighing 70 lbs. per pair, and anchored down at the joints with Howard Cooper's patent joint, 24 ins. long, with six rivets into the bottom flange of each rail. This joint block is simply a piece of rail turned the opposite way and the two flanges riveted together. I have also used an intermediate anchor (either one similar to those at the joints, or what is known as the Ames Crosta Bulldog Anchor), Figure 4. As to price of these anchors, there is very little difference. I am strongly of opinion that it is better to lay the metals first and the concrete afterwards, and this practice I always carry out. I place the metals on wooden blocks packed up with wedges to the exact level, at intervals of about 9 ft. apart, then the line is crowed, afterwards the concrete is packed under the metals and the remainder of the track to the level of the bottom flange of the rail; after this has stood for two days the wooden blocks are taken out and the holes filled in with concrete, and the following day the whole of the crevices under the metals are run in with a mixture of sand and cement (1 to 1). I have had several of the lines tested, but never found one rail in the least hollow.

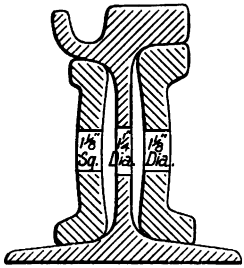


FIG. 1.

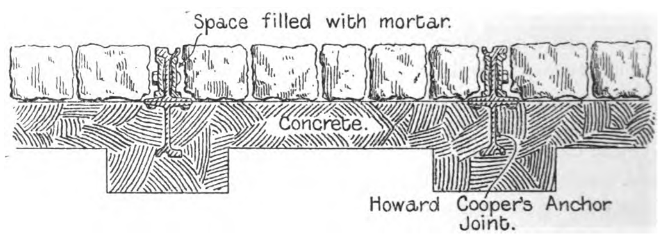


FIG. 2.

Since the year 1900 I have laid about 36 miles of track, and have used rails 60 ft., 45 ft., and a few 35 ft. long, but much prefer the 45 ft. lengths, for the following reasons, viz.: They are much more readily

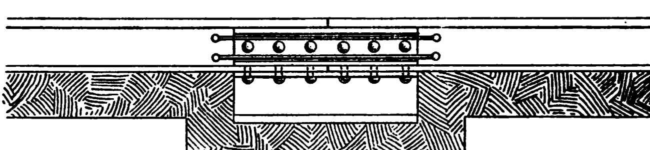


FIG. 3.

handled, especially in narrow and steep roads such as exist in Halifax; secondly, there is not the same danger that occurs with the longer rail, the latter very often acquiring a permanent set or hogback by sagging with its own weight, therefore the 45 ft. rail is better to pave to and it is

much easier to curve. It is true that in the 60 ft. rails we have less joints to contend with, which also means less electrical resistance, but to counteract the cost of the joints the 45 ft. rail only requires one intermediate anchor, whereas the 60 ft. rail ought to have two to each rail or we find the rail will not sit perfectly solid. The rail used in Halifax is 7 ins. deep, with a bottom flange 7 ins. wide and with a tread 2 ins. wide, groove $1\frac{1}{8}$ ins. wide and 1 in. deep.

Formerly the whole of the tracks in Halifax were paved with local stone setts 6 ins. deep, but now I use nothing but granite or wood, except on the very steep gradients (those steeper than 1 in 10), and even then I use granite for the 18 ins. margins. The whole of the pavement is run in or grouted with pitch on the level roads to the top of the setts, but in gradients the top $\frac{1}{4}$ in. is grouted in cement. If this is not done the pitch runs in hot weather, and is liable to run down the tread of the rail, which is dangerous for traffic. I laid in 1900 about half a mile of track on longitudinal sleepers 10 ins. by 4 ins. creosoted red wood; it is less rigid and therefore better for the rolling stock. These sleepers were embedded in concrete and the metals screwed down with coach screws at intervals of 4 ft. 6 ins., but my difficulty has been to keep the setts level with the rails,

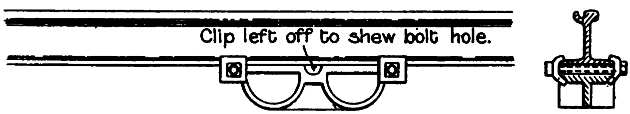


FIG. 4.

owing to the slight vibration that takes place. If it were not necessary to keep the surface of the road in good condition for vehicular traffic, the writer would much prefer the lines to be laid on timber.

The whole of our lines which were laid and were packed up from the concrete have gone badly at the joints, and the setts are continually lifting and dropping, owing to the bumping of the cars on the joints, and these lines have constantly to be re-packed and re-paved. In fact one line, about one mile in length, which was filled in with tar macadam, I had taken up in September of last year and found some of the joints down quite $1\frac{1}{2}$ ins. In re-laying this track I welded the whole of the joints on the Thermit process (cutting off the bad ends of the rail and putting in short pieces) and anchored the rails at the joints and centres and paved the surface with granite. This work has proved very satisfactory up to the present. Of course I may say it is not so perfect as would have been the case had new rails been used, owing, as above stated, to the joints being done and the rails hog-backed, but the road is not a very busy one and will last for years to come.

The life of local setts on busy thoroughfares was certainly not more than three or four years, those next to the rail wearing down quite three inches in that time. I have had little or no repairs as yet on tracks that have been anchored down. It is advisable to use anchors oftener on curves than on the straight, as the rail does not sit

so well. The wear and tear on the metals in Halifax is very great owing to the steep gradients, which necessitate the constant use of the slipper brake.

I think it is essential that every engineer having under his control the construction and maintenance of permanent way should have possession of an emery rail grinder. The one in use at Halifax is, I think, up to the present unique, it being the only one of its kind so complete. It was built by Messrs. Ames Crosta & Co., Sanitary Engineers, Nottingham, specially for Halifax, and is fitted up with an electric motor so that it can travel and work from the overhead wire, It is also fitted with a twelve horse-power petrol motor and can travel and grind from this, if the overhead equipment is cut off, or in case of construction where the same has not been completed. The machine is fitted up with a complete set of drilling apparatus for drilling longitudinally and vertically, or, if required, at any angle, which is very useful and can be run from either motor.

One of the points, apart from the purely mechanical wear and tear of the tramways, which requires the attention of the Permanent Way Constructor, is that of the bonding of the rails, or other means adopted for reducing the electrical resistance of the rails as a return for the current, and it is self-evident that the nearer we can get to a continuous rail the better will be the conductivity and the less danger of electrolysis due to stray current to gas, water, and other metal pipes. It is clear that the welded joint approaches the nearest to the ideal solid way return.

I have had some tests made of the electrical resistance of Thermit welding and otherwise, with the following results :—

		Ohms.
1.	Taking a solid rail 8 ft. long as a standard resistance ...	·000067
2.	With Thermit welded joint, without bonds, same length ...	·000072
3.	do. with one do. ...	·000067
4.	Fish-plates, sole-plate and two bonds, newly constructed	·000078
5.	do. after two years...	·000095

The lay-out of the curves is an important item in the maintenance account, as will be gathered from the following instance: I had a curve with a radius of 30 ft., the wheel base of the car was 5 ft. 6 ins.; this curve never lasted a longer period than six months, but some two and a half years ago this was altered to a 45 ft. radius and it is little worse to-day. On many of the curves which are 35 ft. radius I have to put on a new check rail at intervals of about nine months.

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EXPLANATION OF ABBREVIATIONS.

[P] signifies a reference to the general title or subject of a Paper.

[*p*] signifies a reference to a subject incidentally introduced into a Paper.

[D] signifies a reference to remarks made in a Discussion upon a Paper, of which the general title or subject is quoted.

[*d*] signifies a reference to remarks incidentally introduced into a discussion on a Paper.

[*Ref.*] signifies a reference to the place of publication in the Technical Press of a Paper read at a Local Section, and not yet printed in this Journal.

Note.—The lists of speakers in the Discussion upon any Paper will be found in the Table of Contents at the beginning of the volume.

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